Six papers / by Lord Lister ; with a short biography and explanatory notes by Sir Rickman J. Godlee.

CLASSICS OF MEDICINE.

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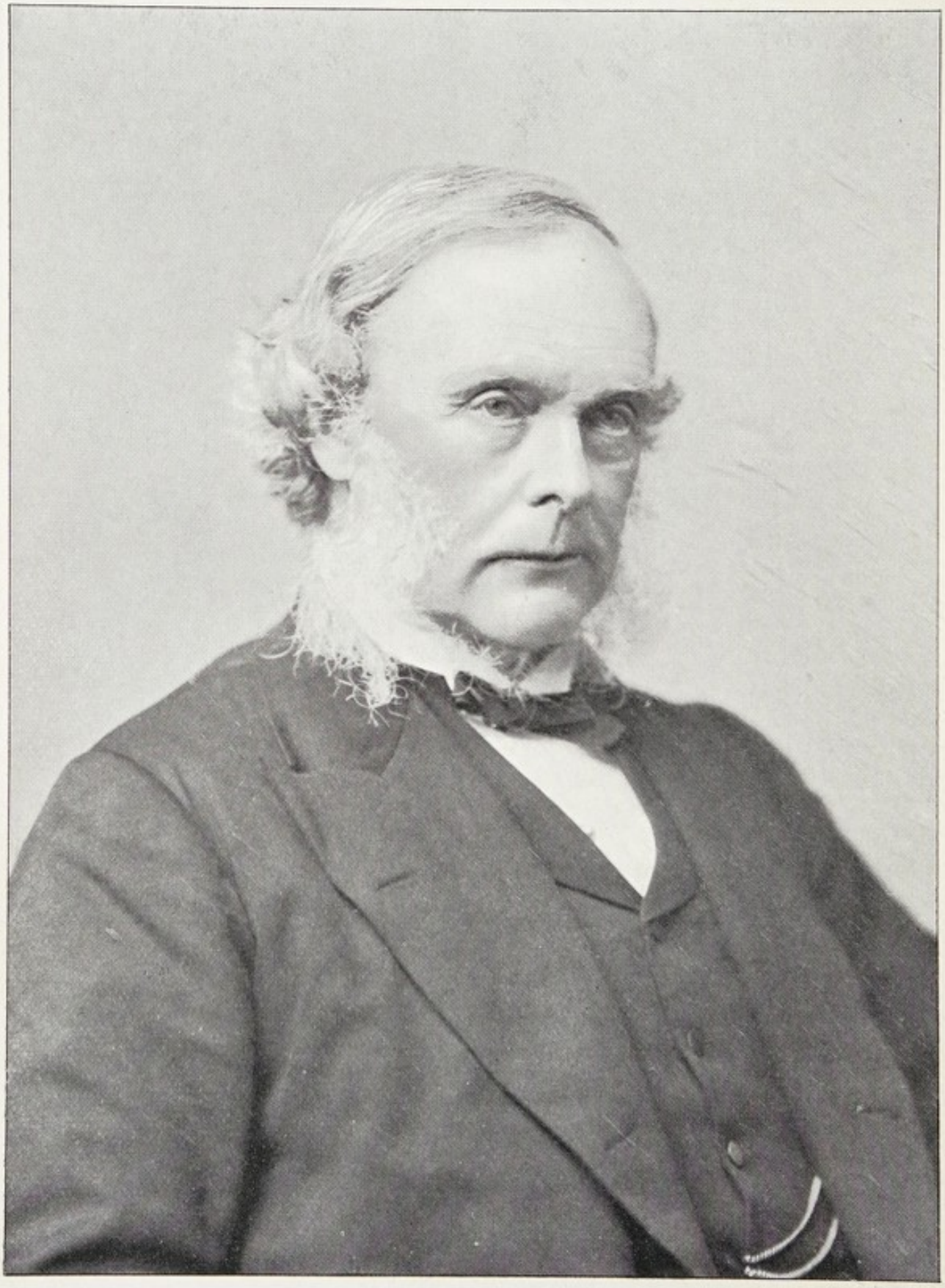
PLATE I.



LORD LISTER.

(From a photograph.)

BALE'S MEDICAL BOOKS AND JOURNALS

SIX PAPERS

BY

LORD LISTER

WITH A SHORT BIOGRAPHY

AND

EXPLANATORY NOTES

BY

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PREFACE.

HERE are six of Lister's published papers, or rather four complete papers and parts of two others. One cannot give a really representative sample in so small a number, for there are more than sixty of them and they deal with histology, physiology, pathology, technical surgery, the antiseptic system, and bacteriology. Some of the very best are so long that for this reason alone they had to be excluded. It involves too heavy a responsibility to abridge the writings of a deceased author, and it is a doubtful compliment to present only parts of them as I have sometimes reluctantly been obliged to do.

In the stately *Collected Papers*, published by the Clarendon Press, the papers are arranged according to subjects. Here they are placed in chronological order, which I think is the better plan if one is aiming at tracing the development of the author's mind and forming a picture of him at the time when each was written.

In order to help the reader to do so, a short epitome of Lister's life is given and a few words of introduction are placed before each paper; but there are many regrettable gaps. Thus no room could be made for an example of his earliest work on the contractile tissue of the iris and the muscular tissue of the skin, nor for that which followed on involuntary muscular fibre; the coagulation of the blood is hardly referred to; and his important additions to technical surgery which might have been

illustrated by one of his articles on amputation, excision of the wrist, or the treatment of fractured patella are omitted altogether. A typical selection indeed would have included still more. There should have been some to illustrate the latest workings of his mind, such as the "Huxley Lecture," one of his Presidential addresses to the Royal Society, or even a speech in the House of Lords.

But as all this was obviously impossible, a less ambitious scheme was adopted. It is confessedly imperfect, but it has the definite object of reminding the present generation that Lister's writings are not mere dull records of past history, but full of living interest, and that they are, or should be, available for consultation in every well-appointed public medical library.

R. J. G.

October, 1920.

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ERRATUM.

Page 34, line 5, for fig. 3 of Plate *read* Plate 4, p. 65, fig. 1.

LISTER:

HIS PLACE IN HISTORY.

> "When I was a little boy I used to imagine that prejudice was a thing peculiar to some individuals. But, alas! I have since learned that we are all under its influence, and that it is only a question of degree. But let us ever contend against it; and remembering that the glorious truth is always present, let us strive patiently and humbly to discover it."
>
> GRADUATION ADDRESS, 1876.

OTHERS before Lister have been called by their contemporaries, or more often by their successors, the founders or fathers of modern surgery. It has been the same with medicine and other sciences. Yet the modern has soon become antique and succumbed before a new revolution. It seems to us to-day, however, as if all the surgery of all the future must be founded upon, or modified by, the new principle which Lister discovered and spent half his life in forcing upon such unwilling listeners that he cried out almost in despair:—

> Can such things be
> And overcome us like a summer cloud
> Without our special wonder?

And yet it is common enough to hear modern surgeons and pathologists blaming almost angrily his misconceptions and mistakes, as if they could possibly have been

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avoided in the absence of the knowledge that grew out of them, and as if the road he pointed out really led astray because it did not follow an absolutely direct course to the goal. Such complaints and the habit of dwelling upon the imperfections of Lister's earliest dressings may easily make one forget that he did, with this primitive weapon, make the first breach in the wall by which the surgeons of the first half of the nineteenth century were so straitly confined. And it is important for us to remember that, because of these restrictions, they belonged almost to a different race from ours. They thought other thoughts and spoke another language. They would have been more at home with John Hunter or even with Cheselden than with the surgeons of to-day, most of whose conversation would have been to them mere gibberish, and most of whose performances would have seemed to be little less than murder.

John Hunter was, of course, much more than a mere surgeon. He thought and acted like a physiologist and a pathologist and his surgery was far more scientific than that of his predecessors. But he was before his time; and up to the middle of last century, indeed until 1865, it is fair to say that British surgeons were chiefly occupied with improving the technical side of their art, and that their ingenuity was directed to such details as the shape of flaps, or the devising of new splints. In this direction they thought they had approached perfection, but from time to time the cry went up, that, in spite of it all, their efforts were constantly thwarted by the rough and inscrutable blind hand of Chance.

This was the state of things when Lister, who was born in 1827, was a student at University College, London, from 1844 to 1853. Brought up in one of the stricter, though not the strictest, Quaker circles, and educated at Quaker schools, he started his medical career with some unusual advantages. First, he did not rebel

against the restrictions which Quakerism involved, but accepted without reserve its serious view of individual responsibility, and its injunctions about perfect honesty and absolute truthfulness. In saying this, it is not intended to claim for Quakerism more than it deserves. Not all who belong to the sect, any more than the members of other denominations, act up to their principles. But it must be granted that any one who does so is well fortified against the pitfalls and temptations that beset the scientific explorer, the teacher of youth and the practitioner of medicine. Secondly, he had the profoundest respect for his father, who, though engaged in business, was a Fellow of the Royal Society and a man of high scientific attainments; and whose special work on the achromatic microscope directed his son's attention from the very first towards the most minute forms of animal and vegetable life and the comparatively new subjects of histology and microscopic physiology. Thirdly, and this was perhaps hereditary, he had exceptionally good abilities, a retentive memory, the power of concentration, and unlimited patience and enthusiasm in investigating every doubtful question and pursuing each investigation to the bitter end. Lastly, he was spared the anxiety of having to earn his daily bread.

As a very junior student he saw the first operation performed under a general anaesthetic in London. This was by Robert Liston in 1846, and there were later opportunities of watching his marvellous dexterity and coolness, then considered to be the most essential qualifications for a surgeon. In the hospital he constantly saw the ravages of erysipelas, pyaemia and hospital gangrene, which between them carried off a large proportion of the cases of compound fracture and amputation, and the fear of which kept his teachers from using the knife except in dire necessity. And he was taught that

these catastrophies must be accepted, not indeed without sadness, but as the unavoidable accidents of surgery. Some early notes and drawings of hospital gangrene and the structure and varieties of pus cells show that he was already thinking gravely about these "accidents," but he chiefly directed his attention at this time to three histological investigations: on the contractile tissue of the iris, the muscular tissue of the skin and the structure of involuntary muscular fibre. On each of these he wrote a paper before his studentship was completed. All three contain important new observations. He was also engaged in physiological experiments, the results of which were published later. It is to be regretted that there is no space in this volume for any of these early papers.

The next period of Lister's life was spent in Edinburgh, where he found a larger and in some ways a better school of medicine than at University College. He had planned a short visit to Mr. Syme, who was perhaps the most thoughtful and skilful surgeon of his day; but he was so much attracted by Syme's personality and the advantages of Edinburgh that he stayed on for nearly seven years, first as House Surgeon, and later as extra-mural lecturer on surgery and Assistant Surgeon to the Royal Infirmary.

In 1856 he was married to Syme's eldest daughter, Agnes. This had a great influence on Lister's career. Amongst other things it led to his leaving the "Society of Friends" and joining the Episcopalian Church. He was thus emancipated from the trammels of Quakerism, some of which, such as the peculiar costume, made its members rather painfully conspicuous; and he became henceforth outwardly indistinguishable from his fellow-men. The marriage was a singularly happy one. They had no children, and for thirty-seven years his wife

PLATE II.

JOSEPH AND AGNES LISTER.

(From a photograph.)

devoted herself heart and soul to her husband, helping him untiringly in his scientific work and sharing to the full in the pursuit of natural history which was their diversion on many holidays at home and abroad. During their long wedding tour they visited most of the important medical schools on the Continent, and Lister made the acquaintance of the leading continental surgeons.

In Edinburgh he was busy, like other surgeons elsewhere, in trying to improve surgical technique; but when he had to face the problem of imparting knowledge to others, he was almost overcome by the responsibility he had undertaken, and staggered at the universal ignorance about the first and most fundamental subject he would have to teach, namely inflammation. He was therefore led to carry out a long series of closely linked investigations, the results of which were published in many papers dealing with coagulation of the blood, the pigmentary system of the frog, the function of the nerves, the contraction of arteries, and the early stages of inflammation. A part of one of these (No. 1, p. 31) is included in the present selection. Thus when he left Edinburgh to enter upon the next period of his life as Regius Professor of Surgery in Glasgow he was not only an accomplished surgeon of the conservative school, but had already gained a worldwide reputation by his scientific work.

Nine of his most active years were spent in Glasgow. Here he had the new experience, and the increased stimulus and responsibility of addressing a very large class of students—a great contrast to the faithful few who followed him in Edinburgh. He also had charge of many more beds in a much more unhealthy hospital. Even under Syme's careful management the dangers of sepsis were so great that he told Lister he thought "that it would be on the whole better if all compound

fractures of the leg were subjected to amputation, without any attempt to save the limb"; but in Glasgow it was ten times worse. Lister could not fail to be impressed by the horror of it, and more than ever convinced that unless the "unhealthy action" of wounds, which he had long taught depended on decomposition, could be prevented, surgery must always remain a restricted and unsatisfactory business. For five years he continued his experiments and observations on inflammation and suppuration without seeing any light, because, though it seemed self-evident that decomposition was somehow brought about by the air, it was equally clear that none of the gases of the air could possibly be responsible for the mischief.

In the meantime he produced his notable articles on Anaesthetics, Amputation, and Excision of the Wrist. It is hardly possible to give a selection of Lister's writing without including one of these; but space is limited and it is hoped that the portion of one of them which is given here (No. 2, p. 71) will induce the reader to study the rest in the larger but easily accessible *Collected Papers*.

We may call 1865 the birth year of Antiseptic Surgery, though no public announcement of the event was made till 1867. Eighteen years had passed since Lister had begun his medical studies. The benefits to be derived from anaesthetics had been fully realized; Florence Nightingale had brought about a radical change in the system of nursing; sanitation, though far from perfect, had been improved; and yet no great advance had been made in the practice of surgery. Such as it was, it had almost been confined to the improvement of methods of operating, for example in the resection of joints and the performance of lithotrity, and in the introduction into common use of the ophthalmoscope, the laryngoscope, the galvanic cautery, and the aspirator. The bar to

progress was still the fear of hospital diseases, which, indeed, were as rampant as ever, in spite of the increasing use of various new antiseptic substances as washes to suppurating wounds.

The subject certainly attracted more and more notice both from the profession and from the public. Attention was drawn to the comparative safety of private practice. The dangers of a hospital were said to be greater than those of a battlefield. The word "Hospitalism" was invented by Sir James Simpson, and the wholesale destruction of existing hospitals was seriously contemplated in order to replace them by villages of temporary hutments.

All the vague surmisings about infected hospital buildings, tainted air, morbific contagious materials, miasmata and other hypothetical causes of hospital diseases came to an end when the true cause of decomposition was demonstrated and accepted as proved. The demonstration came from Louis Pasteur, somewhere about 1860, but it was many a long year before universal recognition was accorded to his discoveries.

It is difficult, in this short biography of Lister, which is intended rather to fix his place in history than to give a detailed picture of his life, to decide how much shall be said about Pasteur, to whose influence on his own discovery Lister always paid the highest tribute of gratitude. Everyone should read Vallery-Radot's admirable life of his father-in-law.[1] Here we must be content with saying that he was five years older than Lister and that he was perhaps the greatest of French chemists; that he had extended his investigations far beyond the limits of chemistry, chiefly in the study of fermentation, and in inquiries as to the occurrence or non-occurrence

[1] *The Life of Pasteur,* by René Vallery-Radot. Translated by Mrs. R. L. Devonshire. 1902. Popular edition, 1906. Latest edition, with preface by Sir William Osler. Constable and Co. 1919. Price, 10s. 6d.

of spontaneous generation. In this field he completed and extended the work of a long series of distinguished chemists and biologists, some of whom had made shrewd guesses as to the nature of fermentation and had almost set at rest the conflict over spontaneous generation. Pasteur's brilliant experiments and sound reasoning definitely settled the question of fermentation. As far as spontaneous generation is concerned he showed that under ordinary circumstances it never does take place, but it is, of course, impossible for anyone to prove that life never can, under any conceivable circumstances, start *de novo* in inorganic substances.

All the world recognized the value of Pasteur's studies in crystallography. Our own Royal Society had awarded him its Rumford medal as early as 1856. But his work on fermentation does not seem to have attracted so much notice in this country. Surely, however, it must have been known to some practical surgeons in Great Britain before Lister's attention was, so to say, casually drawn to it by the Professor of Chemistry in Glasgow in 1864. Perhaps not; for surgeons were not great readers of foreign literature. Whether they had heard of them or not, however, the possibility of applying Pasteur's discoveries to the prevention of hospital diseases appears to have struck no one, with the possible exception of Spencer Wells. This may have been because Pasteur was a chemist and it did not occur to doctors that his work could have any direct relation to medicine. It may have been because no one had yet really reached Lister's standpoint of recognizing that suppuration in wounds depended on decomposition and that the nature and cause of decomposition were still unexplained and unaccounted for.

The particular parts of Pasteur's discoveries, which shone for Lister like the first gleams of sunrise, were that decomposition was a form of fermentation and

was subject to the same laws; that decomposition, like other kinds of fermentation, was caused by vibrios, i.e., micro-organisms, which were conveyed from place to place by the air; that these vibrios could be destroyed by heat and chemical substances or entrapped by mechanical filtration; and that spontaneous generation was a chimera. These facts seemed to explain quite clearly and adequately why the admission of air to a wound made its contents decompose, and accordingly he set to work to try to render the air innocuous by destroying or circumventing its germs. He saw at once, or before very long, that, if he were successful, a complete revolution in surgery must inevitably follow.

His first attempt to apply Pasteur's discoveries to practical surgery—to inaugurate, in fact, aseptic surgery—is described in his paper No. 3, p. 93.

There are many critics of Lister's primitive dressing. Let them try to put themselves in his place. He had decided to begin with the difficult and complicated problem of compound fracture. He had thus always in mind the need of providing in his dressing a protection like that of the skin, because it is the injury to the skin which makes all the difference between compound and simple fractures. He was convinced that the air was teeming with the special vibrios that caused decomposition, which somewhat distracted his attention from their other habitats. Moreover he had only crude carbolic acid to work with—an intractable, irritating, oily fluid, almost insoluble in water. The modern critic should ponder over these things, and bear in mind how little was known of bacteriology and that nothing at all was known about the defence of the living tissues, and then should read again what Lister wrote in 1867, and ask himself whether he could have invented a better plan.

The paper, at all events, made a deep impression upon the surgical world—though by most, alas, it was looked

upon as merely advocating a new *method of treatment*—for the more thoughtful realized that it introduced a new *principle*, which, if it were true, ought to do away with all existing methods of treatment, applicable not only to compound fractures, but also to all wounds made by the surgeon through unbroken skin. This was appreciated and fairly commented on by the medical press.

We have now reached a chapter in the history of aseptic surgery which is at first a puzzling one. It deals with the reception of Lister's new doctrine by his colleagues in this country. It is like a chapter of ancient history, but it would never need to be written about the reception of any novelty, however grotesque, at the present day. Between 1867 and 1870 he wrote seven papers in which he told of improvements in method which largely did away with the objections to the primitive dressing; he explained that the antiseptic principle had nothing specially to do with carbolic acid; he showed how it could be applied to the treatment of abscesses and wounds made by the surgeon; and, more than all, he told of operations successfully carried out by him which other surgeons would never have thought of undertaking. Visitors to Glasgow saw that this was no exaggeration, and they could observe for themselves that his previously unhealthy wards, though now overcrowded and less often cleaned than those of his colleagues, were free from hospital diseases, and in fact models of healthiness. And yet, after the first warm appreciation, British surgeons soon became apathetic and then hotly antagonistic. Lister might say what he liked, they simply did not believe him.

Various explanations of their attitude may be given and there are some excuses for it. The surgeons of that day were not sufficiently well educated to assimilate so novel and so revolutionary an idea. Germs were so

small and so elusive, and it was so tempting to ridicule the germ theory. But the chief discouragement was that the new practice was so difficult to carry out even by the few who accepted the germ theory. It was in fact easy enough to fail, as the reader can see, that Lister himself failed (p. 96); and, after a failure or two, the natural thing was to pooh-pooh the whole affair and conclude that it was only Lister's personal attention that enabled him to get such results—if indeed he really did obtain them. I should not be surprised if, even to-day, although his statements are corroborated by many still living witnesses, some are sceptical about these results simply because he used potent chemical antiseptics and because his methods were not applicable to the treatment of the wounded in the late war.

The reception of his doctrine abroad, being uninfluenced by personal considerations, was more cordial. In the Central Empires and in Scandinavian countries, where the state of the hospitals was far worse than here, and where the chief surgeons were, by education, better fitted than ours to receive and apply an abstruse scientific idea, the antiseptic treatment was carried out with striking success. Amongst these first-rate surgeons Lister made many converts and some personal friendships; but the rank and file were perhaps not much more successful than British surgeons. In France and Italy (with one notable exception) little progress was made for many years; and America, being a very busy and a very practical nation, waited for the verdict of Europe before embarking on what appeared to be of the nature of an experiment.

These doubtings and discords continued long after Lister had entered upon the next and the most brilliant period of his career—his occupancy of the chair of Clinical Surgery in Edinburgh, which lasted for eight years (1869-1877).

It began with many changes and uprootings. Just before he left Glasgow his father died, and their weekly correspondence which contained much of scientific interest and even more of a touching and intimate nature came to an end. As his mother had died some years before, and as his surviving brothers and sisters had long since married, the old family home at Upton in Essex was broken up. Another great loss was the death of Syme in 1870, which left Lister indisputably the foremost surgeon in Scotland.

Edinburgh was an ideal place for elaborating and promulgating a new system of surgery. His class was the largest in the kingdom and the students were fired by an enthusiasm that Lister had hitherto always inspired. There were none of the distractions of a huge metropolis like London. Foreign and British visitors could give their whole time to his clinique. And there was little chance of acquiring one of those enormous and engrossing private practices which have blocked the way of research for so many of our most gifted doctors. As in Glasgow, time had, of course, to be spent on University affairs, but there was time enough to devote to the great business of his life and to bring it before the profession, as he did in more than a dozen papers and addresses. Of these only one, alas (No. 4, p. 135), can find a place in this volume; but some further record of what he did will be found in papers No. 5 (p. 161) and No. 6 (p. 185).

In these papers the reader will see for himself how the methods of applying the antiseptic principle had been modified, but we may briefly enumerate the most important changes. The strength of the antiseptic in the dressings and lotions had been greatly diminished out of respect for the delicate living tissues, which the earlier dressings had needlessly irritated. Non-absorbent had given place to absorbent dressings. The trustworthiness

of the catgut ligature had been established. The whole procedure of tying arteries in wounds and ligaturing them in their continuity had thus been changed, long silk or thread ligatures which had to come away by sloughing of the vessel being no longer used. The necessity of working in an antiseptic atmosphere produced by a spray had become an accepted article of faith—unfortunately, shall we say?—for it proved a stumbling-block to friends and a weapon in the hand of the enemy, till it was finally expunged from the canon in 1887.

The reason for the introduction of the spray is obvious—Lister's belief in the vast number of decomposition-producing vibrios in the air. Its abandonment was due to the gradually accumulated evidence, first, that they are by no means so plentiful; and secondly, that the natural defences of the body, if the tissues be not irritated, are much stronger than had been at first supposed.

To determine the nature and the amount of the defence which the living tissues can put up against the attacks of micro-organisms became a problem of fundamental importance. Lister's studies in bacteriology carried out at this time played an important part in its solution.

There was no time of his life when the intervals between the calls of practice—even "the raspings and parings of hours"—were not devoted to experimental research. We have seen how he worked at histology, the clotting of the blood, and inflammation, in the early Edinburgh days. During the first five years at Glasgow he was absorbed with observations on suppuration, and especially suppuration of blood-clot; but they were abruptly broken off when he heard of Pasteur's experiments on germs. Some of these he felt bound to repeat, and much time had also to be given to seeking for better antiseptic dressings and a perfect catgut ligature, which involved many tedious excursions into the mazes of technical chemistry.

After his return to Edinburgh the need for improved dressings was less urgent; but the differentiation between the various kinds of micro-organisms was daily acquiring greater significance. Bacteriology was emerging from its infancy and observers were beginning to classify these minute creatures according to their morphological and physiological characters, to find out where they lived and their influence upon the substances in which they grew. Obviously the problem of antisepsis was not so simple as it at first appeared to be, and Lister joined the ranks of the explorers, taking for the object of his study the lactic-acid fermentation, which Pasteur had already investigated and partly, though imperfectly, explained. The reader will be able to see from the fifth paper in this series (p. 161) by what devious paths he had to travel before he at last succeeded in isolating and cultivating the chief lactic ferment, which he christened *Bacterium lactis*, and how, in the course of the investigation, he devised a plan by which any organism could be obtained in "pure culture." It was complicated but it was efficacious, and it was the one commonly employed till it was superseded by Koch's simpler method of cultivation on solid media. Lister was well prepared for these bacteriological studies by his knowledge of botany, and a zest was added to them by the pioneering work of his brother Arthur[1] on fungi and myxomycetes in which he took great interest, and was, for a time, himself incidentally occupied.

Amongst the various media in which Lister cultivated micro-organisms were blood-clot and blood serum. He found that neither of them is a suitable nidus for these low forms of life, in fact that both have a repellant action upon them. This seemed at first almost incredible to one

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[1] "Mycetozoa." *Essex Field Club Special Memoirs*, vol. vi, by Gulielma Lister, p. 8. Simpkin, Marshall and Co. 1918.

who had watched wounds pass into a state of hopeless decomposition in the course of a few hours; but when later experiments showed that living clot with its immigrated phagocytes was far more antiseptic than dead clot, certain previously incomprehensible facts in connection with primary union became easy to understand. He was thus led to take up again the investigations into suppuration which were interrupted in 1865. He published no complete account of these researches, but they are often referred to in his later papers. Their bearing upon "aseptic" as opposed to "antiseptic" surgery is obvious.

This is perhaps the best place to say a few words on this thorny subject. It must first be remembered that *aseptic* surgery as it is now understood was, from almost the first, Lister's ideal, and that he tried to adopt it himself as early as 1868 and on other later occasions, but did not persist with the attempt because he thought that antiseptic surgery was equally efficient and far more easily carried out. The scientific father of the aseptic technique was von Bergmann, but he came much later. The explanation of its success was supplied by Metchnikoff and Wright and their schools; but this was later still. The latest demonstration of what it can accomplish was given in the last stages of the late war, when free excision of all damaged tissue, copious ablutions with soap and water and immediate suture of even large wounds were often followed by primary union.

In the early days two apparent incongruities kept the matter always—though very indefinitely—before the profession: first, the fact that a certain number of surgeons suffered so much from the carbolic acid spray that they gave it up, and yet, by adopting Lister's other precautions, obtained just as good results as before; secondly, the amazing successes of some ovariotomists, notably Lister's intimate friend "Tom" Keith, who would

willingly have adopted every antiseptic detail if they had not injured his health, and the truculent Lawson Tait who inherited Simpson's bitter animosity against Lister and swore by the virtues of Birmingham tap-water. The special case of abdominal surgery cannot be entered upon here. And, in this sketch of Lister's life no more must be said on the general subject, except to insist on the identity of the principle underlying the two methods. The object of both is to prevent the access of pathogenic micro-organisms to a wound in such a condition or in such numbers that they have a chance of living and propagating themselves. In the earliest days Lister thought that he had succeeded in keeping germs out altogether, but it was found later that complete exclusion was rare, that it was sometimes impossible, and that, after all, it was not really essential. Both schools use antiseptic agents, but "aseptic" surgeons trust more to heat and less to chemical substances than Lister did. In judging of Lister's relation to the accepted surgery of to-day—which may be only a transitory phase—we must conclude that it is a direct outcome of his labours. In speaking of his life it must be added that he was disappointed to see his simple method yielding place to others which were certainly more complicated, and, in his opinion less adapted for general use and less likely to give uniformly good results. He did not, however, say much about it, and only a few casual references to it are met with in some of his later papers.

In 1875 Lister made a tour on the Continent which developed into a sort of triumphal progress. He wanted especially to see what the German surgeons were doing, and he met with an extraordinarily enthusiastic reception. The following year he attended the meeting of the International Medical Congress in Philadelphia, where he received an equally warm, if less emotional, welcome, although his teaching had up to this time taken no deep

root in America. American surgeons were, however, now convinced by what they heard and saw, and set to work in earnest from this time forward.

These two expeditions greatly increased his personal acquaintance with foreign surgeons, and about the same time he became better known in London from being appointed Crown nominee for Scotland on the General Medical Council, and from the prominent part he took in opposing the "Cruelty to Animals Bill." Thus by the time he reached his fiftieth birthday he was recognized as a leader in the profession, even by those—still a goodly number—who thought his enthusiasm about antiseptics was nothing but a craze.

The stronghold of the opposition was London. When therefore he was offered the chance of returning to his native city as the successor of Sir William Fergusson in the chair of clinical surgery at King's College, a powerful argument in favour of accepting it was that it would give him the opportunity of carrying the war into the enemy's country.

In early days Lister naturally looked upon London as the place where he was destined to spend his life. When he first went to Edinburgh it was with no intention of settling there. Whilst he was at Glasgow he had been a candidate for a professorship at University College, London, and now, when he was actually invited to come, he felt that for many reasons he could not refuse. Yet there were great drawbacks. He knew that the hospital was small and that he would have fewer beds than in Edinburgh. He knew that he was going to enter an unfamiliar circle—if the term is applicable to such a heterogeneous body as the medical profession in London—many of whose members viewed him with suspicion or even dislike. But he could not foresee that he would no longer, as he had done in Scotland, attract a large

class, and that the London student would be cold and uninterested in anyone whose views were unknown or unacceptable at the Royal College of Surgeons.

The thirty and odd years of his life in London are divided naturally into two nearly equal parts. For the first half he was busily engaged in hospital and private practice; but more and more time had to be given to public affairs: congresses, societies, the Council of the College of Surgeons, the Lister Institute, and the like. Laboratory researches continued in full swing, but they were chiefly devoted to the search for a perfect catgut and a perfect gauze. He was given a baronetcy in 1883, one amongst very many distinctions and honours. The old jealousies of the London surgeons gradually disappeared. He fell naturally and by degrees into the position of one of the leaders, and as time went on he was expected to take a prominent part in most questions affecting the interests of the profession, and in public matters where expert medical knowledge was required. Meanwhile the strenuousness of life was relieved by longer and more frequent holidays in many parts of the world.

All this—or nearly all—came to an abrupt end in 1893 with the sudden death of Lady Lister while they were in Italy, and his retirement, owing to the age limit, from the surgeoncy at King's College Hospital.

Now, therefore, at the age of 66 he was a childless widower, and entered on the second half of his life in London, with no hospital appointment and without private practice, for he did not think it right to continue to give expert surgical advice when he no longer had that opportunity of keeping thoroughly abreast of the times, which is the privilege of hospital surgeons alone. Laboratory work was also almost laid aside, partly perhaps because it emphasized the loss of his wife, who had been for so many years his faithful assistant and amanuensis,

but chiefly because—except for the interminable question of the catgut—there was no need for further improvement in the dressings; and, as to bacteriology, it had become so complex and had passed so much into the hands of specialists that there was little temptation to embark in fresh researches of this nature.

There remained therefore only his public duties to which, with a heavy heart, he manfully devoted himself.

Fortunately a new sphere of interest and usefulness presented itself through his appointment to the post of Foreign Secretary to the Royal Society, and this was widened in 1895 when he succeeded Lord Kelvin in the Presidency, which he held for the usual period of five years. A conscientious president must devote much time to routine and to the semi-public and public duties which this high office entails. In Lister's case it involved much reading on many subjects connected with medicine, such as cattle plague, sleeping sickness, bubonic plague, diphtheria, malaria and vaccination. His annual addresses show how completely he kept himself up to date in these and other more technical matters, and should be read by anyone who is disposed to look at him as a man of one idea. During his presidency he was also President of the British Association in 1896. His opening address on "The Interdependence of Science and the Healing Art"[1] fully bears out the truth of what has just been said.

In 1897 he was raised to the peerage, and again gave evidence of his wide outlook on matters outside the province of a surgeon, such as public health and sanitary science, by the part he took in the debates in the House of Lords on venereal disease, vaccination and early closing. It is instructive to observe that, in the arena of politics, a man of such clear and positive views was forced, much against his will, to adopt the tactics of the

[1] *Collected Papers,* vol. ii, p. 489.

opportunist; advocating the second best because the best appeared to be unattainable.

When the Presidency of the Royal Society came to an end in 1900 Lister began to feel almost for the first time that he was an old man, and, indeed, he had scarcely more than two more years of active life before him. The chief events in these two years were his delivery of the Huxley Lecture in 1900, and the part he took in the Tuberculosis Congress in 1901.

The Huxley Lecture is delivered biennially at Charing Cross Hospital. Michael Foster gave the first. The second was by Virchow and was presided over by Lister. It was followed by a great meeting at Liverpool on the occasion of the opening of the Thompson-Yates laboratories for physiological and pathological research. Lister was the hero of the day, and made a much-applauded speech on a favourite theme of his, the absolute necessity for practical demonstration in physiology and pathology as well as mere theoretical instruction. Some, however, doubted the wisdom of fanning again the dying embers of the strife over vivisection.

Lister's was the third Huxley Lecture. He made it the occasion for recalling and reviewing his earlier physiological and pathological work, especially "some of the more salient of such observations as bear more or less directly upon the antiseptic system of surgery." It was a fitting subject for his last great public address.

At the second Tuberculosis Congress, which was held in London, Koch made the unexpected and startling announcement that bovine tubercle is rarely if ever transmitted to human beings. If this had been the case all the accepted teaching with regard to the danger of infection from tuberculous milk and tuberculous meat would have been wrong, and all the precautions which were being taken would have been unnecessary. Lister presided at the meeting and, on the spur of the moment,

exposed, in a well-reasoned speech, a great flaw in Koch's argument, showing that at this time he had lost nothing of his clearness of thought and power of exposition. After the Congress was over a Royal Commission was appointed to look into the whole subject and he took a keen interest in its earlier practical and experimental work. He also continued, up to the autumn of 1902, to superintend some experiments at the Lister Institute which effectually disposed of many of Koch's arguments.

He had already held several Crown appointments. In 1902 he was made Sergeant Surgeon to King Edward VII. He also received the Order of Merit and was admitted to the Privy Council. From this time forward the public heard less and less of him. There were late honours: honorary degrees and other distinctions. There were celebrations of his eightieth birthday at home and abroad, and soon after came his last public function, when the Freedom of the City of London was presented to him. But he never really recovered from a serious illness which attacked him in 1903 and the remaining nine invalid years of his life contain no other incidents that need noting here.

He died on February 10, 1912, well advanced in his eighty-fifth year. There was a great public funeral service in Westminster Abbey, where afterwards a medallion of him was placed amongst a group of some of the most distinguished scientific men of the Victorian age. He was buried by the side of Lady Lister in the West Hampstead Cemetery.

Like Pasteur, and more fortunate than many pioneers, Lister lived long enough to see the value of his work recognized, and died loaded with honours from his grateful contemporaries. He lived long enough indeed to see other men "entering into his labours"; a source of pleasure and thankfulness, perhaps even of a humble sort of pride, but tempered by a few misgivings lest some

of the "advances" might turn out not to be real advances, which caused him a certain amount of genuine sorrow.

Readers of the following papers may perhaps want to know something more about the personality of their author.

He was a well-built muscular man, about 5 ft. 11 in. in height, fond of active exercises, especially of swimming and skating, in the rare intervals of leisure, and all his life he was an eager fisherman.

His face I need not describe as there are two portraits, but may draw attention to the unusual height above the eyes and the irregularity about the mouth. His benevolent smile cannot be given.

We gather from early letters that he was a light-hearted boy, and, with one short interval, he may be said to have been light-hearted and almost boyish for the greater part of his life, always ready with an appropriate joke or jokelet, a play upon words or a ridiculous rhyme. But this was only in off moments. When at work his whole energies were concentrated upon it, regardless of the lapse of time, of meals, or other engagements. This virtue often postponed the beginning of the next duty, so that he was apt to be unpunctual, and seldom completely ready even for his public addresses, the final preparation for which was generally put off to the last possible moment.

His boyhood had been happily spent with his three brothers and three sisters in their parents' old home at Upton in Essex. It was then in the country, though only five miles from London. They were all, parents and children, a "united family," devoted to one another and sharing the same interests. Two of his brothers died in early manhood. It was with his youngest brother Arthur and his father that the closest intimacy existed.

He was a delightful companion to his many nephews and nieces, joining in their pursuits and pleasures and much interested in their careers, though as he grew older and acquired something of an old-world ceremoniousness of manner, young people, and some older ones, owned to feeling a certain amount of awe in his presence.

To his Scottish students he was always "the chief." Their devotion to him was unbounded, and, as they were cosmopolitan, they carried their memory of him and of what they had learned from him to all the corners of the earth.

As a speaker he was clear and gave the impression of being very much in earnest. He spoke rather slowly and with a slight stammer, especially when he was overworked. His voice was soft and melodious, and though it was not loud, he was easily heard by a large audience. He did not aim at eloquence, but merely a perfectly intelligible statement of what he had to say.

He was a good linguist: could make public speeches in French and German, read Italian easily, and continued through life to acquire some knowledge of other continental languages such as Spanish, Norwegian and Dutch. Horace, Dante and Goethe were favourite and constant companions.

He inherited from his father a facility with the pencil and brush and showed it by the style of his innumerable anatomical, pathological, botanical and microscopical drawings. But he did not develop this talent in any other direction.

I suppose it may be said that he was naturally musical, for he learnt to play the flute in a family where music was taboo, and was familiar with the airs of many a Scotch ballad.

No distracting pursuits interfered with his scientific work. He never fell into the snare of making collections, except a modest one of plants which served only as a record of his holidays.

He followed with keen interest both foreign and home politics. Born to a heritage of early Victorian Liberalism which was almost universal amongst the Quakers of his youth, he gradually lost sympathy with its increasingly Radical tendencies, and ended as a warm Unionist or even Conservative.

He was a good operator, very neat, very thorough and consequently slow; most punctilious about the smallest details, especially the arrest of bleeding as was essential for complete success with the antiseptic system. In these respects—except in the matter of speed—his was the pattern on which the modern style of operating is based. He introduced several new operations, and invented some new instruments and in many other ways advanced technical surgery.

As an experimenter he was full of resource, sound in interpreting results, and quick in seeing what the next step in a research should be. With his dexterous fingers he made much of the apparatus which he needed. The rest, which was generally from his own design, was remarkably simple.

Lister never lost that firm hold on "spiritual things" which was an essential part of the education he received from his parents. He took a serious view of the duties and responsibilities of life. Without being obtrusive, this was obvious to all students and friends and casual acquaintances. Conversation therefore in his presence naturally confined itself within the strict limits of decorum; and a jest savouring of irreverence would have drawn from him not a smile but a frown.

It is presumptuous to try to forecast the "verdict of history" on Lister, especially if we mean by this well-worn phrase the opinions of the historian or historians who at some future time may attempt to write his life. The futility of such verdicts is proved by the frequency with which they are reversed. Still it is safe to prophesy

that his genius will be acknowledged, and that it will be explained or accounted for by the tiresome and generally misquoted definition that genius "is transcendent capacity of taking trouble, first of all." Possibly some captious critic may doubt his originality because he did not invent the germ theory himself, or do Pasteur's work before he began his own, but none can deny that he was the first to grasp its bearing on wound treatment, and the first to proclaim the necessity and the possibility of making surgery aseptic, a doctrine which he preached with such untiring vigour that all the world was at last converted. It will have to be allowed that he showed what the new surgery was to be by performing operations that had previously been considered unjustifiable, and that it was only by following his example that the enormous advances of the last half century have been made. The honest historian must give him most of the credit for the introduction of the present slow and careful method of operating in place of the slap-dash performances of our predecessors, and all the credit for the substitution of the short absorbent ligature for the clumsy and dangerous ligatures they employed.

As Lister grew older he looked back with fond affection to his early physiological and pathological work. It troubled him to see how far it had been forgotten and he feared that it would pass still further into oblivion. Perhaps his fears will be justified. The march of the two sciences is so rapid that it is almost impossible to give adequate recognition to each stage, much less to every step. One object of this book will have been attained if its readers are encouraged to search in the *Collected Papers*[1] for more of the early work of which it has been only possible to give one example.

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[1] *The Collected Writings of the Right Hon. Lord Lister, O.M., F.R.S.* 1909. Two volumes. 4to. Pp. 1015. 42s. net. Clarendon Press, Oxford.

ON THE EARLY STAGES OF INFLAMMATION.

Collected Papers, vol. i, p. 209—*Philosophical Transactions*, vol. cxlviii, Part II for 1858, p. 645.

When this paper was read before the Royal Society on June 18, 1857, Lister was just over 30 years old. It was the result—or rather one of the results—of two years' hard work. They had been crowded years; he obtained an assistant-surgeoncy and began to lecture to students soon after his marriage in 1856, and for nine months he had been enjoying the first pleasures of domestic life at 11, Rutland Street, near Syme's consulting rooms, and not very far from the Royal Infirmary.

The research was undertaken in order to obtain a firm groundwork for his lectures on Surgical Pathology. Its outcome was this paper and two others, which were all presented to the Royal Society on the same day. One of these, "On the Cutaneous Pigmentary System of the Frog" (*Collected Papers*, vol. i, p. 48), is of great interest. It clinches the argument that irritants cause "paralysis" of the *tissues* by showing how these same irritants influence the concentration and diffusion of the pigment in the stellate cells of the frog's web. The other paper, "An Inquiry into the parts of the Nervous System which regulate the Contraction of the Arteries" (*Collected Papers*, vol. i, p. 37), is rather tough reading.

The paper was somewhat modified from its original form owing to Sharpey's criticisms. The history of previous observations and theories was omitted and

also some details. "The author," Sharpey reported, "appears to have laboured under the influence of recent discussions in Edinburgh [probably regarding the exploded theory of a 'vital moving force' exercised by the tissues on the flow of blood through the capillaries] which . . . has led him to give unnecessary extent to comparatively trivial and uncertain things." Sharpey objected to the term "paralysis" of tissues, preferring the word "arrestment"; but on this point Lister did not yield.

The text-books of the day contained long articles on inflammation, and the subject was discussed in medical and surgical lectures. As a rule, after enumerating all the theories, ancient and modern, regarding it, and discussing whether it was due to exaltation or diminution of function, the authors expatiated on the meaning of the four classical signs—heat, redness, swelling and pain—and ended by a description of the results of acute and chronic, simple and specific, and other varieties of inflammation in different parts of the body. A few, such as J. Thomson, Wilson Philip and his friend Charles Hastings—the founder of the British Medical Association—had studied, though not profoundly, some of the earlier stages, but no one before Lister had investigated the very earliest stages of all. Perhaps an exception should be made in favour of Georg K. Kaltenbrunner of Munich, who died in 1826 at the age of 30, and who wrote in French "out of compliment to French physiologists"! His observations were made upon the frog's web with the imperfect microscope of his day, and he saw a good deal, and made some shrewd remarks about congestion. The classical symptoms are only referred to in this caustic way near the end of his paper: "La tumeur, la rougeur, la chaleur et la douleur sont des phénomènes qu'on observe assez constamment, lorsque l'inflammation est extérieure. L'esprit humain, pour

dissimuler son ignorance, a, par une vanité bien singulière, pris les effets d'une cause pathologique pour l'essence même de la maladie, et a donné ainsi les quatre phénomènes de l'inflammation comme l'affection lui-même et a transformé les conséquences d'une cause en propriétés." He did not anticipate immediate acceptance or approval of his work and his anticipations came true. But other observers, amongst whom were Wharton Jones and Paget, kept on investigating the phenomena of congestion and determination of blood; and the frog's web was a recognized field for such investigations.

We must keep reminding ourselves that this paper deals only with the very commencement of inflammation, and also that the only exciting causes considered were heat, and mechanical, chemical and electrical irritation. Inflammation brought about through the influence of the nervous system is briefly referred to, and so are "spontaneous" and "idiopathic" inflammation. These last words remind us that the power of micro-organisms to produce inflammation was not dreamt of. The overwhelming share they are now known to take in the process has no doubt helped to distract attention from the value of Lister's work in this field.

And yet it was a remarkable performance for a young surgeon. Examples are, of course, easily recalled of pure surgeons who were pathological experts. There was more reason for this in Lister's early days than now, for there were then no Professors of Pathology. There were Professors of Morbid Anatomy and Pathological Anatomy; but at University College, for example, the chair of Pathology was not founded till 1887, and the first occupant, strangely enough, was a young surgeon, Victor Horsley.

Lister's studies of inflammation were closely linked up with those which he was conducting at the same time on the coagulation of the blood, and bore fruit long

afterwards in his investigations on the vitality of blood clot and its resistance to the attacks of micro-organisms.

It is a long paper, divided into an Introduction, four Sections and a Conclusion. Lack of space prevents us from reproducing more than Section IV and the Conclusion.

Section I, "On the Aggregation of the Corpuscles of the Blood," ends as follows: "From the facts detailed in this section, it appears that the aggregation of the corpuscles of blood removed from the body depends on their possessing a certain degree of mutual adhesiveness, which is much greater in the colourless globules than in the red discs; and that, in the latter, this property, though apparently not depending upon vitality, is capable of remarkable variations in consequence of very slight chemical changes in the liquor sanguinis."

Section II, "On the Structure and Functions of the Blood-vessels," concludes as follows: "The arteries regulate by their contractility the amount of blood transmitted in a given time through the capillaries, but neither full dilatation, extreme constriction, nor any intermediate state of the former is capable *per se* of producing accumulation of corpuscles in the latter."

Section III, "On the Effects of Irritants upon the Circulation," is summed up thus: "The effects produced upon the circulation by the application of an irritant to a vascular part are two-fold, consequent upon two primary changes in the tissues, which, though often concomitant, are entirely independent both in nature and mode of production. One of these is dilatation of the arteries (commonly preceded by a brief period of contraction), giving rise, in proportion to the increase of calibre, to more free flow through the capillaries, the blood remaining unaffected, except in the rate of its progress. This purely functional phenomenon is developed indirectly through the medium of the nervous system, being not

limited to the part acted on by the irritant, but implicating a surrounding area of greater or less extent. The other change is the result of the direct operation of the irritating agent upon the tissues, which experience some alteration, in consequence of which the blood in their vicinity becomes impaired, losing the properties which characterize it while within a healthy part, and which render it fit for transmission through the vessels, and assuming those which it exhibits when removed from the body and placed in contact with ordinary solid matter. The first indication of this disorder of the vital fluid is that its corpuscles, both red and white, acquire some degree of adhesiveness, which makes them prone to stick to one another and to the vascular parietes, and, lagging behind the liquor sanguinis, to accumulate in abnormal numbers in the minute vessels. This adhesiveness may exist, in proportion to the severity of the affection, in any degree, from that which merely gives rise to a very slight preponderance of the corpuscular elements of the blood in the part, up to that which induces complete obstruction of the capillaries; and when the irritation has been very severe, the liquor sanguinis also shows signs of participation in the lesion by a tendency to solidification of the fibrine.'"

Section IV, which has been chosen for the first of this series of papers, is the most important of all. The other sections lead up to it. It begins on the opposite page, and is printed almost entire, with the omission of a page or two and most of the notes. It deals with "The Effects of Irritants upon the Tissues."

ON THE EARLY STAGES OF INFLAMMATION.

SECTION IV (*Collected Papers*, vol. i, p. 246).

ON THE EFFECTS OF IRRITANTS UPON THE TISSUES.

The object of the present section is to inquire into the nature of that primary change which we have seen to be produced in the tissues by the direct action of irritants upon them.

The conclusion already arrived at, that blood flowing through an irritated part approaches more and more nearly, in proportion to the intensity of the affection, the condition which it assumes when separated from the living body, naturally leads us to infer that the tissues concerned are in some degree approximated to the state of ordinary matter, or, in other words, have suffered a diminution of power to discharge the offices peculiar to them as components of the healthy animal frame.

This inference is strongly supported by considering what common effect is likely to be produced upon the tissues of the frog's web by all the various agents known to cause inflammatory congestion. To take first the case of mechanical violence. A forcible pinch of the delicate web seems likely, *a priori*, to impair its powers; for if the lesion be sufficiently severe, complete death of the part will result. An elevated temperature proves equally destructive if carried far enough; and its operation to a degree just short of this, while it produces congestion, can hardly fail to cause diminished vigour in the tissues. So also powerful chemical agents, if used cautiously, give rise to inflammation; but if otherwise, kill the part they act on. Even the pungent irritants which do not exert much chemical action, seem to benumb the energies of the spot to which they are applied. Thus

a morsel of capsicum placed on the tip of the tongue speedily produces numbness there, and a piece of mustard lying on the finger for an hour or two dulls the sensibility of the skin. Chloroform, too, while it very readily induces stagnation followed by vesication in the frog's web, is an agent which appears likely to benumb the vital energies. If a small frog be put into a bottle of water highly charged with carbonic acid, and removed from it some time after all motion of the limbs has ceased, it will be found that, though the heart is still beating, the blood-vessels of the webs are loaded with stagnant corpuscles. After a while, however, resolution will take place, and some time later the animal will regain its consciousness. Here it appears probable that the carbonic acid, poisoning the web as well as the brain, paralyses for a time the functional activity of both; and that the return of the circulation, like the recovery of the cerebral functions, depends on a restoration of the dormant faculties of the affected tissues.

Perhaps the most instructive case is that of the galvanic shock, which the following circumstances first showed me to be capable of causing inflammatory congestion. Being desirous of ascertaining the effects of galvanism upon the cutaneous pigmentary system, I applied the poles of a battery in rather powerful action to the skin of the head of a frog, when, the shock affecting the brain, the animal was stunned and lay perfectly motionless. This state of things being favourable for pursuing my inquiry by aid of the microscope, I drew down one of the passive limbs, and having placed the foot under the instrument, arranged the fine platinum-wire extremities of the poles at a short distance from one another at opposite sides of one of the webs, so that the current might pass through a part in the field of view, the circulation meanwhile remaining healthy. I now completed the circuit of the battery, when the leg became instantly drawn up by reflex action; yet on re-examination of the web, I found that, momentary as the shock had been, the part through which it had passed had become affected with intense inflammatory congestion, gradually shading off towards the healthy condition, which existed at a little distance. After about a quarter of an hour resolution of the confused mass of stagnant corpuscles occurred, and shortly after this the creature regained the power of voluntary motion. I afterwards repeated the experiment, both upon

the same animal and upon another specimen, and always with the same results; and I particularly observed in one case that the white corpuscles were affected with great adhesiveness in the congested region.

With regard to the manner in which the abnormal condition of the blood was brought about in these cases, it has been already mentioned in Section I that the galvanic current produces no increase of the adhesiveness of the red corpuscles of blood outside the body; but after what has been stated in the last section, the reader will see no reason to think such an effect likely. It may, however, seem not improbable that the galvanic shock might, by its direct action upon the blood within the vessels, reduce it to the same condition as if removed from the body. But that this was not really the cause of the congestion, was clear from the fact that in the parts less intensely affected, where the corpuscles still moved slowly though possessed of considerable adhesiveness, the same condition continued long after all the blood which was in the vessels when the shock was transmitted had passed away. In this case, therefore, as in all others which we have considered, the blood was affected secondarily to the tissues. This being established, the natural interpretation of these experiments appears to be, that the portion of the web affected was, as it were, stunned by the shock, and its functions suspended like those of the brain; the resolution of the inflammation, like the return of volition, depending on recovery of function on the part of the tissues concerned.

From such considerations as these, it appears that all those agents which produce inflammatory congestion when applied to the web, though differing widely in their nature, agree in having a tendency to inflict lesion upon the tissues and impair their functional activity.

But powerful as are the arguments thus obtained by inference, it is very desirable to confirm them by direct observation, and it fortunately happens that the cutaneous pigmentary system of the frog is a tissue which discharges functions very apparent to the eye, so that it is easy to trace their modifications under the influence of irritation.

In the first experiment with mustard described in the last section (performed September 29, 1856), the space on which the irritant had acted presented a very striking difference from the

rest of the web in the appearance of the pigment, which in healthy parts was in the form of small roundish black dots; while in the mustard area, and accurately corresponding to the extent of stagnation in the capillaries, each spot was extended to a stellate figure. [*Cf.* fig. 3 of Plate.]

I thus became for the first time aware that the pigment is capable of variations, and my attention having been directed to the subject, I soon found that similar changes occur spontaneously, and give rise to alterations in the colour of the skin, which is paler in proportion as the colouring matter is more completely collected into round spots. For some weeks I supposed myself to have been the first discoverer of this curious fact, till I was referred by Dr. Sharpey to the recent labours of the Germans on the subject. They, however, as I afterwards found, had taken an entirely erroneous view of the phenomenon, attributing the round form of the masses of pigment to contraction of the branching offsets of stellate cells; whereas it turned out that the chromatophorous cells do not alter in form, but that the colourless fluid and dark molecules which constitute their contents are capable of remarkable variations in relative distribution, the molecules being sometimes all congregated in the central parts of the cells, the offsets containing merely invisible fluid, while at other times the colouring particles are diffused throughout their complicated and delicate branches; and between these extremes any intermediate condition may be assumed. It further appeared that concentration of pigment takes place in obedience to nervous influence, while diffusion, though also an active vital process, tends to occur when the pigment-cells are liberated from the action of the nerves. But for further particulars on this subject, I beg to refer the reader to a previous paper in this volume.[1]

The contrast between the pigment in the area on which the mustard had acted and that of surrounding parts in the case last alluded to, at once struck me as probably the result of a direct action exercised upon the tissues by the irritant. It seemed possible, however, that it might be a secondary effect of the state of the blood in the congested vessels; and in order to ascertain

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[1] See *Collected Papers*, vol. i, pp. 48 *et seq.; Philosophical Transactions,* pt. ii for 1858, p. 627.

which was the truth, I performed, on October 14, the following experiment :—

Having cut out a piece of the web of a healthy frog, I placed a small portion of mustard upon its centre when all the blood had escaped from it. After a while the spots of pigment seen through the thin margin of the mustard, presented a stellate form, while in the rest of the piece they were still of a rounded figure. Hence it was clear that the change in the disposition of the pigment was the result of the direct action of the mustard upon the tissues of the web.

A new field of investigation was thus opened before me, promising to throw great light upon the nature of inflammation.

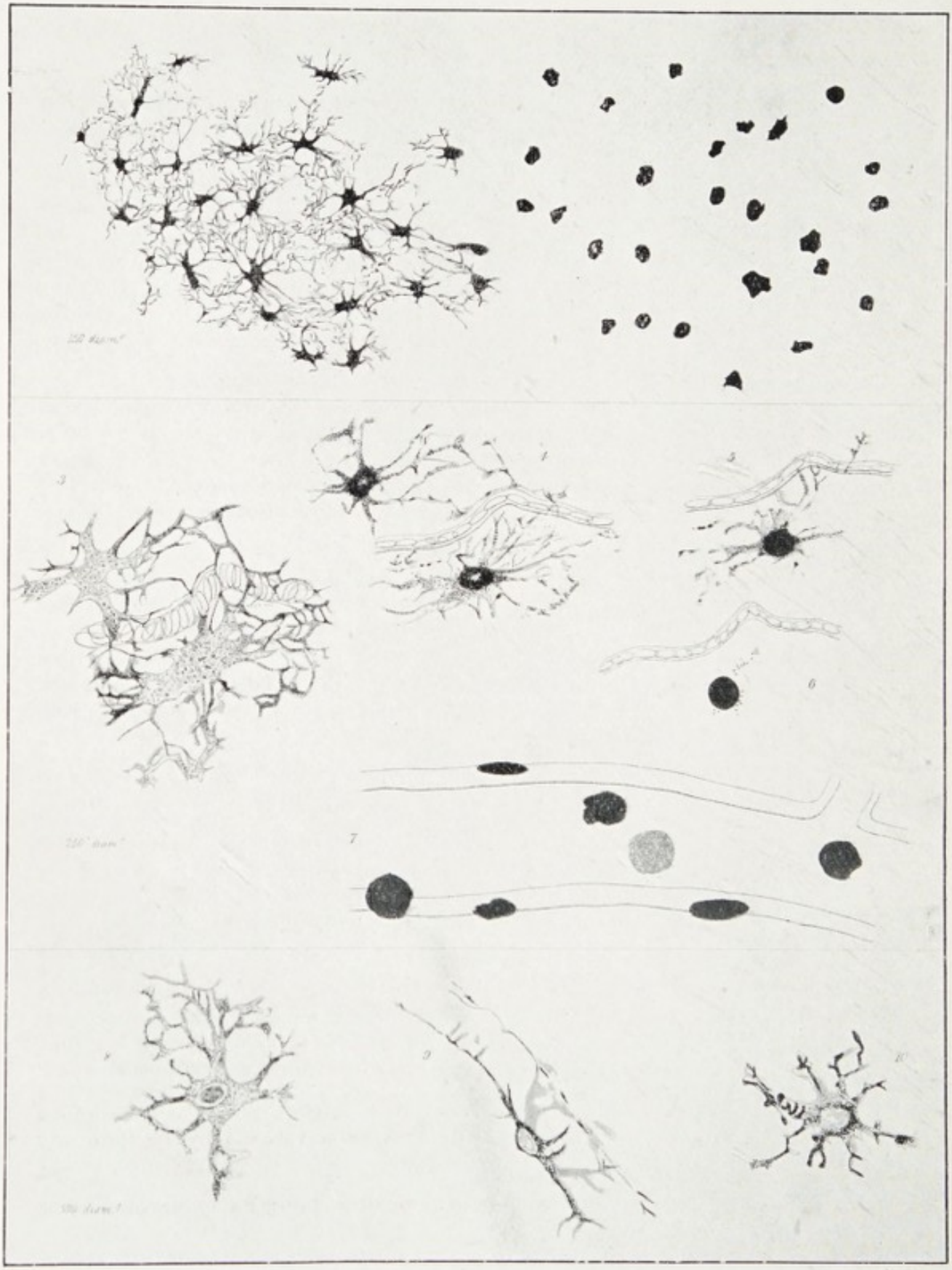
To explain the effects of irritants upon the pigmentary tissue proved, however, to be a matter of considerable difficulty. Tincture of cantharides and croton oil, which happened to be among the first substances which I employed with reference to this subject, resembled mustard in causing diffusion of the pigment. Taking, in the first instance, the same view of this change as the German authorities, I attributed it to the relaxation of contractile cells, and regarded its occurrence, in consequence of irritation, as an indication of loss of power in the tissues, a view which was in harmony with the nature of the derangement of the blood in a congested part. Croton oil, curiously enough, acted very slowly on the web, not producing any change on either pigment or blood for an hour or more : also its effects appeared inconsistent with my theory; for while it ultimately gave rise to diffusion of the pigment to even a greater extent than I had seen occur with mustard, yet it induced only comparatively slight appearances of congestion. Chloroform also seemed at first still more anomalous in its operation, though in the opposite way; for though it was pre-eminently potent in inducing congestion, it caused no alteration whatever in the appearance of the pigment, whether mildly or strongly applied.

Afterwards, as the true nature of the pigmentary functions became unveiled, and further facts were developed, these difficulties were completely cleared away. The first step towards their solution was made in an experiment with ammonia. A frog being placed under chloroform, I covered the whole of the foot with sweet oil, except a small area in one of the webs,

the pigment being in the stellate condition, i.e., about midway between perfect concentration and full diffusion. An assistant then held at a short distance above it a piece of lint soaked in the strongest liquor ammoniae, so that its pungent alkaline vapour might play upon the exposed area, while the rest of the foot was protected by the oil. This having been continued for a few seconds, accumulation of corpuscles and stagnation occurred in the vessels of the area, without any change in the appearance of the pigment. After a while, however, the creature happened to grow pale, and, in the web generally, the pigment became completely concentrated so as to assume the dotted aspect, but in the part which was the seat of congestion it remained stellate as before. Hence it appeared that though the ammonia did not cause any change in the distribution of the pigment, it had in reality produced a great effect upon the chromatophorous cells, which, in the area exposed to its influence, had been deprived of the power of concentration by the mildest degree of action of the alkali that sufficed to induce stagnation of the blood. On examination of the web about four hours later, resolution of the stagnation was found to have taken place, though there was still some excess of corpuscles, with marked adhesiveness of the colourless ones in the vessels of the ammonia area. The creature was now released for the night. Next morning the integument was in the opposite extreme of colour, being almost black, and the pigment had the reticular appearance, being fully diffused throughout the whole web, except the central part of the ammonia area, where it retained the same stellate condition as the day before. Hence it appeared probable that the diffusive power, as well as the concentrating, had been paralysed by the ammonia, but had been recovered in all the area except the part that was likely to be the last to regain its functions. To ascertain whether the concentrating power had also been regained, I killed the frog and amputated the leg; soon after which the usual post-mortem concentration took place completely in the web generally, while in the central part of the area the medium state was still retained, and in the rest of its extent concentration considerably beyond the medium state, but short of the full degree, supervened, showing that recovery of function had taken place to a considerable extent, but was not yet quite complete.

I now felt little doubt that chloroform also possessed the

PLATE III.



Lister, del. Tuffin West, sculp. Bale & Danielsson, Ltd., imp.

PIGMENT CELLS OF THE FROG.

PLATE III

Illustrates the anatomy and physiology of the cutaneous pigmentary system of the frog.

Figs. 1 and 2 are sketched from webs of different feet of the same animal. The creature was dark when it was killed, but one of the legs afterwards underwent the usual post-mortem change to a pale colour, and such was its state when Fig. 2 was drawn. The other limb was deprived of the power of thus altering by immersion for half a minute in chloroform; and Fig. 1 shows the appearance of the colouring matter in the permanently dark condition of the integument.

Fig. 3 represents two chromatophorous cells with the pigment-granules fully diffused, the animal having been at the time coal-black. The bodies of the cells are seen to be pale, containing chiefly colourless fluid, while some of the finest branches of the offsets are quite black, in consequence of the dark molecules being closely packed together in them. In the same figure a capillary fully distended with blood-corpuscles is also given.

Fig. 4 represents the colouring matter in the same two cells during the progress of the process of concentration. The dark molecules are already for the most part collected about the middle of the body of each cell; but in the very centre of each cell is a pale point, where the granules seem not yet to have insinuated themselves between the cell-wall and the nucleus. The same capillary is here seen much reduced in calibre.

Fig. 5 shows the pigment in the lower of the two cells, concentration being still further advanced.

In Fig. 6 the process is seen to be almost absolutely completed, the molecules being almost all of them aggregated into a black circular mass, occupying the middle of the body of the cell, the more circumferential parts of which contain only colourless fluid, and are therefore invisible.

Fig. 7 is an outline of the wall of a large blood-vessel, with chromatophorous cells in its external coat. The pigment is almost completely concentrated, but the form of the section of the black masses, where they are seen edgewise, shows that they are not spherical, but disc-shaped.

Figs. 8, 9 and 10 are drawn, with a much higher power, from young frogs, with small pigment-cells; they exhibit especially the form and relations of the nucleus.

In Fig. 10 the pigment is shown in an unhealthy state, the molecules being irregularly aggregated.

power of arresting the pigmentary functions; but in order to prove the fact I killed a dark frog, and placed one of its legs in that fluid for half a minute, and then wrapped both it and the other leg in damp lint. After some hours the limb which had not been treated with chloroform was quite pale, while the other, having lost the faculty of post-mortem concentration, remained as dark as before. The appearance presented by the pigment in the two feet is shown in the Plate, figs. 3 and 4.

Mechanical violence proved similar in its effects on the pigment, which, in the area pinched, retained the same appearance as before, except that in parts where the pressure operated most severely the cells seemed sometimes to have suffered rupture. Fig. 2 of the Plate is a camera-lucida sketch of part of a spot which had been compressed by means of padded forceps, with an adjoining uninjured portion of the web. The pigment was fully diffused before the experiment was performed, and remained so afterwards in the area squeezed, while it became concentrated elsewhere, and this was the condition of things when the drawing was made. The concomitant differences in tint between the blood in the affected and the sound parts in consequence of the accumulation of closely packed red discs in the former, are also strikingly shown in the sketch.[1]

The galvanic shock, too, produced no effect apparent to the eye upon the pigment of the parts in which it caused stagnation of the blood, but experiments afterwards made showed me that, like ammonia, it exerted a paralysing agency both upon the concentrating and the diffusive powers; and the same results

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[1] Much more gentle pressure, if long continued, may give rise to similar results, as I happened to notice in the following manner. Being desirous of watching the process of post-mortem concentration of the pigment, I amputated a leg of a dark frog, and, having stretched out the foot over a glass plate, put a small piece of thin glass upon part of one of the webs, and applied a high power of the microscope to it. I was disappointed to find, however, that the change I wished to observe did not take place; but on looking at other parts of the web, found that immediately beyond the edge of the slip of thin glass the pigment was on all sides considerably concentrated, although remaining fully diffused where the glass covered it; an effect which I could attribute only to the gentle squeezing to which the two plates subjected the part of the web that lay between them.

ensued on the application of dry heat in the cases mentioned in the last section.

From these and other similar facts it appeared that mustard, croton oil, and cantharides are exceptional as regards the diffusion to which they give rise, the usual course being that irritants, when applied so as to produce stagnation of the blood, suspend at the same time both the functions of the pigment-cells.

It afterwards turned out that mustard was, in reality, no exception to this general rule. Subsequent experiments showed that diffusion takes place to very different degrees in different instances under the action of this substance, but that in all cases, after reaching a certain point, it becomes incapable of advancing further in the irritated part, however much it may increase in the body generally, in case of the animal changing to a darker colour. These differences depend partly upon the strength of the mustard, the diffusion being least when the irritant is most potent. Thus, on one occasion, when a solution of the volatile oil in spirit of wine was applied to a web in which the pigment was fully concentrated, congestion was very rapidly developed, without any alteration in the appearance of the chromatophorous cells. That the diffusion is in inverse proportion to the energy with which the mustard acts, was well illustrated by the experiment which furnished the drawing given in the Plate, fig. 1.[1] In that case, a frog having been prepared [by dividing the spinal cord from the brain], a portion of very strong mustard was placed upon the middle of one of the webs, the pigment being in the stellate condition, such as is seen on the left-hand side of the sketch, which represents a part of the edge of the spot to which the irritant was applied, together with an adjoining portion of the web. Shortly after this had been done, I noticed that the pigment was in a state of full diffusion in a ring round about the opaque mass, producing the reticular appearance shown in the stripe down the middle of the sketch. I had in a previous case seen a similar ring become affected with congestion, when a portion of mustard had been applied for a long time, in consequence of the pungent vapour of the volatile oil playing upon the neighbouring parts of the web, and

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[1] This experiment was performed subsequently to the reading of the paper.

there could be no doubt that the effect on the pigment in the present instance was due to the same cause; but in the latter no material change was as yet visible in the blood except close to the edge of the mustard, where the corpuscles were seen to be abnormally adhesive. After the lapse of about an hour, the area on which the irritant had lain being examined, was found to be the seat of the most intense inflammatory congestion, indicated in the drawing by the crimson colour of the vessels, but the pigment there had experienced only an exceedingly slight degree of diffusion, being, in fact, almost exactly in the same state as at the commencement of the experiment. Thus the vapour of the volatile oil, though operating too mildly to cause inflammatory congestion, nevertheless induced the highest possible degree of pigmentary diffusion; but the mustard, where it lay actually in contact with the web, and acted energetically upon it, arrested that very process of diffusion to which its gentler operation gives rise.

In the progress of the case it happened that the animal changed from the medium tint which it had at first to a very pale colour, the pigment, in the web generally, assuming the dotted condition depicted on the right-hand side of the drawing. Yet many hours after the mustard had been removed, the pigment on which it had acted retained its stellate disposition, and the reticular appearance in the surrounding ring also remained unchanged, showing that the power of concentration had been permanently lost in those parts, and affording a favourable opportunity for obtaining by means of the camera lucida a delineation of the medium, and both extreme conditions of the pigment in the same web. Next day the experiment was rendered still more instructive by the skin becoming excessively dark, the pigment undergoing full diffusion in the healthy parts of the web, so that the contrast between the ring about the congested area and the surrounding regions no longer existed: yet the stellate condition was still maintained where the mustard had lain, showing that it had suspended the faculty of diffusion no less than that of concentration.

Croton oil now no longer seemed anomalous in its operation. Its curiously slow action upon the frog is comparable to the mild influence of the vapour of mustard, and the slight amount of inflammatory appearance which I had sometimes

observed in a part where it had caused a great degree of pigmentary diffusion, is strictly analogous to the healthy state of the circulation in the reticular ring round the congested area in the last experiment.

Cantharides also presents a parallel case. Its action is even more slow than that of croton oil; and on referring to notes taken at an early period in this investigation, I find that in one instance, when two hours and a half had elapsed after the application of a small drop of the tincture to the web, though diffusion of the pigment had become apparent in the area on which it had acted, no change of the blood had yet been observed; and an hour and a half later, the red corpuscles, though abnormally adhesive as compared with those in surrounding parts of the web, were still moving slowly through the vessels.

Hence it appears that diffusion of the pigment may be produced by either of these three substances without the blood undergoing any material derangement, and therefore that its occurrence under their influence is to a great extent, if not entirely, independent of the inflammatory process. On the other hand, it has been demonstrated, as regards mustard, that when stagnation of the blood has been developed through its action, the state of the pigment-cells is the same as is induced by irritants generally, viz., a complete suspension of functional activity; and, from analogy, we may be pretty sure that this is also true of croton oil and cantharides, although their slow operation renders it difficult to obtain absolute proof upon the point.

In a physiological point of view, it is an interesting question, what is the cause of the diffusion of the pigment induced by these three irritants. I have shown elsewhere[1] that concentration is the invariable result of the action of the nerves upon the chromatophorous cells, and that diffusion takes place whenever they are liberated from nervous influence. Also in the tree-frog of the Continent, which is much more liable to changes in the colour of the integument, in consequence of direct irritation, than our own species, the invariable experience of the German observers was, that concentration followed the application of a local stimulus, while secondary diffusion sometimes occurred in the

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[1] See the paper "On the Pigmentary System," *Collected Papers,* vol. i, p. 64.

irritated spot, depending apparently upon exhaustion. From these facts, diffusion ensuing on irritation cannot well be regarded as an increased action excited by the stimulus, but rather as an evidence of diminished vigour. With croton oil and cantharides, which have not an irritating vapour, the diffusion is exactly limited to the extent of the irritant, showing that it is due to a direct action on the tissues; and the most probable explanation of its occurrence appears to be that mustard, croton oil, and cantharides have the peculiarity among irritants of affecting the nerves of the pigment-cells in the part they act on, somewhat more rapidly than the cells themselves, and, paralysing the former while the latter still retain their powers more or less intact, permit diffusion to go on unrestrained by nervous influence, till the further operation of the irritant completely suspends the pigmentary functions. It may be objected to this view, that diffusion occurs on the application of these substances to an amputated limb, but, from evidence given elsewhere, it is probable that the pigment-cells possess a local nervous apparatus, on which the occurrence and maintenance of post-mortem concentration depend, and the paralysis of which, while the pigment cells retain their powers, would give rise to diffusion in an amputated limb. Be this as it may, the fact that the state of full diffusion continued in the ring around the congested area in the last mustard experiment for hours after the irritant had been removed, although, during that time, complete concentration occurred in the web generally, is pretty clear evidence that the pigment-cells in that part had not merely been stimulated to increased action (for in that case they would have returned to their former condition soon after the stimulus had ceased to operate), but had suffered a loss of the faculty of concentration. Whether the loss of power resided in the nerves of the pigment-cells, or in those cells themselves, is a matter of indifference as regards the objects of the present inquiry; the important fact being that an action of the mustard so mild as to give rise to little or no derangement of the blood, nevertheless produced a certain degree of loss of power in the part on which it operated. There can be no doubt that the same principles apply to the cases of croton oil and cantharides; and thus the diffusion caused by these three irritants assumes a high interest, as visible evidence of diminished functional activity accompanying, if not preceding,

the earliest approaches to inflammatory congestion in parts which have been subjected to their influence.

With the view of ascertaining the nature of the effect produced on the pigment-cells by the mildest action of chloroform which is capable of causing inflammatory disorder, I ascertained, by repeated experiments, the shortest time in which the vapour of that liquid gave rise to unequivocal signs of a congestive tendency in the web of the living frog; and having found this to be about half a minute, suspended for that period one of the legs of a recently killed dark frog in a vessel, the bottom of which was covered with chloroform, having previously examined the webs microscopically, and found that full diffusion of pigment existed throughout them. The result was that the limb exposed to the chloroform vapour remained dark, while the other became gradually pale. On re-examination of the former after some hours, each web presented stripes of full diffusion of pigment alternating with others in a medium condition; their direction being at right-angles to the margin of the web. The longitudinal folds in which the webs had happened to be, had prevented the chloroform vapour from gaining equally free access to all parts; yet the chromatophorous cells in the stripes that had been thus partially protected from its influence had been incapable of complete concentration, showing that even the exceedingly slight degree of action which the chloroform could have exerted upon these places sufficed to diminish, though not to destroy, the functional activity of the pigmentary tissue.

In one of the experiments performed in order to determine the effect of mechanical violence, as before alluded to, the pigment remained unchanged for days in the area which had been pinched, while varying in other parts of the web; yet, though great excess of red corpuscles existed in the vessels of the affected spot, they never ceased to move; showing that the functions of the pigment-cells might be completely suspended by a degree of irritation short of that which occasions actual stagnation of the blood.

The same thing was afterwards seen in a case in which a small drop of wood-vinegar was placed upon one of the webs of a frog which had been deprived of the power of voluntarily moving the limbs by passing a knife between the occiput and the atlas, so as to sever the brain from the cord. The fluid being

thus allowed to lie quite undisturbed, did not spread at all upon the web, which was dry before it was applied. It produced its effects very slowly, so that, after the lapse of three and a half hours, the blood in the area covered by it, while everywhere presenting inflammatory appearances, was still only partially stagnant. Yet throughout this space the pigment retained exactly the same moderate degree of diffusion as it had at the beginning of the experiment, although in the interval complete concentration had taken place elsewhere; and a very striking contrast was presented between the stellate pigment with the adhesive though still moving blood-corpuscles, where the web was wet with the vinegar, and the dotted pigment and perfectly healthy circulation in the dry parts immediately adjacent.

Seeing, then, that complete suspension of the pigmentary functions may be caused by an amount of irritation which induces only a minor degree of congestion, and further, that (as we learn from the experiment with chloroform vapour) a still milder operation of an irritant renders these functions sluggish though not completely arresting them, we seem to have sufficient evidence that impairment of the functional cavity of the chromatophorous cells occurs in the very earliest stages of that primary change in the tissues which leads to inflammatory derangement of the blood.

It was seen in the ammonia experiment related above, that resolution having taken place in the congested area, the pigment-cells of the part recovered the faculty both of diffusion and concentration. This might have been pretty confidently predicted; for as congestion is a necessary consequence of the disorder produced in the tissues by irritants, we might have been almost sure that the return of the vital fluid to that healthy condition in which it is fit for free transmission through the vessels, must be preceded by a restoration of the living solids to their normal state. In the case alluded to, however, no sign of recovery of the pigment-cells appeared till after the circulation had become re-established; and even when several hours had elapsed, they still remained paralysed in the central part of the area on which the ammonia had acted. This is in harmony with the fact lately pointed out, that complete suspension of the pigmentary functions may accompany a state of the blood short of actual stagnation; and both appear to depend upon the circum-

stance that the chromatophorous cells are an extremely delicate form of tissue.

The rate of recovery of the pigment-cells varies greatly, however, in different cases, and in this respect much depends upon the nature of the irritant. An example of an agent of this class producing only very transient effects on the pigmentary functions is presented by carbonic acid. It has been before mentioned that the immersion of a living frog for about a quarter of an hour in water highly charged with that gas, gives rise to complete stagnation of the blood in the webs, although the heart still continues beating, but that resolution occurs after the animal has been exposed for a while to the atmosphere. With a view to ascertaining whether the congestion was due to the direct action of the acid upon the tissues, I made the following experiment. Having killed a dark frog and amputated both legs, and ascertained by microscopic examination that the pigment was fully diffused in the webs, I put one limb into a bottle of "aerated water" and the other into ordinary water: the latter soon became pale through post-mortem concentration, but the former remained as dark as ever during the two hours for which it was retained in the solution of carbonic acid, the direct action of which upon the bloodless tissues was thus demonstrated. An hour after the limb had been taken out, however, it was evidently recovering, being distinctly lighter in colour than it had been, and two hours later it was quite pale, and the pigment in the webs was found to be in almost the extreme degree of concentration. In subsequent similar experiments I left the leg in the aerated water for a longer time, during which it always retained precisely the same tint as it had when first introduced; and, if left for many hours, showed signs of loss of vitality, by the early supervention of cadaveric rigidity and exfoliation of the epidermis; but if it was taken out within about four hours, the pigment-cells recovered completely; and in one case a leg not removed for nine hours regained, nevertheless, to a considerable extent, the faculty of concentration. Thus it appears that carbonic acid, though exercising a powerful sedative influence upon the tissues, and paralysing for the time their vital energies, so as to give rise to intense inflammatory congestion, yet, even after a very protracted action, leaves them in a state susceptible of speedy recovery.

Here we see for the first time a satisfactory solution of the much-debated problem of the cause of congestion of the lungs in asphyxia; for there can, I conceive, be no doubt that the pulmonary tissues, exposed under ordinary circumstances to the influence of a free supply of oxygen, suffer, like those of the frog's web, from the vicinity of an abnormal proportion of carbonic acid, and inflammatory congestion is the necessary consequence. At the same time, the rapid recovery of the lungs from asphyxial congestion of considerable duration, when the normal atmosphere is readmitted, finds an equally close parallel in the speedy return both of the pigment-cells and the blood to the healthy condition when the foot of the frog is removed from the aerated water.

But the most important lesson to be learnt from these simple experiments with carbonic acid upon amputated limbs, is that the tissues possess, independently of the central organs of the nervous system, or of the circulation, or even of the presence of blood within the vessels, an intrinsic power of recovery from irritation, when it has not been carried beyond a certain point; a principle of fundamental importance, which has never before, so far as I am aware, been established or conjectured. It applies equally in the case of other irritants. Thus having transmitted for about a quarter of a minute, through one of the webs of a dark amputated limb, powerful galvanic currents, such as I had before ascertained to cause stagnation of the blood when operating for an instant upon the living animal, I found, after the lapse of an hour and a quarter, that the process of concentration had advanced considerably in the next web, but in that on which the galvanism had acted had only just commenced, even in the parts most remote from the point to which the poles of the battery were applied; while in the vicinity of that spot the state of full diffusion still continued. After the lapse of three more hours, however, the pigment was almost fully concentrated in the part of the web where it was before only slightly so; and even where it had been most directly subjected to the galvanic influence, it had undergone a certain, though very slight degree of the same change, the chromatophorous cells having even there partially recovered their functions.

This inherent power in the tissues of recovering from the effects of irritation, explains the occurrence of resolution in an

amputated limb, such as I once observed in a case where a moderate amount of congestion had been induced under the action of oil of turpentine before the animal was killed, and the blood resumed to a great extent its normal characters in the vessels several hours after the limb had been severed from the body.

The return of the blood along with the tissues to the state of health is a very interesting circumstance. Whether it depends upon an intrinsic power of recovery on the part of the vital fluid, or on the living solids resuming an active operation upon it, is at present uncertain; but in the meantime, the phenomena of resolution already assume a far more intelligible aspect than heretofore, on the hypothesis that the tissues generally are endowed with the same faculty of self-restoration as the pigment-cells.

It may be well to give here a list of all the agents whose effects upon the pigmentary functions I have investigated. They are as follows: Mechanical violence, the galvanic shock, desiccation of the tissues,[1] dry heat, warm water at 100° Fahr., intense cold, caustic ammonia, a strong solution of common salt, carbonic acid, acetic acid, tincture of iodine, chloroform, oil of turpentine, mustard, tincture of cantharides, and croton oil.

These are all of them irritants, i.e., give rise to inflammatory congestion through their direct action upon a vascular part, as I have witnessed in the frog's web in every case except that of cold, the influence of which in causing intense inflammation in the human subject is, however, familiar to all.[2] All of them

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[1] The effects of deficiency of moisture in the web were observed in amputated limbs, in which I have seen both suspension of pigmentary functions from this cause and recovery from that state after the application of water. While the circulation is going on in the living animal I have not found desiccation of the web to occur, unless the tissues had been weakened more or less by irritation.

[2] The only experiment which I made with cold was performed by introducing a test-tube, containing a dark amputated limb, into a freezing mixture of ice and salt at about 20° Fahr. for ten minutes. When the frozen limb had thawed, I ascertained, on microscopic examination, that the pigment had undergone a slight degree of post-mortem concentration, but five hours later it was still in much the same condition.

also afforded, in their effects upon the pigment-cells, ocular evidence of impairment of the functional activity of the tissues on which they act; and considering the number included in the list, and their great variety in essential nature, we need not hesitate to admit that similar effects are produced by the entire class of irritants.

There is another tissue in the frog's web which discharges functions apparent to the eye, viz., the arterial muscular fibre-cells, the contractions of which are readily recognized in consequence of the changes of calibre which they produce in the vessels; and the manner in which the arteries are affected in a congested part of the web indicates that the muscular, like the pigmentary tissue, has its functional activity impaired by a certain amount of irritation. Thus I have repeatedly been struck with the fact, and noted it before I knew its significance, that an artery running through a limited area on which an irritant has acted, remains dilated in the spot, although it may vary in other parts of its course. This I have observed in one experiment with mustard, in one with acetic acid, in two with ammonia, and in one with heat. In the last-mentioned case the appearance was particularly striking, from the circumstance that two arteries happened to pass through the burnt part, and were constricted to absolute closure in the rest of their course, contrasting strongly with their fully dilated state within the area.

In the ammonia experiments also the artery concerned was, in the progress of each case, seen to be completely constricted beyond the congested area, though still dilated within it. The limitation of this effect on the arteries, to the extent of the part acted on by the irritant, proves that it is the result of its direct action on the tissues; differing remarkably in this respect from the dilatation of the vessels, which is produced indirectly through the medium of the nervous system, and affects a wide space round about the spot irritated.

But with regard to both the muscular fibre-cells of the arteries and the pigment-cells, it may fairly be questioned whether the diminution of power to act resides in them or in those portions of their nerves which are situated in the irritated region. The view that the nerves are paralysed by irritants is consistent with the benumbing influence well known to be exerted upon the human skin or mucous membranes by some of those

agents, e.g., mechanical violence, the galvanic shock, cold, and chloroform. I have also observed, as before alluded to, that mustard produces a similar result on the cutaneous sensory nerves, and hence it seems probable that the same is true of the whole class of irritants. Again, the diffusion induced by mustard, croton oil, and cantharides indicates, according to what we have seen to be its most probable explanation, that the nerves of the pigment-cells suffer impairment of functional activity under the action of these three substances. On the other hand, the fact that diffusion is arrested equally with concentration by most irritants, appears to prove that the chromatophorous cells are themselves also affected with loss of power; for, as has been before alluded to, the withdrawal of nervous influence from them in a healthy state of the tissues invariably gives rise to diffusion, and the same result would necessarily follow the action of an irritant which merely paralysed the nerves. I have also observed, on two occasions, after the energetic operation of an irritant upon a part of a web containing a large artery,[1] that drawing the point of a needle firmly across the vessel failed to induce the slightest contraction in it, even at the very point crossed by the needle; proving that the muscular fibre-cells had lost their irritability. At the same time it is by no means improbable that the nerves of the arteries may suffer before their muscular constituents, just as in the intestines, after death, the functions of the intrinsic nervous apparatus are lost some time before muscular contractility ceases.[2]

[1] The main arteries lying between the layers of skin of which the web consists, are not so speedily acted upon by irritants as the capillaries of the dermis. This is most marked in large frogs with thick webs. In one such specimen, a drop of chloroform caused first stagnation and then discoloration from chemical action on the blood in the capillaries of the dorsal layer of the web to which it was applied, while a main artery lying beneath still contained blood of natural appearance, and showed evidence of languid contractility, while in the capillaries of the plantar layer of the web, the circulation was still going on in a pretty healthy state. This frog, however, seemed endowed with unusual powers of vitality in the tissues. This observation, as well as that in the text to which this note refers, was made subsequently to the reading of the paper.

[2] See the paper "On the Functions of the Visceral Nerves," before referred to (*Collected Papers*, vol. i, p. 87; *Proceedings of the Royal Society of London*, vol. ix, No. 32, 1858).

The question whether the suspension of function induced by irritants is confined to the nerves or affects the tissues generally, being one of great interest, I was anxious to obtain clear evidence regarding it; and it occurred to me that valuable information would probably be derived from observing the effects of such agents upon the action of the cilia, which, though not present in the web of the frog, exist in abundance upon the mucous surfaces of the mouth and œsophagus of that animal. Dr. Sharpey, in his celebrated article "Cilia" in the *Cyclopædia of Anatomy and Physiology,* mentions experiments made by Purkinje and Valentin, and also by himself, with a great variety of substances, including among the rest some irritants, which, when applied with sufficient energy, arrested completely, by their chemical action as it was supposed, the movements of the lashing filaments. It is evident, however, that in order to produce effects at all comparable to the state of the tissues of the frog's web in congestion, it would be necessary to adopt some more delicate method of experimenting, and the most eligible means for this purpose seemed to be to allow an irritating vapour to play upon a ciliated surface. Accordingly, on the 30th of November, 1857, having cut off a small piece of the tongue of a frog killed about an hour before, and placed it upon a slip of glass under the microscope, with just enough water to permit the free play of the cilia, I held near to it a piece of lint soaked in chloroform, keeping my eye over the microscope. The effect was instantaneous cessation of the previously rapid action of the cilia, which now stood out straight and motionless, like the hairs of a brush. I now immediately withdrew the lint, after which the same state of complete inaction continued for about half a minute, when languid movements began to show themselves, and after the lapse of five minutes more the ciliary action was going on pretty briskly in some parts, and ten minutes later seemed to have almost completely recovered.

Thus chloroform vapour produced in the ciliated epithelium-cells a condition precisely similar to that brought about in the pigment-cells by irritants applied so as to cause inflammatory congestion of the web, viz., a state of suspension or temporary deprivation of functional activity. And as the removal of the epithelium-cells from the surface on which they grow does not arrest the movements of their cilia, no mere paralysis of nerves

could account for this result, which necessarily implied that the epithelial tissue itself was affected with loss of power to discharge its accustomed functions. In repetitions of this procedure upon the same and other portions of the tongue, I did not generally get complete cessation of movement of all the cilia, but usually some retained a languid action, which improved after the chloroform had been removed. In one instance, however, the same perfect stoppage took place as in the first case, and the recovery was also very general, though the returning action was languid. Under these circumstances, a piece of lint dipped in strongest liquor ammoniae was brought within about 1½ inches of the object, and retained there for about fifty seconds, during which time the ciliary motion became progressively and greatly diminished, and within twenty-five seconds of the removal of the lint had ceased altogether. Some water was then added, so as to get rid of the absorbed alkali, when the cilia soon began to move again, and within about three minutes their play was more vigorous and general than before the ammonia was used, and three minutes later it was universal, as it was prior to the application of the chloroform. On another occasion, in a different animal, the cilia having been ascertained to be in rapid motion on a fresh piece of tongue, lint containing liquor ammoniae was held at a short distance from it for thirty-three seconds. The cilia very soon grew languid, and by the end of the time mentioned had quite ceased to act. The lint was at once withdrawn, but no recovery occurred; the operation of the irritant had been rather too energetic, and the vitality of the tissue had been destroyed. A languid state of the cilia was also produced by placing freshly prepared mustard near them, and improvement took place when it had been removed; but the essential oil itself, applied on lint like the chloroform and ammonia, though not acting so rapidly as might have been expected, permanently arrested the vibratile filaments. The vapour of strong acetic acid, if acting for four seconds, caused great diminution of the motion, and in another instance arrested it completely in five seconds. I did not, however, see any recovery from the effects of this agent, which produced obvious organic injury in the cells. The introduction of a portion of the mucous membrane of the mouth into a bottle of aerated water for about twenty minutes gave rise to permanent stoppage of

the cilia, and similar treatment for three or four hours caused disorganization of the epithelium, whereas the same period of immersion in ordinary water did not arrest the cilia. Powerful interrupted galvanic currents, transmitted for a few seconds through a particular spot in a piece of tongue on which the cilia were in free movement, abraded a portion of the epithelium there, and arrested completely the cilia of adjacent cells still *in situ,* and rendered those of other parts of the specimen extremely languid in their action. But the most satisfactory results were obtained from experiments with heat, which has the great advantage over chemical irritants, that it leaves no material behind it to act upon the delicate tissue. On the 14th of December, 1857, having ascertained that steeping a piece of tongue of a frog for five minutes in water of 110° Fahr., caused total and permanent cessation of ciliary action and desquamation of the epithelium, at 9h. 9m. p.m. I placed a portion of that organ, in which the vibratile movements were equable though rather languid, in water at 100° Fahr., and retained it there for a minute and a quarter, when it was transferred to cold water. On examining it after the lapse of nearly two minutes, I found the cilia acting decidedly more briskly than at first, but in the course of the next quarter of an hour they flagged very much, and in many parts ceased to move altogether. By this time I had fixed the specimen securely at the bottom of a glass trough, which I now suddenly filled up with water at 102° Fahr., and on first catching sight of the object, within a quarter of a minute of this procedure, found all the cilia absolutely motionless. I then at once drew off the warm water with a siphon previously arranged; and no sooner had this been done, than movements already began to show themselves in the cilia, and their action increased rapidly on my filling up the trough with cold water, and in a short time was all but universal and brisk, far superior to what it was before the hot water was put in. After a few minutes more, however, it was again very languid, and ceased entirely in many parts. I now, at 9h. 38m., filled up the trough with water at 104° Fahr. : at 9h. 38m. 17s. the cilia were almost all motionless; by 9h. 38m. 55s. the trough had been again emptied, but at 9h. 39m. 5s. there was even less movement seen. Cold water was again poured in at 9h. 39m. 35s., and after eighteen seconds, action was reappearing in the cilia, and it con-

tinued to increase during the next seven minutes, at the end of which time it was again almost universal. At 9h. 52m. the cold water was drawn off, and the same condition of the cilia having been ascertained to exist, the trough was, at 9h. 52m. 27s., filled up again with water at 104° Fahr.; eighteen seconds after this had been done, the ciliary action was found much diminished, but had not fully ceased; and after nine seconds more, during which the warm water was drawn off, the cilia were still acting very slightly. Within twenty-three seconds of this time the trough was again filled with cold water: now, however, the epithelium was in many parts beginning to exfoliate, swelling up by endosmose in obedience to the ordinary laws of chemical affinity, and so indicating that it was losing its vitality. I also lost sight of the precise spot which I had been observing, but noticed that ciliary action was again going on pretty quickly in some places. There can be no doubt, although there was no opportunity for observing the fact, that the first immersion in hot water caused cessation of the ciliary action; and that being admitted, we have in this case suspension of function and recovery four times repeated in the same fragment of tissue in consequence of as many applications and withdrawals of the irritant. It is a curious circumstance that each recovery, except the final one, brought up the action of the cilia for a time to a better state than they had just before the last introduction of warm water. But the discussion of this and other circumstances in this case will be best reserved till after the mention of another set of experiments.

In order to eliminate the nerves completely from among the causes both of the suspension of function produced by irritants and the recovery from that state, it seemed desirable, if possible, to observe those occurrences in detached epithelium-cells, and on the 22nd of January, 1859 [*sic*], I made the attempt to do so. At first, however, it proved more difficult than I had anticipated. It was of course easy to obtain the material to operate on, by gently scraping the surface of the palate of a recently killed frog with a knife, and placing the mucus-like product on a plate of glass with a drop of water. But the tissue thus separated from its connections was in an exceedingly delicate condition, and any agent used for arresting the action of the cilia was very apt to destroy at the same time the vitality of the cells. Thus when

the object was warmed by placing the glass plate on a piece of iron at about 100° Fahr. for half a minute, the vibratory movements were arrested, but never recovered, and in a short time the cells swelled up by endosmose. It appeared probable that the tissue had suffered during the time required for the cooling of the glass: and in order to avoid this, and also prevent the object-glass becoming obscured by vapour from the warm water condensing upon it, the epithelium was placed between two slips of thin covering glass, kept from too close approximation by fragments of the same material interposed, the whole forming a layer so thin that it would be rapidly heated if any hot body were placed in its vicinity, and cooled as quickly on its removal. A small cautery iron, with a bulbous extremity about as big as a hazel-nut, just too hot to bear in contact with the finger, was now put behind the stage of the microscope, within about three-quarters of an inch of the object, the diaphragm plate having been removed to afford room for this being done without interfering with the light sent up by the reflector. The result, which I watched from the first, was the same that I had once before observed from the very gentle application of heat to a portion of a frog's tongue, viz., primary increase in the action of the cilia which had previously been languid,[1] but which, within ten seconds of the approximation of the cautery, were moving with great rapidity, and continued to do so for about twenty-five seconds, at the expiration of which their motion was seen to be diminishing, and after another minute and a half it was considerably more languid than at the beginning of the experiment. The cautery being now removed was found to be much cooled though still warm, and its withdrawal did not affect the cilia, which still remained much in the same state after the lapse of eight minutes. I now repeated the experiment upon a fresh portion of epithelium, but this time used the cautery red hot,

[1] Professor Weber, of Leipsig, observed several years ago that the action of cilia upon epithelium-cells removed from the human nostril was increased by gentle warmth, but retarded by cold. In that case the elevated temperature was natural to the tissue, and might be supposed to operate by restoring it to its normal conditions. In the cold-blooded reptile, however, the accelerated movement under the influence of heat has, of course, a very different significance.

placing it about two inches behind the object: no sooner had this been done than the action of the cilia became excessively increased, but this did not continue for more than five seconds, when they become perfectly motionless. The hot iron was now at once withdrawn, but the cilia under special observation did not recover. In other situations in the same specimen, however, movements were observed in the course of the following minute, and it was still continuing seven minutes later, when a part having been brought into the field where there were two considerable groups of cells in moderate activity, the cautery was again applied at a distance of about 2½ inches. The motions of the cilia immediately became distinctly increased, but, as in the former case, this condition gave place in five seconds to universal quiescence. The iron was then removed, and on re-examination after three minutes, the cilia were again moving, though in a somewhat languid manner in both parts of the field. For the sake of confirmation I again operated in a similar manner upon another specimen, on which I performed no less than five successive experiments with similar results in all. In the first three of these trials I had the very same cilia under observation, and saw them time after time become first increased in action and then arrested under the influence of the cautery, and gradually recover after its removal. In some instances the times of cessation and of recovery were noted as follows:—In the first the cilia were arrested in two seconds after the application of the hot iron, but the exact time of recovery was not observed; in the second, cessation of movement was produced in two seconds, and return began in fifteen seconds; in the third, cessation was in fifteen seconds, and recovery also in fifteen seconds; in the fourth, the times were not noted; in the fifth, movement ceased in about two seconds and returned in twenty. The experiments were performed within about five minutes of each other, or something less. It is also to be remarked, that there were some slight differences in the degree of heat of the cautery and its vicinity to the object.

These experiments are as instructive as they are simple and easy of performance. They show conclusively that a component tissue of the animal frame may, independently of the nervous system, have its actions either excited or paralysed by the direct operation of an irritant upon it, and that it may possess an

equally independent power of recovery. Also in the accelerated movements of the cilia elicited by very gentle heat, as compared with the cessation of their vibrations under a higher temperature, we have a striking confirmation of the view which I had taken of the relaxation of the arteries and hollow viscera in consequence of nervous irritation.[1] For the law which we thus see regulating the effects of heat upon the epithelium-cells is precisely that which I had inferred must govern the action of afferent nerves upon nerve-cells; this law being, that an agency which, when operating mildly, stimulates a tissue to increased activity, may, when more energetic, temporarily arrest its functions. Whether or not the converse always holds, viz., that any agent which is capable of suspending the functional activity of a tissue may also excite it if applied with sufficient gentleness, or, in other words, whether irritants are in all cases also stimulants, seems very doubtful. As regards the nerves, such does appear to be the case; for while many, and probably all influences which induce inflammatory congestion cause temporary paralysis of sensation in parts on which they act severely enough, they all stimulate the afferent nerves in the first instance, as is shown by the reflex changes in the calibre of the arteries which occur round about any irritated spot. The nervous centres, too, present an illustration of the same principle, not only in the effects produced upon them by the nerves, as lately alluded to, but also in the excitement well known to be occasioned by small doses of many sedative narcotics, such as alcohol, opium, and chloroform, which may be regarded as special irritants of the nervous centres. In the case of the cilia, I have not observed primary increase of movement to be induced by any agent besides heat; but I am not prepared to say that it might not by careful management be made to occur with some other irritants.

The pigment-cells in the common frog give very little indication of the stimulating properties of irritants, as is evident from several of the experiments which have been recorded in this section. In the tree-frog, however, as we are informed by the German authorities, a part of the integument subjected to such influences rapidly assumes a pale tint, and that even in a portion of skin removed from the body. I have also several

[1] In Section III of this paper.

times noticed, after pinching the web of a common frog, that, although in the spot actually squeezed, the pigment-cells were deprived of their power of changing, a pale ring about one-sixteenth of an inch in breadth has gradually formed in its immediate vicinity in the course of the next hour; whence it seems probable that direct irritation tends to excite concentration in the English species as well as in the continental, but that in the former the effect is developed much more slowly, so that it is apt to pass unnoticed. I further, on one occasion, saw post-mortem concentration greatly accelerated by heat. It is doubtful, however, whether these results are due to direct action upon the pigment-cells; for in the tree-frog, as well as in the English kind, the pale tint was not confined to the precise spot operated on, but affected a limited area of surrounding tissue; whence it seems likely that it is developed through the medium of a local nervous apparatus contained in the skin.[1] If this be true, we have no proof that the pigment-cells are capable of being stimulated except by nervous influence, although they are, as we have seen, peculiarly susceptible of suspension of function through the direct operation of irritants upon them.

With regard to the nature of the change experienced by the tissues when temporarily deprived of power by irritants, the primary increase of motion of the cilia, lapsing into quiescence, under the operation of heat, may suggest the idea of exertion followed by exhaustion. But that the state of incapacity is not dependent on previous action, seems clear from the fact that in the pigment-cells it is maintained and aggravated by an irritant continuing in operation after complete suspension of function has been induced, the same kind of effect being still produced upon the tissues which are unable to act as upon healthy parts. As an illustration of this, I may revert to the results of immersion of an amputated limb in aerated water. The carbonic acid, as we have seen, entirely prevents the occurrence of post-mortem concentration, implying that the powers of the chromatophorous cells are completely suspended by it within a few minutes at most of its first acting on the part; yet, however long the tissues thus paralysed are kept subjected to its influence, they remain

[1] See the paper "On the Cutaneous Pigmentary System," before referred to.

without any sign of action. They will, however, recover speedily and completely if soon taken out and exposed to the air, so that the irritating gas may be dissipated; whereas if retained for several hours in the aerated water, they may, indeed, have their powers restored to a certain extent on removal from it, but exhibit only very feeble action. Such facts as these prove conclusively that the tissues may have their functional activity impaired without loss of vitality by the direct action of irritants, independently of any stimulating effects which may be at first produced by them; and also that the influence thus exerted is of an injurious tendency.

The imperceptible transition from suspension of function to loss of vitality displayed by the long-continued operation of carbonic acid upon the pigment-cells is also well illustrated by some of the experiments upon the cilia, especially those with heat and ammonia, which, unless employed with extreme caution, not only permanently arrested the vibratile filaments, but reduced the epithelium-cells to a condition in which they were amenable to the ordinary laws of chemical affinity. All irritants appear to be agents which, if operating with sufficient energy, completely destroy vitality, probably by inducing, through chemical or physical action, an irreparable derangement of the molecular constitution of the tissues. Their essential property, however, is that of causing, when applied somewhat more mildly, a minor degree of disturbance or disorder in the component textures of the body, which are rendered for the time being unfit for discharging their wonted functions,[1] though afterwards, by virtue of their innate powers, capable of spontaneous recovery, the rapidity and completeness of which bears an inverse ratio to the intensity and duration of the previous irritation. Lastly, these same noxious agents, if in a still more gentle form, operate, upon some of the tissues at least, as stimulants, rousing them to increased exertion of their vital functions. How this effect is brought about must, I believe, be only matter of uncertain speculation, so long as the real nature of life in the animal frame

[1] The word "irritant" is etymologically ill-adapted to express the possession of this property; but as it is universally employed in professional nomenclature, it is perhaps best to continue to use it.

remains, as it probably ever will remain to our finite capacities, an impenetrable mystery.

It is an interesting circumstance that, in the experiments with warm water, the cilia, after recovering from the state of quiescence, moved for a while more briskly than they did immediately before the application was made. This increased action cannot be attributed, like the primary acceleration resulting from very gentle warmth, to a mild operation of the irritant; for the epithelium-cells must have been completely cooled down before it commenced. It must therefore be regarded as a true reaction on the part of the tissue, whether dependent on accumulation of vital energy during the period of suspended function, or excited, as by an irritant, by the state of disorder which the warm water had induced, seems uncertain.

Considering the number and variety of the functions which direct observation has shown to be suspended by irritants, viz., pigmentary concentration and diffusion, ciliary motion and nervous action, it appears probable that all the vital processes are liable to similar temporary arrest.

Different tissues, however, seem to differ in the facility with which they are affected by irritants. The pigment-cells are very susceptible to their influence as is indicated by the complete paralysis which we have seen to be produced in them by agencies that give rise to only a minor degree of inflammatory congestion; and also by the circumstance which I have often observed in the web of the frog, that, as in the choroid coat of the human eye, they become absorbed in parts which have been injured, having been deprived of vitality by causes which inflicted on other textures only a recoverable lesion. The epithelium-cells, too, are very sensitive to irritation, exhibiting its results more rapidly than can be accounted for merely by their exposed situation. In those which invest the mucous membrane of the mouth, the cilia with which they are provided furnish the opportunity of which we have availed ourselves, of observing the state of suspension of function in consequence of very gentle treatment; and though the epidermis does not admit of this, it shows the further stage of loss of vitality by exfoliating after an amount of injury from which the immediately subjacent tissues readily recover. John Hunter was unquestionably correct in the opinion that the elevation of the cuticle in vesication depends not only

on the effusion of serum beneath it, but on a primary separation arising "from a degree of weakness approaching to a kind of death in the connection between the cuticle and cutis."[1] For I find that in an amputated limb free from blood, although no effusion of serum can occur, the epidermis becomes speedily loosened in a part to which an irritant is applied, as for example, in a web treated with oil of turpentine, whereas it remains elsewhere firmly attached for days if the weather be cool.

The temporary abolition of the normal relations between the blood and the tissues in inflammatory congestion,[2] must be added to the list of instances of suspension of vital properties by irritation. The tissues the healthy state of which seems most likely to be essential to that of the vital fluid, are those contiguous to it, viz., the walls of the blood-vessels; and that these are really deprived of their vital endowments during inflammation seems implied by the character of the material which is transmitted through them in that condition. For we have seen that the vascular parietes differ, in the state of health, from all ordinary solids in being destitute of any attraction for the fibrine, if not positively repelling it,[3] and that this is probably the cause of the merely serous character of the effusion which takes place in mechanical dropsy depending upon abnormal pressure of the blood within healthy vessels. On the other hand, the exudation of the liquor sanguinis in its integrity, such as occurs in severe inflammation, cannot, I think, be satisfactorily explained by the mere abnormal pressure of the blood produced by dilatation of the arteries and concomitant obstruction in the capillaries; but seems naturally accounted for on the hypothesis that the walls of the vessels, like other tissues, lose, for the time, in inflammation, their vital properties, and, acquiring an attraction for the fibrine like that exercised by ordinary solids, permit it to pass without opposition through their porous parietes.

It may be well to present a brief summary of the principal results arrived at in the present section.

It appears that the various physical and chemical agents which, when operating powerfully, extinguish the life of the

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[1] *Works of John Hunter*, Palmer's edition, vol. iii, p. 349.
[2] See close of Section III of this paper.
[3] See Section III of this paper.

constituents of the animal body, produce by a somewhat gentler action a condition bordering upon loss of vitality, but quite distinct from it, in which the tissues are, for the time being, incapacitated for discharging their wonted offices, though retaining the faculty of returning afterwards, by virtue of their own inherent powers, to their former state of activity, provided the irritation has not been too severe or protracted. This suspension of function or temporary abolition of vital energy is the primary lesion in inflammatory congestion; the blood in the vicinity of the disabled tissues assuming the same character as when in contact with ordinary solid matter, and thus becoming unfit for transmission through the vessels; while the return of the living solids to their usual active state is accompanied by a restoration of the vital fluid to the healthy characters which adapt it for circulation.

Conclusion.

It remains to glance at the application of the principles established in the preceding pages to human pathology.

The post-mortem appearance which is universally admitted to indicate that the early stages of inflammation have occurred during life, is intense redness, depending essentially not upon peculiar distension of the vessels with blood, but upon abnormal accumulation of the red corpuscles in their minutest ramifications. A beautiful example of this condition, developed idiopathically, was presented by the case of incipient meningitis mentioned in the Introduction, in which the vessels of an affected spot of pia mater were filled with a crimson mass of confusedly compacted corpuscles, exactly as in an area of the frog's web to which mustard has been applied. The derangement of the vital fluid in the human subject being thus closely parallel to that which we have studied in the batrachian reptile, we can hardly doubt that in the former, as in the latter, the living solids are in a state of more or less complete suspension of functional activity during inflammatory congestion. This view is supported by the effusion of liquor sanguinis in its integrity in the more advanced stages of the disease in man, and by the speedy coagulation of fibrine upon inflamed serous surfaces, or in the interior of vessels affected with arteritis or phlebitis. For these circumstances, as has been before remarked, appear to indicate that the

tissues are for the time being reduced still more towards the condition of ordinary solid matter. These arguments, derived from the appearances of the blood, are further corroborated by the immediate transition which is apt to occur from intense human inflammation to gangrene.[1]

But a comprehensive and complete account of the inflammatory process must embrace not merely the state to which the tissues are brought when the disease is fairly established, but also the causes which lead to it.

Inflammation is sometimes brought about in man in a way strictly analogous to that in which we induce it in the web of the frog's foot, viz., by the immediate operation of some noxious agent from without, as when boiling water is poured upon the skin. One peculiarity connected with such cases, as compared with those which are of idiopathic origin, is the great rapidity with which the various stages of the disorder are often developed. This, however, is the natural consequence of the direct manner in which the prejudicial influence is exerted upon the tissues; the inflammatory phenomena supervening more speedily in proportion to the energy of the irritant. This principle is well illustrated by the effects of mechanical violence upon the human integument. A moderate degree of pressure applied continuously gives rise, during the first few hours, to nothing more than inflammatory congestion, which disappears soon after the pressure has been removed, as seen in the red mark produced upon the forehead by a tightly-fitting hat. But if such treatment be continued for a considerably longer period, vesication will result, as when apparatus employed for the treatment of fractures is applied too firmly for many hours together over a bony prominence. The same effect which is thus slowly developed under a moderate degree of mechanical irritation, may, however, be induced very rapidly through the same agency in a more intense form, as is shown by the bullae which are often observed in surgical practice very soon after the infliction of a severe contusion. Here the source of irritation being no longer in operation, there is no blush of redness in the vicinity dependent upon

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[1] The degenerations of tissue which result from inflammation, ably delineated by Mr. Paget in his *Lectures on Surgical Pathology,* are additional evidence in the same direction.

arterial dilatation, and hence such cases are often supposed to have nothing in common with inflammation; and I have known these vesicles mistaken for evidence of gangrene, so as to lead to unnecessary amputation. The suddenness with which inflammatory oedema arises in consequence of the bites or stings of venomous reptiles is explicable on the same principle. The poison appears to diffuse itself among the tissues so as to operate directly upon them, and when extremely virulent, kills them outright; but when less potent, produces merely a temporary though rapid prostration of their vital energies with consequent inflammatory effusion. Again, the congestion of the lungs, which comes on so quickly in asphyxia,[1] has been before alluded to as probably the result of the sedative influence which, from experiments upon the frog, we are led to believe must be produced upon the pulmonary tissue by the abnormal amount of carbonic acid in the air-cells.

In the class of cases hitherto considered, the derangement of the part, and the causes which lead to it, being both, to a considerable extent, understood, the disease may, I think, be regarded as in so far satisfactorily explained. But one important lesson taught by the results of this investigation is, that it is necessary to draw a broad line of demarcation between inflammation produced by direct irritation, and that which is developed indirectly through the medium of the nervous system, whether in the immediate vicinity of a source of irritation, as around a tight stitch in the skin, or a thorn in the finger, or at a distance from the disturbing cause, as when the kidneys are affected in consequence of the passing of a bougie, or the lungs through exposure of the feet to cold. Nothing can better illustrate the importance of this distinction, than what takes place in a recent wound. In consequence of the injury inflicted by the knife, together with the subsequent manipulation and exposure, the tissues, in a thin layer at the cut surface, are thrown into that condition which leads to effusion of liquor sanguinis, the fibrine of which, speedily coagulating, remains to constitute the bond of primary union, while the serum trickling away between the lips of the wound produces the discharge which soaks the dressing during the first twenty-four hours. But neither during the

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[1] See p. 45.

exudation of the lymph in such a case, nor during its subsequent organization, is there necessarily any inflammation induced in the lips of the wound through the nervous system; and if this complication does occur, it interferes with the healing process in a degree proportioned to its intensity. In other words, while a certain amount of inflammation as caused by direct irritation is essential to primary union, any degree of it as induced indirectly is both unnecessary and injurious.

The question how inflammation is developed through the medium of the nervous system, possesses a high degree of interest, in consequence of its bearing upon the manner in which counter-irritation operates therapeutically. In the integument, where we have the opportunity of seeing the affected part, the first indication of the supervention of inflammatory disorder around a centre of irritation is a blush of redness, which, as before shown,[1] consists, in the first instance, of mere dilatation of the arteries with rapid flow of blood through the capillaries. It is quite conceivable that arterial dilatation, carried to an extreme degree along with powerful action of the heart,[2] may so increase the tension upon the tissues as to impair their powers gradually by mechanical irritation, just as the frontal integument is affected by long-continued gentle pressure from without, as above alluded to; for we know that when inflammation does exist, mere increase of tension upon the blood in the vessels will greatly aggravate the disorder, as when an inflamed foot is kept in a dependent posture. Supposing this to be the whole mechanism of the disease, its origin would be sufficiently intelligible; for we have seen that vascular dilatation caused by irritation operating through the medium of the nervous system appears to depend on a depressing influence produced by excessive action of the afferent nerves upon the ganglia which preside over the arterial contractions. There are, however, some circumstances, such as the dryness of the nostril which may exist in the early stages of coryza, and sudden suppression of urine in

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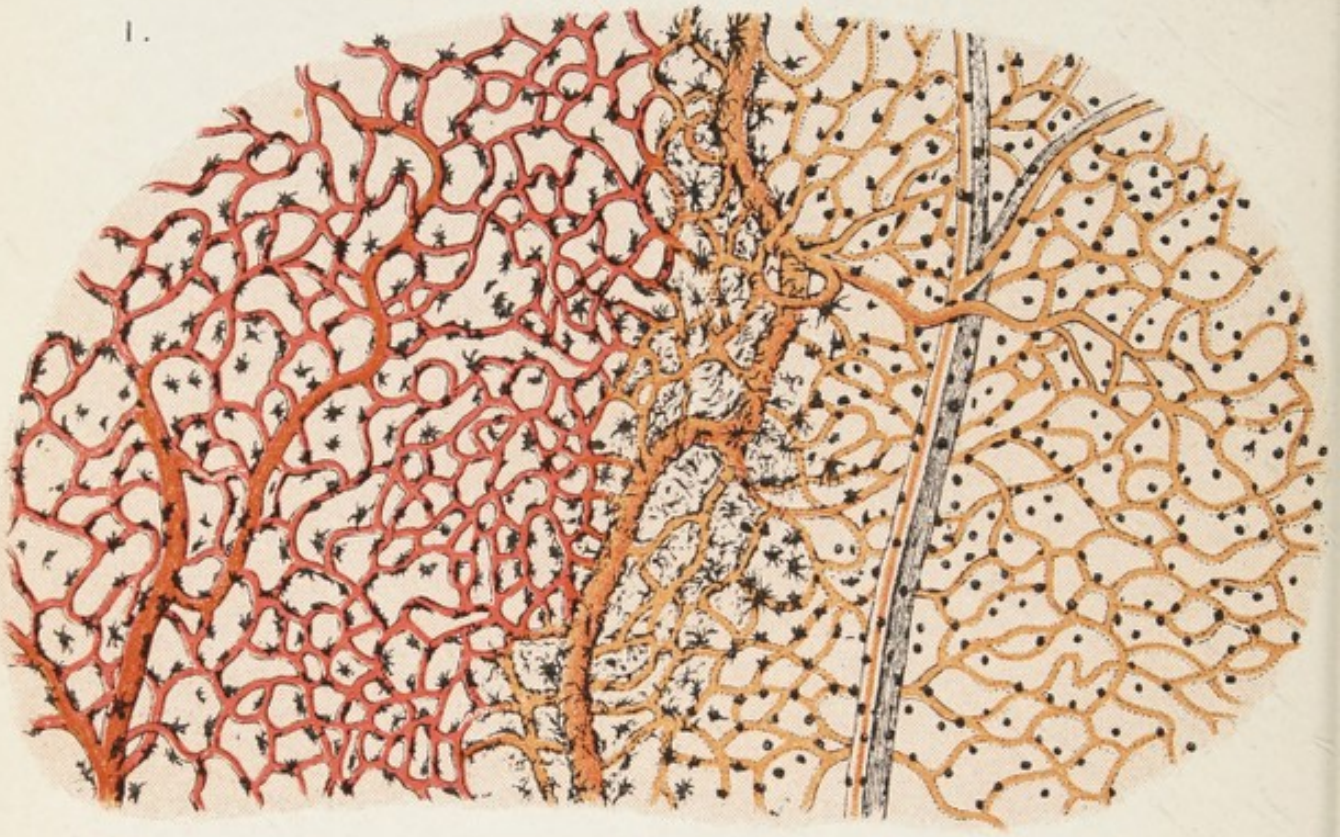
[1] See Section III of this paper.

[2] It is to be observed, that in the frog, when full dilatation of the arteries lasts for days together without the production of inflammatory congestion, the state of the vessels has been brought about by a very serious operation which greatly weakens the action of the heart.

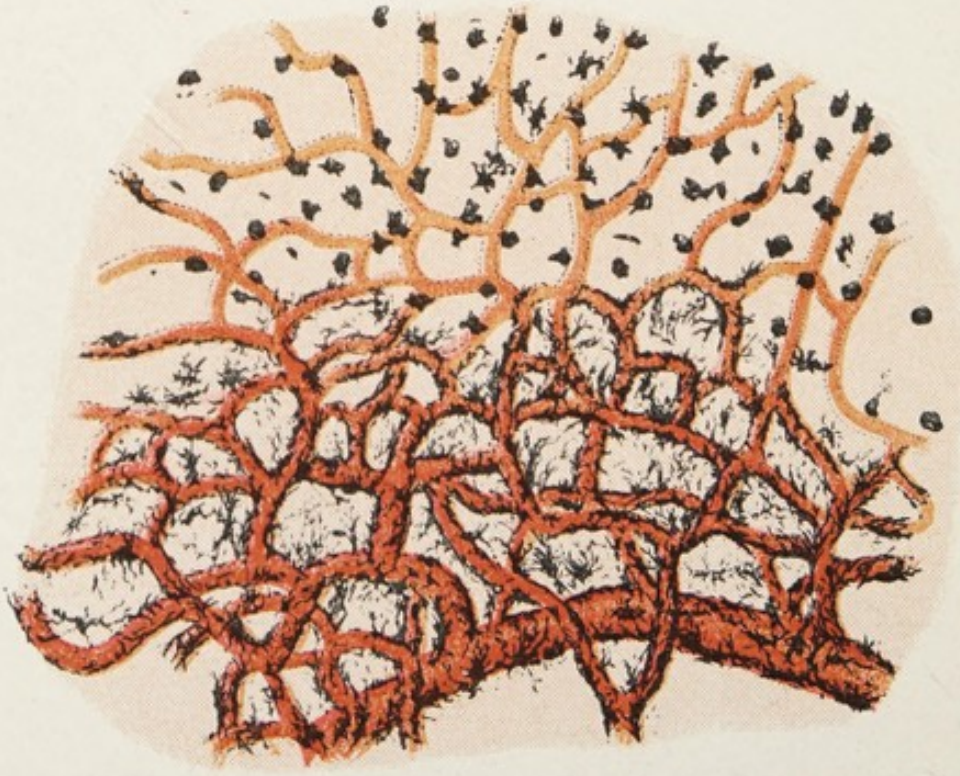
consequence of urethral irritation in cases where renal congestion becomes ultimately established, which seem to indicate that other functions as well as arterial contraction may be primarily arrested by nervous agency in the early stages of inflammation. The study of the pigmentary system of the frog has afforded proof that other tissues besides muscular fibre are under the control of the nerves, and it seems not unlikely that gland-cells or other forms of tissue may, like nerve-cells, be reduced to a state of inactivity by excessive nervous action; and thus we seem to have a clue to comprehending what at first appears anomalous, that prostration of the vital energies of the part actually inflamed may be brought about by unusually great activity in the parts of the nervous system specially concerned with it. This, however, is a wide subject, which requires further investigation. But in the meantime we may, I think, consider as satisfactorily established the fundamental principle, that whenever inflammatory congestion, or, in other words, that disturbance of the circulation which is truly characteristic of inflammation, exists in any degree, the tissues of the affected part have experienced to a proportionate extent a temporary impairment of functional activity or vital energy.

PLATE IV.

1.



2



EFFECTS OF IRRITANTS ON PIGMENTARY TISSUE.

Lister, del. Tuffin West, sculp. Bale & Danielsson, Ltd lith.

PLATE IV

represents the effects produced by irritants upon the pigmentary tissue and the blood-vessels of the frog's foot.

FIG. 1 shows the results of the application of mustard to the web. The pigment was at first in the stellate condition as on the left-hand side of the sketch, and it remained permanently in that state in the part on which the mustard lay, while at the same time intense inflammatory congestion was produced there, indicated by the deep red colour of the vessels. Just beyond the edge of the mustard the irritating vapour of the volatile oil gave rise to full diffusion of the pigment (an effect peculiar to mustard and a few other irritants when acting mildly), but without material inflammatory disorder of the blood; as seen in the stripe down the middle of the drawing. During the progress of the case the animal changed to a pale colour in the body generally, assuming the dotted aspect depicted on the right-hand side of the sketch in all parts which had not been acted on by mustard, and thus deprived for the time being of the power of concentration. It will be observed that the blood-vessels of the healthy part are not materially smaller than those of the congested region; the deep colour of the latter being due to their containing closely-packed red corpuscles, while the former are pale in consequence of the blood within them having the normal proportion of colourless liquor sanguinis.

FIG. 2 illustrates the effects of mechanical violence. The lower half of the sketch represents parts of an area in one of the webs of a dark frog, which was pinched with a pair of padded forceps so as to give rise to inflammatory congestion. The animal afterwards grew much paler, so that in healthy parts the pigment assumed the angular or slightly stellate appearance shown in the upper part of the drawing. But on the particular spot on which the mechanical violence had operated, the chromatophorous cells being incapable of discharging their usual functions, the pigment remained in the fully diffused state in which it was at the commencement of the experiment.

AUTOGRAPH LETTER FROM LISTER TO HIS FATHER REFERRING TO THE ADMINISTRATION OF CHLOROFORM.

17 Woodside Place

22nd Sept/61

My dear Father

The case which has been detaining us has been going on favourably, though not yet quite in a condition to leave. But we hope to get away tomorrow evening or Thursday morning. If all continues to go on as at present.

The delay seems to have had good results as far as my work is concerned.

In Saturday made
some observations which
seem of much importance
with respect to the
administration (
Christopher; which observations
I should probably not
have been able to make
had I been at Upton:
I fear as I can see,
Emma, all seems over
dear for our visit to
you, though it cannot be
altogether out of Emma
for me, as I must be
getting on with my
writing if I write well.

The observations were made by means of an instrument recently invented, by which the vocal cords of the larynx (the vocal apparatus) can be inspected with great ease considering their apparently inaccessible position.

But I must defer any detailed account of the subject till we meet — which I hope will now be very soon.

Meanwhile Agnes joins in dearest love to you all with thy ever affect.te son

Joseph Lister

ON ANAESTHETICS.

Collected Papers, vol. i, p. 135—Holmes's *System of Surgery*, vol. iii, third edition, 1883.

The following paper on Anaesthetics is one of two which Lister contributed to the first edition of Holmes's *System of Surgery* in 1861. The other was on Amputation. These articles show that practical surgery was then chiefly occupying his thoughts, but he could only apply it in private practice, for, though he had been Professor of Surgery more than a year, he was not yet on the staff of the Royal Infirmary. He was in his thirty-fifth year and very fully occupied with his class of over 170 students, probably the largest in Great Britain. Holmes's "System" was something new in surgical literature. Nearly all the contributors were Londoners. It had a great vogue and passed through three editions. Only the article as it appeared in 1861 is given here; not the short addition of 1871, nor the longer one of 1883. All three portions are printed in the *Collected Papers;* as the original article and the addition of 1870 are unaltered, a good idea may thus be obtained of the way in which the views of the profession were modified during the first period of ten years and the second of over twenty.

The reader will find no complicated account of apparatus, but a reasoned statement of the physiological and pathological effects of chloroform and ether on the heart and respiration. He must bear in mind that anaesthesia had only been discovered fifteen years before the article was written, and also that Lister was

a staunch supporter of chloroform and the Edinburgh method of administering it. Even to the end of his professional life he appeared to give only a grudging acknowledgment of a few of the proved merits of ether; to its defects he was perhaps more fully alive than most of his contemporaries. At different times he made many clinical observations, physical experiments and practical tests upon himself, and even sometimes upon Lady Lister. The method of administration which was the outcome of these was a model of safety and simplicity (*Collected Papers*, vol. i, p. 168).

In the second edition he reiterates his former statements, protests against special anaesthetists ("chloroform givers"), and makes further remarks about the falling back of the tongue and idiosyncrasy.

By the time the third edition appeared the "close method" of administering ether had been introduced, a committee of the British Medical Association had issued a misleading report, and Paul Bert had expressed some "startling views" about dosage. The greater part of the matter added in 1883 consists of a searching criticism of these three occurrences. He also explains why he abandoned the wasteful, though "safe" Edinburgh plan of giving chloroform in favour of the simple and economical method he then employed. This was suggested to him by "Skinner's mask," which was a substitute for and an improvement on "Junker's inhaler." Near the end is a dissertation on the causes and treatment of collapse.

This paper illustrates the philosophical way in which Lister approached subjects which might have been treated from the purely technical point of view.

ON ANAESTHETICS.

PART I—WRITTEN 1861. *Collected Papers,* vol. i, p. 135.

To prevent or diminish pain in surgical operations is an object so desirable, that many in various ages in the history of Medicine have sought to attain it, either by means of narcotic drugs designed to act on the body generally, or by compressing or otherwise locally affecting the nerves of the part concerned.

The first really valuable suggestion, however, was made in the year 1800 by Sir Humphry Davy, who, having himself experienced relief from pain when breathing nitrous oxide gas, threw out the hint that it might probably be employed with advantage to produce a similar effect in surgical practice.

The same idea occurred, after the lapse of nearly half a century, to Dr. Horace Wells, a dentist in Hartford, Connecticut, who, in 1844, underwent the extraction of a tooth without pain after inhaling the gas, and gave it with satisfactory results to several of his patients; but he soon after found the practice so uncertain that he abandoned it entirely.

About the same period Dr. W. T. G. Morton, of Boston, in America, who had previously been a partner with Wells, but did not, as he informs us, receive any suggestion from him, became possessed with the desire of discovering an efficient anaesthetic, and commenced a series of experiments upon himself and the lower animals, which at last resulted in his extracting a tooth painlessly from a patient to whom he had administered the vapour of sulphuric ether by inhalation. This was on September 30, 1846. Soon afterwards he publicly exhibited his method at the Massachusetts General Hospital; and thenceforward anaesthesia in surgery was an established blessing to mankind.

Sulphuric ether is still extensively used as an anaesthetic in America, but in Europe chloroform is generally preferred to it. Disguised under the name "chloric ether," in which it

exists diluted with spirit of wine, this agent was the subject of Dr. Morton's first experiment upon himself; and it was used in the same form at St. Bartholomew's Hospital, in preference to sulphuric ether, by Mr. Lawrence in the summer of 1847. In the autumn of that year Dr. (afterwards Sir James Y.) Simpson, who was engaged in a series of experiments with various narcotic vapours, employed for the first time the active principle of chloric ether, at the suggestion of Mr. Waldie, of the Apothecaries' Hall of Liverpool; and finding that the pure chloroform was more potent than sulphuric ether, yet caused less bronchial irritation, while its odour was more agreeable, and its inferior volatility rendered its exhibition more easy,[1] he zealously recommended it to the profession, and it has since been generally employed throughout Europe.

The effects produced by chloroform are such as to fit it remarkably for the purposes of the surgeon. Like most narcotics, it tends to cause, after temporary excitement, suspension of the functions of the nervous centres, but affects them not simultaneously, but in a certain order; and the brain is the first to show loss of power in failure of sensation and voluntary motion. If this were all, anaesthesia would be a questionable boon, as the work of the surgeon would be interrupted and often marred by involuntary struggles on the part of the patient. But very soon the spinal cord also is subdued, and the reflex functions of the cerebro-spinal axis are abolished so far as concerns the voluntary muscles, which consequently lie perfectly relaxed and passive, better suited for operative purposes than the most resolute will could render them. To this, however, there is one remarkable exception, viz., that the parts concerned in the respiratory movements remain active; and the same is the case with the sympathetic ganglia of the heart. In other words, when the administration of chloroform is carried to a certain point, the nervous system is deprived of such powers as would cause pain to the patient or inconvenience to the surgeon, but retains intact the faculties essential to life.

There are, however, yet other advantages derived from the

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[1] For operations performed by artificial light, chloroform has another advantage over ether, in the fact that its vapour is not inflammable.

inactivity of the cerebro-spinal centre. It seems now clearly established that the cessation of the contractions of the heart in the shock of injury depends upon an action of the brain and cord upon the cardiac ganglia through the medium of the vagus and sympathetic nerves; and chloroform, rendering this action impossible, protects the heart from the indirect effect of external violence. In this way it has greatly diminished the risk of death upon the operating table, and also has overthrown the old rule of deferring amputation in cases of injury till the patient has recovered from the state of collapse, thus shortening the period of mischief to the system from the presence of the mangled limb, and in extreme cases sometimes saving life where it would be hopeless to wait for returning consciousness. Indeed, an amputation performed under chloroform has often the effect of improving instead of lowering the pulse.

The most striking instance of this that has fallen under my notice occurred in a labourer, whose right arm and thigh had been destroyed by a railway accident, just enough sound tissue being left to admit of amputation through the hip and shoulder joints, which was accordingly performed as a forlorn hope by the surgeon in charge of the case. The vital powers being in a state of extreme depression, it is probable that without chloroform this severe measure would have killed him outright, but by help of the anaesthetic it was followed by marked improvement of the pulse, which continued for some hours, so as to lead us to entertain hopes of his recovery.

Faintness during the operation, a species of shock, is also got rid of by chloroform; and this, besides its obvious convenience, has the advantage of lessening the chance of secondary hæmorrhage; for the vessels which require ligature declare themselves as such by bleeding, instead of deceptively eluding observation in consequence of the feebleness of the heart and the general arterial contraction which co-exist in the state of syncope.

The welfare of the patient is besides greatly promoted by the mental tranquillity arising from the prospect of immunity from suffering, which also induces persons to submit much more readily to the necessary operations, and often to undergo without hesitation treatment which was formerly impracticable because intolerable.

Such being the great benefits conferred by this agent, it is

melancholy to reflect that in many parts of Europe, and even of the United Kingdom, it is either withheld altogether or given so scantily as to be nearly useless. This arises from fear, inspired by several fatal cases that have occurred. But when I state that Mr. Syme has given chloroform about five thousand times without ever meeting with a death, and that Sir J. Simpson's experience, also very extensive, has, so far as I am aware, been equally satisfactory, it is clear that it may be used so as to be practically free from any risk whatever.

How then are the fatal cases to be accounted for? Heart disease has been supposed to be a common cause of them; and it is a prevalent opinion that it is highly dangerous to administer chloroform to persons affected with cardiac disorder.

It happens that the only death I ever witnessed under chloroform occurred in a person whose heart proved, on examination, to be extensively affected with fatty degeneration, such as would be regarded as sufficient explanation of sudden death under any circumstances. The particulars of this case, however, presented peculiar features, which lead me to take a different view of the part played by the chloroform from what might at first be assumed. The patient was a man above the middle period of life, affected with cancer of the penis, for which amputation of the organ was to be performed. The gentleman in charge of the chloroform, considering the momentary nature of the operation, purposely abstained from giving it as fully as usual, and had removed the cloth containing it from the face before the operation was commenced. The surgeon now placed his finger on the patient's wrist, and having ascertained that the pulse was good, at once affected the amputation almost instantaneously. I observed that the passage of the knife through the member was accompanied by a start of the patient's body; the bandage used to control the bleeding was then removed, but no blood flowed from the arteries; he was found to have no pulse at the wrist; in short, he was dead. From these facts we can hardly doubt that death was a consequence of the shock of the operation acting on a diseased heart; and the only question is whether the circumstance that he had taken chloroform promoted that result. From the foregoing considerations such a thing seems altogether improbable, as we have seen that chloroform protects the heart from the effect of shock. The fact that the patient started proved that reflex action was not abolished in the voluntary muscles, and confirmed the statement of the administrator that the chloroform was imperfectly given. My own impression is, that if it had been pushed to the usual degree the fatal occurrence would have been averted.

I have given this case in detail because I believe it may be regarded as typical of a considerable class in which death has taken place suddenly at the commencement of an operation with imperfect administration of chloroform, which stands to the fatal event in the relation of an accidental concomitant, or rather a preventive insufficiently used.[1]

A death essentially similar, though more obviously unconnected with chloroform, took place on the occasion when it was intended to have administered it for the first time in the Edinburgh Infirmary; but Dr. Simpson being prevented from attending, the operation was commenced without the anaesthetic, and the patient died suddenly immediately after the first incision. It has often been remarked that if the original intention had been carried out, chloroform would never have been heard of again in Edinburgh, but it is very likely that the man might then have lived to testify to its benefits.

There is another class of fatal cases in which the use of chloroform seems to have been simply a coincidence, the real cause of death being mental emotion, acting usually upon a disordered heart.

Dr. Snow mentions a distinct example of this, where a mere profession of administering chloroform was made, and the patient died of fright; and I am able to give, from Edinburgh experience, an instance in which chloroform was still more remotely concerned. The late Dr. Richard Mackenzie, being called to see a gentleman who had fractured his radius, had some thought of employing chloroform in examining the arm, but, changing his mind, made the necessary manipulations without it. He then proceeded to leave the house, but had not got down the steps leading from the door when he was called back with the announcement that his patient had suddenly expired.

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[1] An observation made several years ago by Mr. Bickersteth, of Liverpool, has an interesting bearing upon this class of cases. He noticed on three occasions in amputation of the thigh that the pulse stopped suddenly at the moment the knife entered the limb, but recovered itself in a few seconds. The patients were under the influence of chloroform; but as Mr. Bickersteth never observed the same thing again, though he watched the pulse carefully at the same period in a great number of capital operations under chloroform, it seems probable that the anaesthetic was not administered to its full degree in those instances. (See *Monthly Journal of Medical Science,* September, 1853.)

Had chloroform been held near the face a few seconds before this occurrence, it would certainly have been blamed, though with manifest unfairness; and a similar injustice seems to have been committed with regard to several cases in which fatal syncope has taken place early in the administration of the anaesthetic, when the brief period of inhalation concurred with the symptoms in showing that the patient was little, if at all, under its influence. A fear of the chloroform itself seems to have been the exciting cause in some of these cases; and one reason why no such instance has occurred in the Edinburgh Infirmary is probably the unlimited confidence reposed in this agent by the inmates of that institution.

It might, perhaps, have been expected *a priori* that chloroform, in the early or exciting stage of its operation, would act upon a diseased heart like mental emotion, and cause irregularity or cessation of its contractions. This, however, does not seem to be the case; and, judging from my own experience, I should say that it tends rather to remove intermission or irregularity of the pulse. On the whole I believe that chloroform, by preventing shock and mental effort during the operation as well as anxiety before it, is in reality a great source of safety in heart disease; and that if a person with known cardiac affection decides to place himself in the hands of the surgeon, so far from being unsuited for the anaesthetic, he is before all others the man who stands most in need of its protecting influence.

Nevertheless, even when the heart is perfectly healthy, it is quite possible to administer chloroform so as to produce a directly sedative and deadly influence upon the cardiac ganglia. This truth was deeply impressed upon me eight years ago by the following occurrence.

An eminent London physician, desirous of making some experiments upon the heart, selected a young donkey for the purpose, and requested me to maintain artificial respiration, which was done by means of a large pair of bellows connected with a tube tied into the trachea, the animal having been previously put under the influence of chloroform. The chest having been opened, the investigation was continued for a while, when the creature began to exhibit signs of returning consciousness. To avert this I removed the bellows, and poured into them a considerable quantity of chloroform, and resumed the artificial

respiration with energy for a short time, the natural respiratory movements meanwhile continuing; when suddenly the heart, which lay exposed before us, ceased to beat, and refused to contract again even when its muscular substance was pinched, which showed that its nervous apparatus was paralysed.

This was no doubt caused by the air becoming highly charged with chloroform in passing over the extensive evaporating surface presented by the interior of the bellows. For it had been before shown by Dr. Snow, from experiments upon the lower animals, that an atmosphere containing more than a certain percentage of the narcotic vapour stops the heart before breathing ceases, whereas the reverse occurs when the chloroform is more diluted with air.[1] Hence, with the view of preventing fatal syncope, Dr. Snow contrived an inhaler for regulating the amount of chloroform vapour in the inspired air, and used it in upwards of four thousand cases, of which only one was fatal, and even that seemed to be so independently of the chloroform. Finding his ingenious efforts crowned with such success, and charitably supposing that all were as careful as himself, he concluded that fatal cases in the hands of others could result only from a faulty method of administration; and assuming that when chloroform is given from a folded cloth it is apt to be in too concentrated a form, he attributed most of the deaths that have occurred to paralysis of the heart from this cause.

But the cloth being the means which has been used from the first in Edinburgh, with success even superior to Dr. Snow's, I have been long satisfied that his argument was fallacious; yet as his special devotion to the subject, and the valuable facts which he has communicated regarding it, render his opinion influential, I have thought it worth while to subject a matter of such great practical importance to experimental inquiry; and about the usual quantity of the liquid being employed, I find that, so far from the amount of chloroform given off from the cloth being in dangerous proportion to the air inhaled, the whole quantity which evaporates from the under surface, even

[1] I have noticed, however, that different animals differ in their susceptibility to chloroform. Thus frogs or mice may be kept for any length of time under its influence; but bats are very apt to die when treated in exactly the same way.

when the rate is most rapid, viz., just after the liquid has been poured upon it, is below Dr. Snow's limit of perfect security against primary failure of the heart.[1]

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[1] The experiments were performed in the following manner: A cloth, similar in all respects to what would be used in practice, was supported upon a light wire framework, and suspended at a little distance from the floor by a thread, connected with one end of the beam of a balance, projecting over the edge of the table on which it stood. The weight of the cloth having been ascertained, a weighed quantity of chloroform, corresponding to 1½ fl. dr., which is the amount commonly used, was poured upon the middle of the lower surface of the cloth, which was then allowed to hang close above my face, so that I might breathe fully upon it, while inspiration was performed through a long india-rubber tube to avoid inhaling the chloroform vapour. The amount lost by the cloth was indicated by the weights in the scale at the other end of the beam. At the commencement of an experiment the weight was made a few grains less than the sum of the weights of the cloth and chloroform together, and an assistant noted the second when the scale with the weights in it came to preponderate; then removed ten grains so as to allow the scale to rise, and again watched the time of its descent; and repeated this process several times, thus obtaining a very accurate record of the rate of alteration in the weight. The lower surface of the cloth, which was made slightly concave, was circumstanced just as in the early period of the administration of chloroform, except that the inspired air was drawn from a distance. Inspiration does not, however, materially affect the rate of evaporation, as was found by experimenting with a cloth arranged above the mouth of a tube into which air was drawn by an appropriate apparatus. Allowance being made for the slight gain in weight that the cloth would obtain from absorbing moisture from the breath, the amount of chloroform lost from both surfaces together was thus easily determined. In order to ascertain how much escaped from the upper surface, experiments were made with the same cloth, having first the upper and then the under side securely covered with oil-silk, the arrangements being as above described, except that my face was not below the cloth. The quantity given off from the upper surface in a normal atmosphere was thus determined; and this being subtracted from the whole loss from both surfaces under the circumstances of inhalation, gave the amount that evaporated from the lower surface only. At the temperature of 70° Fahr. this proved to be, from the average of several experiments, at about the rate of 24 gr. per minute during the first half-minute; and allowing, with Dr. Snow, that 20 gr. of chloroform correspond to 15·3 cubic inches of the vapour, and that 400 cubic inches of air are in-

But considering the great diffusibility of the vapour, and the large amount blown away in expiration, it is evident that only a small proportion of that which comes from the lower surface of the cloth really enters the lungs. Were it otherwise, it would be extremely dangerous to give chloroform with the cloth to infants, for as they inhale but a small amount of air, they would then breathe the vapour in a very concentrated state; yet all are agreed that infants are peculiarly favourable subjects for chloroform. In truth, the quantity dissipated into the surrounding air when the cloth is used involves considerable wastefulness in this means of administration, which is its only disadvantage as compared with an inhaler, but this is abundantly compensated by its greater simplicity, and consequent greater safety. For any apparatus which has the effect of preventing the free access of the atmosphere must be liable to operate in the same deadly manner as the bellows in the case above related, and even when constructed upon the best principles, it will require most careful management, as is admitted by Dr. Snow with regard to his own inhaler. On the other hand, there can be no mistake about the manner of using the cloth, which is also always at hand under all circumstances.

The theory of syncope from too great strength of the anaesthetic vapour when the cloth is employed being erroneous, the greater number of the deaths still remain unaccounted for; and, if we except a very few instances for which we seem to have nothing to fall back upon but an idiosyncrasy so rare that it may practically be left out of consideration altogether, their explanation will, I believe, be found in an overdose of this potent narcotic from too long continued administration.

This is what might be expected from a general view of the

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haled in a minute, we get 4·5 per cent. as the proportion of the chloroform to the inspired air, on the hypothesis that all that evaporates from the lower surface enters the lungs; 5 per cent. being what Dr. Snow was led by his experiments to regard as the proportion at which the respiration was quite sure to fail before the circulation, and that at which he aimed with his inhaler. On the other hand, Dr. Snow assumed that, when the cloth is used at a temperature of 70° Fahr., 9·5 per cent. of chloroform is really inhaled; whereas, in truth, of the 4·5 per cent. a large amount is dissipated into the surrounding air.

statistics. Were we to ask ourselves in what sort of operations we should have anticipated most frequent deaths during the employment of chloroform, we should say in those which are likely to inspire great dread on account of their magnitude and severity, and to cause great shock and great hæmorrhage. More especially should these preponderate among fatal cases in general hospitals, where serious operations constitute the majority of those performed. The reverse of this, however, is what we actually find. Of the whole number of cases recorded by Dr. Snow in 1858, as due to the use of chloroform throughout the world during ten years, nine only occurred in any considerable surgical procedure at a general hospital; remarkably few, considering the enormous number of important operations that must have been performed during so long a period, and the variety in the qualifications of those who administered the chloroform. On the other hand, fourteen took place at similar institutions in connection with the most trivial matters, such as the removal of a toe-nail, the amputation of a finger, the passing of a catheter, or the cauterizing of a wart. The only rational explanation of this seems to be, that when some great operation is to be performed, like the amputation of a thigh or the removal of a stone from the bladder, plenty of well-qualified assistants are present, and each of them, including the giver of the chloroform, is duly impressed with the importance of his office, and bestows the requisite pains upon it. But when some trifle is to be done, the whole affair is apt to be regarded too lightly, and the administration of the anaesthetic is perhaps confided to some unsuitable person, who also allows his attention to be distracted by other matters. This conclusion is entirely in accordance with my own experience, which, while it has convinced me more and more of the safety of chloroform if properly given, has impressed me deeply with the necessity for more vigilant care in its employment than is sometimes apt to be bestowed.

But an overdose of chloroform may be caused by attention misapplied, as well as by want of attention. The requisites for safety in using it will be best introduced by a short account of what ordinarily occurs in the mode of administration with which I am most familiar. A common towel being arranged so as to form a square cloth of six folds, enough chloroform is poured upon it to moisten a surface in the middle about as large as the

palm of the hand, the precise quantity used being a matter of no consequence whatever. The patient having been directed to loosen any tight band round the neck, and to shut his eyes to protect them from the irritating vapour, the cloth is held as near the face as can be comfortably borne, more chloroform being added occasionally as may be necessary. After a time, varying considerably in different individuals, but generally longest in adults who have been accustomed to the free use of narcotics, and shortest in young children,[1] signs of excitement begin to manifest themselves in various ejaculations and muscular efforts, which soon give place to a state of complete repose. The struggles of the patient are sometimes so violent as to require considerable force to restrain them, and, for this reason, at least one efficient assistant should always be in attendance. On the other hand, I have seen chloroform induce nothing but a tranquil slumber; and it is important to bear in mind that the stage of excitement cannot be reckoned on as invariably declaring itself at all.

The most convenient test of the patient being prepared for undergoing the operation is presented by the eye; not in the size of the pupil, which is inconstant in its indications, but in what is commonly spoken of as insensibility of the conjunctiva, though in truth it has no relation to sensation, which is abolished considerably earlier; but when unconscious winking no longer occurs on the eyeball being touched with the tip of the finger, we have a good criterion of the suspension of reflex action in the body generally. At this period the pulse is in about a normal condition, and the respiration is usually either natural or very slightly stertorous, though persons with a strong tendency to

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[1] I once met with an instance in which chloroform seemed incapable of affecting a patient. It occurred in the private practice of Mr. Syme, who was about to perform an operation, for which we proceeded to administer the anaesthetic; but after we had used the cloth till we were tired without any apparent effect, Mr. Syme went on with the operation while the patient was conscious. Such a case is, no doubt, excessively rare, but it is interesting as giving some colour to the hypothesis that idiosyncrasy in the opposite direction has existed in some very few fatal cases, which seem to admit of no other explanation, as alluded to in the text.

snore may do so almost from the commencement of inhalation. But if the administration of the chloroform be further persisted in, strongly stertorous breathing will soon be induced, and will become aggravated till it passes into complete obstruction to the entrance of air into the chest, though the respiratory movements of the thoracic walls still continue. Occasionally, however, the premonitory stertor is deficient, and the breathing becomes more or less suddenly obstructed. This is a point of great importance, for without close attention it may escape notice, when the patient will be placed in imminent peril. For though the respiration may be resumed spontaneously, this cannot be relied on, and it would seem that when chloroform is given in an overdose, the cardiac ganglia are apt to become enfeebled; and on this account asphyxia produces more rapidly fatal effects under its influence than in ordinary circumstances. But if the obstructed state of the breathing is noticed as soon as it occurs, and the cloth is immediately removed from the face, and the tip of the tongue seized with a pair of artery forceps[1] and drawn firmly forwards, the respiration at once proceeds with perfect freedom, the incipient lividity of the face is dispelled, and all is well.

I am anxious to direct particular attention to the drawing out of the tongue, because I am satisfied that several lives have been sacrificed for want of it. In order that it may be effectual, firm traction is essential. I have, more than once, seen a person holding the end of the organ considerably beyond the lips without any good effect, and, placing my hand on his, have given an additional pull that has re-established the respiration.

A simple experiment, which anyone may perform upon himself, is illustrative of this point. Stertorous breathing, such as occurs under chloroform, may be produced at will, and may be carried on even while the tongue is protruded to the extreme degree. But if the tongue is laid hold of with a handkerchief and pulled so as to cause decided uneasiness, stertorous breathing of any kind becomes impossible. That further traction, when extension already exists to the utmost, should produce such an

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[1] The artery forceps are the most convenient means of drawing the tongue forwards. The puncture which they inflict is of no consequence; the patient, if he notices it at all, supposes that he has bitten his tongue when under the chloroform.

effect is an apparent anomaly which it seemed important to explain. On investigating the subject, I noticed in the first place that stertorous breathing is of two essentially different kinds, of which one, that may be called *palatine,* consists in vibrations of the velum, and has either a buccal or nasal character, according as the air passes through the mouth or the nose; while the other, which is the profound stertor essentially concerned with chloroform, depends on a cause seated further down the throat, and, for reasons to be given immediately, may be termed *laryngeal.* By digital examination of my own throat I found that the latter variety, and the complete obstruction into which it passes, could still be produced when the tongue was separated by a considerable interval from the back of the pharynx, while a free passage for the air existed onwards to the lips, which showed that the general belief, that the obstruction depends on a "falling back of the tongue," is erroneous. Also the epiglottis, instead of being folded back during the obstruction, as some have supposed, had its anterior edge directed forwards; and though it was thrown into vibrations when the stertor was strongest, it was evident that the cause of the sound was more deeply placed. I also found that, although firm traction upon the tongue abolished the obstruction and the stertor, it did not appear to produce the slightest change in the position of the base of the tongue; nor did it move the os hyoides upon the thyroid cartilage, as examined from without. Hence I was led to conclude that the beneficial effect of this procedure could not be explained mechanically, but must be developed in a reflex manner through the medium of the nervous system. The fact that, when sensation is perfect, some degree of pain is caused in the process, implying an irritation of the nerves, was in favour of this view; while the general abolition of reflex action by chloroform did not seem strongly opposed to it, considering that the reflex respiratory movements, including those of the glottis, go on in a person under the influence of chloroform.

For further elucidation of the matter I had recourse to the laryngoscope; and, after a little patience, found no difficulty in inspecting my own vocal apparatus without employing any depressor of the tongue, using simply the small oblique long-handled speculum and a common mirror in bright sunlight. I

then ascertained that the true laryngeal stertor results from the vibration of the portions of mucous membrane surmounting the apices of the arytaenoid cartilages, i.e., the posterior parts of the arytaeno-epiglottidean folds (thick and pulpy in the dead body, but much more so when their vessels are full of blood), which are carried forwards to touch the base of the epiglottis during the stertorous breathing, and are placed in still closer apposition with it when the obstruction becomes complete. Having one hand at liberty, I was able to observe the effect of drawing forward the tongue under these circumstances, and I saw that firm traction induced the obstructing portions of mucous membrane in contact with the epiglottis to retire from it for about an eight of an inch, so as to allow free passage for the air, while the epiglottis itself was not moved forwards in the slightest degree.[1]

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[1] While the true laryngeal stertor was thus produced and thus removed, a sort of spurious snoring might be made by approximation of the vocal cords; but this spurious stertor was, like the voice, quite unaffected by drawing out the tongue. These observations were made on September 21 of the present year (1861). I find that there are four ways in which the passage through the larynx may be closed. First, the folding back of the epiglottis over the opening into the pharynx, as is generally believed to take place in swallowing, and may be demonstrated by arresting an act of deglutition in its progress, and insinuating the finger between the tongue and the roof of the mouth to the epiglottis, which is then felt to be turned backwards, and to return to its usual position as the act of deglutition is finished. Secondly, an approximation of the *sides* of the superior orifice of the larynx, in which the epiglottis is directed forwards, but folded longitudinally, so that its edges are in contact with one another while the arytaeno-epiglottidean folds are also in lateral apposition. (Note written in April, 1908: This fact was observed in the retching caused by the application of a solution of nitrate of silver to the larynx of a patient. I found that it is not universally recognized.) This occurs in retching, and doubtless also in vomiting, when a folding back of the epiglottis, instead of protecting the larynx, would tend to direct into it the material passing from below upwards. Thirdly, an *antero-posterior* co-aptation of the structures of the laryngeal aperture at a somewhat deeper level, without any change in the position or form of the epiglottis, towards which the folds of mucous membrane above the apices of the arytaenoid cartilages are carried forwards, till they are in contact with its base. This is

Whether pulling the tongue operates by inducing or relaxing muscular contraction in the larynx may be matter for discussion, but the main conclusion, that it does not act merely mechanically, but through the nervous system, appears satisfactorily established. I have not hesitated to give the evidence on which it rests in full, as it appears to me to be of the highest practical moment. For it shows at once how grievous a mistake is committed by those who content themselves with gently drawing the apex of the tongue a little beyond the teeth, or pushing forward its base with the finger, or perhaps ascertaining that the epiglottis is not folded back. Such proceedings are instances of attention misapplied, and waste the golden opportunity for rescuing the patient from death. The proper treatment, like many other good things in medical practice, owes its origin to a false theory, but though the erroneous notion of obstruction by the tongue did good service in the first instance by suggesting the original method, it now tends to encourage supposed improvements upon it, which rob it entirely of its efficacy.

If the above description is correct, if it is true that when the administration of chloroform with the cloth is carried too far, the first serious symptom is an obstructed state of the respiration, which without watchful care may occur unnoticed, and, if allowed to continue, will endanger the life of the patient, but, if promptly treated, will harmlessly disappear—it follows that the attention of the administrator ought to be concentrated on the breathing, instead of being, as it too often is, diverted by the pulse, the pupil, or other matters still less relevant.

As an example of the risk that is run by want of close attention to the respiration I may mention the following case. A surgeon of considerable experience was giving chloroform to a patient on whom an operation was being performed, of which I

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seen in coughing, and also in laryngeal stertor; and it is probable that during sleep, when the respiration is so apt to become stertorous, there is but a very narrow chink between the epiglottis and these folds of mucous membrane, which would thus serve to protect the deeper parts of the air-passages from the introduction of foreign matters in the state of unconsciousness. Fourthly, the closure of the *rima glottidis* in the production of voice. The white *chordae vocales* form a beautiful contrast with the highly vascular structures in their vicinity.

was a mere spectator, but I noticed that stertorous breathing came on, and gradually passed into complete obstruction, at a time when the administrator was gazing with interest upon the proceedings of the operator. Seeing that the patient was in danger, I suggested to the giver of the chloroform the propriety of pulling forward the tongue. He replied that this was uncalled for, and pointed to the heavings of the chest as evidence that breathing was proceeding freely. Knowing from what had gone before that those efforts were doing nothing for the respiratory function, and feeling that there was no time for discussion, I stepped out of my province so far as to seize the tongue myself and draw it forward, when a long and loudly stertorous inspiration demonstrated the necessity for the interference. Had the delusive movements of the chest been trusted, it is probable that they might have continued till the heart had become so enfeebled by the asphyxial state as to cause no perceptible pulse at the wrist; and had death occurred under these circumstances, the case would have been set down as one in which the circulation failed before the respiration. The administrator would thus have been absolved from all blame, and the fatal event would have been attributed to idiosyncrasy, or to any heart disease which might have been discovered on post-mortem inspection.

The very prevalent opinion that the pulse is the most important symptom in the administration of chloroform is certainly a most serious mistake. As a general rule, the safety of the patient will be most promoted by disregarding it altogether, so that the attention may be devoted exclusively to the breathing. The chance of the existence of heart disease may seem to make this practice dangerous, but having followed it myself with increasing confidence for the last eight years, and knowing that it has been pursued all along by Mr. Syme, who has also acted on the maxim that every case for operation is a case for chloroform, and must, therefore, have given it to very many patients in whom cardiac disorder existed unknown to him, besides some in whom its presence had been ascertained, I feel no hesitation in recommending it. Even when serious disease of the heart is known to exist, it must be remembered that there is much less risk of syncope than of obstruction to the respiration; and while the latter will demand and repay immediate attention, the former, should it by any chance occur, being in all probability independent of any excess of chloroform, would not imperatively demand its discontinuance; nor would it be much influenced by treatment, supposing the patient to be already in the horizontal

posture, which is generally considered safest in all cases when chloroform is given.[1]

From these considerations it appears that preliminary examination of the chest, often considered indispensable, is quite unnecessary, and more likely to induce the dreaded syncope, by alarming the patient, than to avert it.

The obstructed state of the breathing, if allowed to continue long, would lead to a far more serious affection—paralysis of the nervous centre concerned in the respiratory movements. Pulling out the tongue would then of course have no good effect of itself, but it should be done to clear the way for artificial respiration, which is the means to be essentially trusted to under such circumstances; and if the air still fail to enter freely into the chest, an opening ought to be made without delay through the crico-thyroid membrane. Cold water should also be occasionally dashed upon the face and chest; and if a galvanic battery happen to be in readiness, one of its poles may be applied over the spinous processes of the upper cervical vertebrae, and the other to the praecordial region, with the object of rousing the respiratory and cardiac ganglia. This, however, is a means not very likely to prove beneficial, and if used in too intense a form it may do harm instead of good.

Preparatory to taking chloroform the patient should be directed to omit the last meal which would naturally precede it, as any food in the stomach is almost sure to give rise to troublesome vomiting during the inhalation. The only after-treatment necessary is to allow the effects of the chloroform to pass off

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[1] From the views expressed in the text regarding the relation of syncope to the administration of chloroform, it might be inferred that no great danger would be incurred by giving it in the sitting posture when circumstances particularly require it; and accordingly Dr. Snow informs us that he has done this on several occasions without any bad result. But considering the possibility of an overdose, and the feebleness of the heart which that seems to entail, it is no doubt wisest, as a general rule, to have the patient reclining. Dentists, it is true, give chloroform in the sitting posture; but, so far as I have seen, they do not carry the administration beyond a slight degree, sufficient to deaden sensation without affecting reflex action, dexterously managing to open the mouth and operate upon it while the muscles of the jaws are rigid.

in a quiet sleep; and the only bad consequence likely to arise is a tendency to sickness, which sometimes causes annoyance during the first twenty-four hours or so.[1]

Chloroform is universally applicable in the various departments of surgery, except in some few cases in which the assistance of the patient is required, and in operations involving copious haemorrhage into the mouth. Blood may trickle in small amount into the pharynx without risk of choking, deglutition being carried on unconsciously during anaesthesia; and even in some instances when the bleeding is more serious, as in removing portions of the jaws, pain may be avoided to a great extent by giving the chloroform during the more superficial parts of the operation, and allowing the patient to recover partially before undertaking its deeper stages.

The main conclusions arrived at in this article may be expressed in a few words. It appears that chloroform, though resembling many other valuable means of treatment in being deadly when mismanaged, is free from danger if properly used, the following being the rules for its safe administration. A drachm or two of the liquid having been sprinkled upon the middle of a folded towel, hold it near the face, taking care that free space is afforded for the access of air beneath its edges, till the eyelids cease to move when the conjunctiva is touched with the finger. Meanwhile watch the breathing carefully; and if at any time it should become obstructed or strongly stertorous, suspend the administration and draw the tip of the tongue firmly forwards till the tendency to obstruction has disappeared.

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[1] It has been supposed by some that the use of chloroform increases the risk of pyaemia after careful operations; but experience has now abundantly proved the groundlessness of this apprehension. To take a single instance, the veins of the pelvic viscera being perhaps, next to those of the bones, more liable than any others to originate phlebitis after surgical interference, lithotomy would be much more fatal now than formerly were there any foundation in fact for the notion. The reverse, however, appears to be really the case. Thus, Mr. Cadge, one of the surgeons of the Norfolk and Norwich Hospital, an institution long celebrated for the successful treatment of stone, in a district abounding in calculous disease, informs me that the mortality after lithotomy has been still further reduced there since the introduction of chloroform.

These simple instructions may be acted on without difficulty by any intelligent medical man. The notion that extensive experience is required for the administration of chloroform is quite erroneous, and does great harm by weakening the confidence of the profession in this valuable agent, and limiting the diffusion of its benefits.

ON A NEW METHOD OF TREATING COMPOUND FRACTURES, ABSCESSES, ETC.

WITH OBSERVATIONS ON THE CONDITIONS OF SUPPURATION.

Collected Papers, vol. ii, p. 1—*Lancet*, 1867, vol. i, pp. 326, 357, 387, 507; vol. ii, p. 95.

THE next paper, which has been called "epoch-making," was published six years after the article on anaesthetics, when Lister was at the height of his career in Glasgow and had charge of some of the most unhealthy wards in the kingdom (*Collected Papers*, vol. ii, p. 124).

Before reading it we should recall some almost forgotten facts. (1) Compound fractures were then very grave injuries. They almost always suppurated and often led to the loss of a limb, and often to the patient's death. (2) When the wound was very small an attempt, sometimes successful, was made to avoid suppuration by allowing a scab to form or applying a patch of collodion; but, if the wound was large, they were usually treated with water-dressings, fomentations, or poultices. (3) As Lister thought—following Pasteur's teaching—that the air was teeming with germs, all of which were capable of causing sepsis, he considered the air to be the chief source of infection.

We must then imagine him contemplating a recent, but already contaminated compound fracture and his sample of crude carbolic acid, and pondering how he

should kill the germs already there and prevent the entrance of others. Was it not natural that he should first try what could be done with an antiseptic scab, influenced, as we know he was, by John Hunter's advice to encourage healing by scabbing? Unluckily the antiseptic scab was an irritating one, and it was fortunate that so large a measure of success was obtained in the early cases; especially as he was at first so ready to return to the septic dressing he was used to as soon as granulation seemed to be well established. If we did not all know how hard it is to break away from old habits, we should marvel that he went back to "simple water dressing" on the thirteenth day in Case 1, and on the eleventh day in Case 2; and also that in the latter he did not attribute to this cause the onset of hospital gangrene. An interesting relic of pre-antiseptic days was the application of hot fomentations outside the antiseptic dressings.

It will be observed that the first systematic application of carbolic acid to the interior of the wound was in Case 4, and thus was started a contest which is not settled after the lapse of fifty years.

Reading between the lines one appreciates the keen personal interest Lister took in his patients, and the excitement and wonder as new clinical and pathological facts were revealed.

But we will now leave the reader to make his own comments. He will no doubt be struck with Lister's patience, and his firm conviction that a new truth had been revealed, which carried him through in spite of such great difficulties and some failures.

When he attacked the far simpler problem of the treatment of abscesses described in the second part of the paper the difficulties were much less, and his uniform success proved that his faith had been justified.

ON A NEW METHOD OF TREATING COMPOUND FRACTURE, ABSCESS, &c., WITH OBSERVATIONS ON THE CONDITIONS OF SUPPURATION.

ON COMPOUND FRACTURE.

THE frequency of disastrous consequences in compound fracture, contrasted with the complete immunity from danger to life or limb in simple fracture, is one of the most striking as well as melancholy facts in surgical practice.

If we inquire how it is that an external wound communicating with the seat of fracture leads to such grave results, we cannot but conclude that it is by inducing, through access of the atmosphere, decomposition of the blood which is effused in greater or less amount around the fragments and among the interstices of the tissues, and, losing by putrefaction its natural bland character, and assuming the properties of an acrid irritant, occasions both local and general disturbance.

We know that blood kept exposed to the air at the temperature of the body, in a vessel of glass or other material chemically inert, soon decomposes; and there is no reason to suppose that the living tissues surrounding a mass of extravasated blood could preserve it from being affected in a similar manner by the atmosphere. On the contrary, it may be ascertained as a matter of observation that, in a compound fracture, twenty-four hours after the accident the coloured serum which oozes from the wound is already distinctly tainted with the odour of decomposition, and during the next two or three days, before suppuration has set in, the smell of the effused fluids becomes more and more offensive.

This state of things is enough to account for all the bad consequences of the injury.

The pernicious influence of decomposing animal matter upon the tissues has probably been underrated, in consequence of the healthy state in which granulating sores remain in spite of a

very offensive condition of their discharges. To argue from this, however, that fetid material would be innocuous in a recent wound would be to make a great mistake. The granulations being composed of an imperfect form of tissue, insensible and indisposed to absorption, but with remarkably active cell-development, and perpetually renovated as fast as it is destroyed at the surface, form a most admirable protective layer, or living plaster. But before a raw surface has granulated, an acrid discharge acts with unrestrained effect upon it, exciting the sensory nerves, and causing through them both local inflammation and general fever, and also producing by its caustic action a greater or less extent of sloughs, which must be thrown off by a corresponding suppuration, while there is at the same time a risk of absorption of the poisonous fluids into the circulation.

This view of the cause of the mischief in compound fracture is strikingly corroborated by cases in which the external wound is very small. Here, if the coagulum at the orifice is allowed to dry and form a crust, as was advised by John Hunter,[1] all bad consequences are probably averted, and, the air being excluded, the blood beneath becomes organized and absorbed, exactly as in a simple fracture. But if any accidental circumstance interferes with the satisfactory formation of the scab, the smallness of the wound, instead of being an advantage, is apt to prove injurious, because, while decomposition is permitted, the due escape of foul discharges is prevented. Indeed, so impressed are some surgeons with the evil which may result from this latter cause, that, deviating from the excellent Hunterian practice, they enlarge the orifice with the knife in the first instance and apply fomentations, in order to mitigate the suppuration which they render inevitable.

Turning now to the question how the atmosphere produces decomposition of organic substances, we find that a flood of light has been thrown upon this most important subject by the philosophic researches of M. Pasteur, who has demonstrated by thoroughly convincing evidence that it is not to its oxygen or to any of its gaseous constituents that the air owes this property, but to minute particles suspended in it, which are the germs of various low forms of life, long since revealed by the microscope,

[1] See "Works of J. Hunter," edited by Palmer, vol. i, p. 429.

and regarded as merely accidental concomitants of putrescence, but now shown by Pasteur to be its essential cause, resolving the complex organic compounds into substances of simpler chemical constitution, just as the yeast plant converts sugar into alcohol and carbonic acid.

A beautiful illustration of this doctrine seems to me to be presented in surgery by pneumothorax with emphysema, resulting from puncture of the lung by a fractured rib. Here, though atmospheric air is perpetually introduced into the pleura in great abundance, no inflammatory disturbance supervenes; whereas an external wound penetrating the chest, if it remains open, infallibly causes dangerous suppurative pleurisy. In the latter case the blood and serum poured out into the pleural cavity, as an immediate consequence of the injury, are decomposed by the germs that enter with the air, and then operate as a powerful irritant upon the serous membrane. But in case of puncture of the lung without external wound, the atmospheric gases are filtered of the causes of decomposition before they enter the pleura, by passing through the bronchial tubes, which, by their small size, their tortuous course, their mucous secretion, and ciliated epithelial lining, seem to be specially designed to arrest all solid particles in the air inhaled. Consequently the effused fluids retain their original characters unimpaired, and are speedily absorbed by the unirritated pleura.

Applying these principles to the treatment of compound fracture, bearing in mind that it is from the vitality of the atmospheric particles that all the mischief arises, it appears that all that is requisite is to dress the wound with some material capable of killing these septic germs, provided that any substance can be found reliable for this purpose, yet not too potent as a caustic.

In the course of the year 1864 I was much struck with an account of the remarkable effects produced by carbolic acid upon the sewage of the town of Carlisle, the admixture of a very small proportion not only preventing all odour from the lands irrigated with the refuse material, but, as it was stated, destroying the entozoa which usually infest cattle fed upon such pastures.

My attention having for several years been much directed to the subject of suppuration, more especially in its relation to decomposition, I saw that such a powerful antiseptic was peculiarly adapted for experiments with a view to elucidating

that subject, and while I was engaged in the investigation the applicability of carbolic acid for the treatment of compound fracture naturally occurred to me.

My first attempt of this kind was made in the Glasgow Royal Infirmary in March 1865, in a case of compound fracture of the leg. It proved unsuccessful, in consequence, as I now believe, of improper management; but subsequent trials have more than realized my most sanguine anticipations.

Carbolic acid[1] proved in various ways well adapted for the purpose. It exercises a local sedative influence upon the sensory nerves; and hence is not only almost painless in its immediate action on a raw surface, but speedily renders a wound previously painful entirely free from uneasiness. When employed in compound fracture its caustic properties are mitigated so as to be unobjectionable by admixture with the blood, with which it forms a tenacious mass that hardens into a dense crust, which long retains its antiseptic virtue, and has also other advantages, as will appear from the following cases, which I will relate in the order of their occurrence, premising that, as the treatment has been gradually improved, the earlier ones are not to be taken as patterns.

CASE 1.—James G——, aged 11 years, was admitted into the Glasgow Royal Infirmary on the 12th of August, 1865, with compound fracture of the left leg, caused by the wheel of an empty cart passing over the limb a little below its middle. The wound, which was about an inch and a half long, and three-quarters of an inch broad, was close to, but not exactly over, the line of

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[1] Carbolic acid is found in the shops in two forms—the glacial or cystalline, solid at ordinary temperatures of the atmosphere; and the fluid, which sometimes passes under the name of German creosote. The fluid variety is sold in various degrees of purity. The crude forms are objectionable from their offensive odour; but the properly rectified product is almost fragrant. Different samples, however, differ much in energy of action, and hence, though I have hitherto employed the liquid kind in compound fracture, it would probably be better to use the crystallized form, melting it by placing the vessel containing it in warm water for a few minutes. Carbolic acid is almost absolutely insoluble in water, but dissolves readily in various organic liquids, such as the common fixed oils or glycerine.

fracture of the tibia. A probe, however, could be passed beneath the integument over the seat of fracture and for some inches beyond it. Very little blood had been extravasated into the tissues.

My house-surgeon, Dr. Macfee, acting under my instructions, laid a piece of lint dipped in liquid carbolic acid upon the wound, and applied lateral pasteboard splints padded with cotton wool, the limb resting on its outer side, with the knee bent. It was left undisturbed for four days, when, the boy complaining of some uneasiness, I removed the inner splint and examined the wound. It showed no signs of suppuration, but the skin in its immediate vicinity had a slight blush of redness. I now dressed the sore with lint soaked with water having a small proportion of carbolic acid diffused through it; and this was continued for five days, during which the uneasiness and the redness of the skin disappeared, the sore meanwhile furnishing no pus, although some superficial sloughs caused by the acid were separating. But the epidermis being excoriated by this dressing, I substituted for it a solution of one part of carbolic acid in from ten to twenty parts of olive oil, which was used for four days, during which a small amount of imperfect pus was produced from the surface of the sore, but not a drop appeared from beneath the skin. It was now clear that there was no longer any danger of deep-seated suppuration, and simple water dressing was employed. Cicatrization proceeded just as in an ordinary granulating sore. At the expiration of six weeks I examined the condition of the bones, and, finding them firmly united, discarded the splints; and two days later the sore was entirely healed, so that the cure could not be said to have been at all retarded by the circumstance of the fracture being compound.

This, no doubt, was a favourable case, and might have done well under ordinary treatment. But the remarkable retardation of suppuration, and the immediate conversion of the compound fracture into a simple fracture with a superficial sore, were most encouraging facts.

CASE 2.—Patrick F——, a healthy labourer, aged 32, had his right tibia broken on the afternoon of the 11th of September, 1865, by a horse kicking him with its full force over the anterior

edge of the bone about its middle. He was at once taken to the infirmary, where Mr. Miller, the house surgeon in charge, found a wound measuring about an inch by a quarter of an inch, from which blood was welling profusely.

He put up the fracture in pasteboard splints, leaving the wound exposed between their anterior edges, and dressing it with a piece of lint dipped in carbolic acid, large enough to overlap the sound skin about a quarter of an inch in every direction. In the evening he changed the lint for another piece, also dipped in carbolic acid, and covered this with oiled paper.[1] I saw the patient next day, and advised the daily application of a bit of lint soaked in carbolic acid over the oiled paper; and this was done for the next five days. On the second day there was an oozing of red fluid from beneath the dressing, but by the third day this had ceased entirely. On the fourth day, when, under ordinary circumstances, suppuration would have made its appearance, the skin had a nearly natural aspect, and there was no increase of swelling, while the uneasiness he had previously felt was almost entirely absent. His pulse was 64, and his appetite improving. On the seventh day, though his general condition was all that could be wished, he complained again of some uneasiness, and the skin about the still adherent crust of blood, carbolic acid, and lint was found to be vesicated, apparently in consequence of the irritation of the carbolic acid. From the seventh day the crust was left untouched till the eleventh day, when I removed it, disclosing a concave surface destitute of granulations, and free from suppuration. Water dressing was now applied, and by the sixteenth day the entire sore, with the exception of one small spot where the bone was bare, presented a healthy granulating aspect, the formation of pus being limited to the surface of the granulations.

I now had occasion to leave Glasgow for some weeks, and did so feeling that the cure was assured. On my return, however, I was deeply mortified to learn that hospital gangrene attacked the sore soon after I went away, and made such havoc that amputation became necessary.

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[1] A cheap substitute for oiled silk, devised by the late Dr. M'Ghee, of the Glasgow Infirmary, and very useful for covering poultices, &c.

While I could not but feel that this case, by its unfortunate issue, might lose much of its value in the minds of others, yet to myself it was perfectly conclusive of the efficacy of carbolic acid for the object in view. At the same time it suggested some improvement in matters of detail. It showed that the acid may give rise to a serous exudation apt to irritate by its accumulation, and therefore that a warm and moist application would be advantageous to soothe the part, and also ensure the free exit of such exuded fluid. At the same time it appeared desirable to protect the crust with something that would retain the volatile organic acid more effectually than oiled silk or gutta-percha, through which it makes its way with the utmost facility. For this purpose a metallic covering naturally suggested itself, and as ordinary tin-foil is unsuitable from its porosity, I employed thin sheet-lead, and afterwards block-tin, such as is used for covering the jars of anatomical preparations, superior to lead on account of the facility with which it can be moulded to any shape that is desired.

For a long time, however, I had no opportunity of giving this improvement a trial, the compound fractures admitted into my wards during the next eight months being merely two cases with small wounds. One of these was a fracture of the ulna into the elbow-joint in a woman so old that suppuration, had it occurred, would probably have proved fatal. The orifice in the integument was extremely small, and all would most likely have gone on well had the bit of dry lint applied to check the free bleeding from the interior been left undisturbed, instead of being saturated with carbolic acid as it was. This, however, could not but be an additional safeguard, and at the same time it was satisfactory to find that the caustic application did not interfere with the usual healing by scabbing, cicatrization being found complete when the crust was removed.

The other case was a fracture of the humerus a little above the elbow in a young man, caused by a fall from a height of thirty-five feet, the wound, which was not quite half an inch in length, being situated at the inner side of the limb, where it must necessarily be covered by a splint. Dr. Watson, then my house surgeon, applied lint dipped in carbolic acid covered with a slightly concave piece of sheet-lead about as large as a shilling, and put up the limb in pasteboard padded with cotton. At the

end of ten days the inner side of the limb was uncovered for the first time, and merely as a matter of curiosity, when the lead, with the lint adhering to it, dropped off, disclosing a small superficial granulating sore without the slightest suppuration, just as in ordinary healing by scabbing. This case is interesting, not so much because the compound fracture was converted into a simple one, for this might have occurred under ordinary treatment, but because it showed that in any case of fracture complicated with a small wound, we have in carbolic acid a means which enables us to disregard the wound altogether after the splints have been applied, instead of being under the necessity of daily disturbing the apparatus to change the dressing.

At length a case presented itself well calculated to test the value of carbolic acid in compound fracture.

CASE 3.—John H——, aged 21, a moulder in an iron foundry, was admitted on the 19th of May, 1866, with compound fracture of the left leg, produced in the following manner. He was superintending the raising by crane of an iron box containing sand ready for a casting, the box and its contents weighing about 12 cwt., when one of the chains by which it was suspended slipped, and the box fell from the height of four feet with unbroken force upon the inner side of his leg, which was planted obliquely beneath it. Both bones were fractured, the tibia about its middle, and a wound an inch and a half in length, and three-quarters of an inch broad, was made at the inner aspect of the limb, on a level with the fracture of the tibia, and obviously communicating with it. At the same time the soft parts generally were much contused, as was evident from the great distension of the limb with extravasated blood. Dr. A. Cameron, my house surgeon, finding, on manipulating the limb, that bubbles escaped along with the blood, implying that air had been introduced during the movements of the leg as the patient was being carried to the infirmary, thought it best that I should see the case, which I did at 3 p.m., three hours and a half after the accident. In order to expel the air I squeezed out as much as I could of the clotted and fluid blood which lay accumulated beneath the skin, and then applied a bit of lint dipped in carbolic acid slightly larger than the wound, and over this a piece of sheet-tin about four inches square. Finally the limb was placed in paste-

board splints, resting on its outer side with the knee bent. At 8 p.m. some more acid was added with another piece of lint, so that the crust of clots, carbolic acid, and lint was about one-third of an inch in thickness. A hot fomentation also was applied over the inner aspect of the leg, the crust being protected by the tin. Next day he was pretty easy, and had passed a quiet night, though occasionally awakened by starting pains; the pulse was 90, but he took some food with relish. The surface of the crust was touched again with carbolic acid, and the fomentation was continued, and in place of the internal pasteboard splint, a large sheet of tin was applied over the flannel from the knee to the ankle, being retained in position by looped bandages. This proved a very satisfactory arrangement, the tin having sufficient firmness to answer the purpose of a splint, while it most effectually retained the moisture of the flannel, which, again, served as an excellent padding. The fomentation was changed night and morning, and gave great comfort to the patient, and once a day carbolic acid was applied lightly to the crust.

Two days after the accident the limb was easier, but the circumferential measurement of the calf continued the same, and the pulse was 96, though soft. On the fourth day—the critical period with reference to suppuration—the limb was free from pain, and the calf less tense, and distinctly reduced in dimensions; while the pulse had fallen to 80, and the patient had enjoyed his food after a good night's rest. After this the swelling steadily subsided, the skin remaining, as it had been from the first, free from the slightest inflammatory blush, and his general health was in all respects satisfactory. Seven days after the receipt of the injury there was some puriform discharge from the surface of the skin where the carbolic acid, confined by the smaller piece of tin that covered the crust, had produced excoriation by its caustic action; and to prevent needless irritation from this cause, the tin was reduced so as to leave only a narrow flat rim round a bulging part which corresponded to the crust.

About a fortnight after the accident a sense of fluctuation was experienced over the seat of fracture, but, as all was going on favourably otherwise, I hoped that this was due simply to serum from the effused blood; and in a few days it had com-

pletely disappeared, not a drop of pus meanwhile having escaped from beneath the crust. About this time the edges of the crust became softened by the superficial discharge from the surrounding parts, and these softened portions were daily clipped away with scissors. Thus the circumferential part of the crust which had overlapped the skin was removed, and that which lay over the extravasated blood in the wound was also reduced to smaller and smaller size.

On the 7th of June, nearly three weeks after the accident, an observation of much interest was made. I was detaching a portion of the adherent crust from the surface of the vascular structure into which the extravasated blood beneath had been converted by the process of organization, when I exposed a little spherical cavity about as big as a pea, containing brown serum, forming a sort of pocket in the living tissues, which when scraped with the edge of a knife, bled even at the very margin of the cavity. This appearance showed that the deeper portions of the crust itself had been converted into living tissue. For cavities formed during the process of aggregation, like those with clear liquid contents in a Gruyère cheese, occur in the grumous mass which results from the action of carbolic acid upon blood; and that which I had exposed had evidently been one of these, though its walls were now alive and vascular. Thus the blood which had been acted upon by carbolic acid, though greatly altered in physical characters, and doubtless chemically also, had not been rendered unsuitable for serving as pabulum for the growing elements of new tissue in its vicinity. The knowledge of this fact is of importance; as it shows that, should circumstances appear to demand it, we may introduce carbolic acid deeply among the blood extravasated in a limb, confident that all will nevertheless be removed by absorption. A few days later all traces of the little cavity had become obliterated by the granulating process.

At the close of the third week the application of carbolic acid to the crust was discontinued, and the original internal pasteboard splint padded with cotton was again employed, instead of the tin and fomentation. What remained of the crust was still kept protected with the tin cap, with the view of ascertaining how long it would continue to adhere; and at length, nearly four weeks after the accident, I tore it off from the

vascular surface beneath, which bled as I did so. The crust had preserved the subjacent parts from disturbance as effectually as if it had been a piece of living integument; and it is worthy of remark that the vascular surface below had not the pulpy softness of granulations, but was comparatively firm and substantial. The bit of crust still smelt of carbolic acid, though none had been applied for five days.

At the expiration of six weeks from the receipt of the injury the fragments were found firmly united in good position, just as if the fracture had been a simple one, though the cicatrization of the rather extensive sore was not complete till a later period.

CASE 4.—James W——, aged 10, was engaged in a turner's factory worked by steam power on the 8th of June, 1866, when his right arm was drawn in between a strap and a shaft turned by it. He called out for assistance, but thinks two minutes must have elapsed before the machinery was stopped, and during the whole of this time the strap, which was still moving while he held the arm steady, was cutting into the ulnar side of the forearm, breaking through the ulna about its middle, while the radius was bent with "green-stick" fracture. He was taken at once to the infirmary, where the wound was found to be about an inch and a half in depth, occupying more than half the circumference of the limb, chiefly at the dorsal aspect, but extending round also to the palmar side. The upper fragment of the ulna was protruding about an inch, and two strips of muscle, about a quarter of an inch in thickness and from two to three inches in length, were hanging out; the lacerated state of the parts confirming the boy's account of the accident.

On seeing him about two hours afterwards, I sawed off the protruding portion of the ulna, and the tags of muscle having been previously clipped away, I applied carbolic acid freely to the whole interior of the wound, including the exposed surface of the bone; and having straightened the radius, which gave way during the process, placed the limb upon a wooden palmar splint. Avoiding any attempt to approximate the lips of the wound, I covered it with a piece of sheet-tin, sufficiently large to overlap the sound skin about a quarter of an inch in every direction. The limb was fixed to the splint by a bandage, so arranged as to permit the removal of the tin without disturbing

the apparatus; and hot fomentations were applied over the whole. A few minutes after the carbolic acid was applied he said he was perfectly easy. At 7 o'clock he asked for food, and took it. His pulse was then 84. At 8 p.m. I saw him again, and applied beneath the tin a piece of lint dipped in carbolic acid, about as large as the wound. Noticing some distortion in the upper arm, I found that the humerus also was broken in its lower third, and applied splints accordingly, the limb being kept supported upon a pillow beside him. He slept a good deal during the night, though moaning and starting occasionally. Next day his pulse was 108; but he took his breakfast heartily, and the tongue was healthy, while he complained only of a little uneasiness about the elbow, and even this disappeared on changing the fomentation cloth. A piece of sheet-tin was now arranged so as to form a sort of cover for the forearm, including the hand. Being retained in position by looped bandages, it increased the steadiness of the limb, while it ensured efficiency of the fomentation.

Two days after the accident the oozing of blood and serum, which had been considerable during the previous twenty-four hours, had nearly ceased; but he still experienced comfort from the fomentation, though any pain which he felt was connected with the simple fracture of the humerus. His pulse was 88; his tongue clean and appetite good after a sound sleep at night; and from this time onward his general health continued perfectly satisfactory. On the fourth day a small quantity of pale, grey, slimy discharge was observed from beneath the crust at one part; and thinking that this might, perhaps, have occurred for want of proper action of the carbolic acid, I applied the latter with unusual freedom to the surface of the crust. This was repeated at night; and the same energetic use of the carbolic acid, twice in the twenty-four hours, was continued on the fifth day. Yet, on the sixth day, the discharge beneath the crust, instead of being diminished, was increased, and more puriform to the naked eye; while, under the microscope, there was clear indication of new cell-formation, whereas, on the day before, nothing but fibrinous material, with granular and other débris, had been discoverable. On the seventh day the discharge was still greater in amount; yet the limb remained free from pain, and was steadily diminishing in circumference, and pressure in the neigh-

bourhood of the crust failed to induce any increase of the discharge, which appeared to be merely superficial.

In the course of the next few days it became apparent that this discharge, so far from being the result of insufficient action of the carbolic acid, was caused by the stimulating influence of the acid itself, applied with greater freedom over a crust much thinner than that of Case 3. Suppuration from this cause is, however, productive of no mischief, as will be better understood from the sequel. That such was the case in this instance was manifest on the fourteenth day, when the crust, which was nearly detached, was removed, disclosing an appearance for which I confess I had not been prepared. In place of the deep and ragged wound was a granulating sore, nearly on a level with the skin, and pretty uniform in surface, except at one part about its middle, where there was a depression about half an inch in depth, at the bottom of which a small portion of the outer surface of the ulna was visible, bare, but of pink colour. Not only had the compound of blood and carbolic acid which had existed in the depths of the wound been organized, but the portions of tissue killed by the violence to which they had been subjected in the accident, and also those destroyed by the caustic action of the carbolic acid, had been similarly acted on, and all had been, so to speak, fused together into a living mass, without the occurrence of any deep-seated suppuration.

By the nineteenth day the exposed part of the bone was covered, and the depression in the sore obliterated by granulation, without any exfoliation occurring; and two days short of seven weeks after the accident the sore was entirely healed.

The extensive loss both of bone and of the soft parts made osseous union of the ulna a matter of difficulty, and on the 5th of August the limb was placed in a starched apparatus, to promote complete consolidation, and he was soon after discharged from the hospital.

About six weeks later he presented himself at the infirmary, and the bandage was removed in my absence, when, the bone appearing firm, he was allowed to dispense with the apparatus, and was unfortunately not directed to show himself again. In the course of a few weeks, however, he appeared with the fragments again movable. The starched bandage was therefore reapplied, but when I last saw him, some weeks ago, bony union

had not yet occurred. A good deal of osseous formation had, however, taken place, so that the fragments now overlapped each other; and should the cure be still incomplete when he next shows himself, the case will be a fair subject for Bickersteth's method of treating ununited fracture by drilling. Meanwhile, the radius being firm, and the injured extensors of the fingers having completely regained their powers, he will, in any event, have a very useful hand.

This case indicated a greater range of applicability of the treatment by carbolic acid than I had anticipated, and encouraged me to employ it under the almost desperate circumstances of the following case.

CASE 5.—Charles F——, a fine, intelligent boy, 7 years of age, was knocked down at 8 p.m., on the 23rd of June, 1866, by an omnibus crowded with passengers inside and out, and one if not both wheels passed over his right leg, breaking both the bones and inflicting a frightfully extensive wound. The person who brought him to the infirmary said that he had lost a great deal of blood, and the presence of a compress in the ham, placed there by the medical man who saw him at the time of the accident, corroborated this statement. When I saw the child, after an unavoidable delay of three hours, he was greatly prostrated by shock as well as haemorrhage, so much so that amputation appeared likely to afford but a slender chance of life, although the state of the injured parts seemed at first sight to admit of no alternative. The tibia, which was broken about its middle, lay exposed in a wound occupying almost the entire length and breadth of the inner aspect of the leg, reaching from the inner condyle of the femur to within an inch and a quarter of the tip of the internal malleolus; the skin having been stripped back so as to lay bare the gastrocnemius as well as the bone. The large flap of integument was perforated about two inches from its edge opposite to the seat of fracture, and there was also an opening in the skin on the outer side of the leg, implying that the violence had acted with full effect upon the whole thickness of the limb. Yet the bone was not comminuted, and the muscles, though evidently severely contused, were not much lacerated, while the anterior tibial artery was felt beating in the foot; and, hopeless as would have been the idea of trying to save the limb by

ordinary treatment, I determined to make the attempt by the help of carbolic acid.

Chloroform having been administered, the acid of full strength was applied with great freedom, the contused mass being repeatedly squeezed, to induce the liquid to insinuate itself into all its interstices, including that between the riding fragments of the tibia. The flap of skin was then brought towards its natural position, and lint soaked in the acid was placed upon the wide raw surface which still remained exposed, and over the lint a piece of sheet-tin. The other openings in the integument were similarly treated; and, the riding of the fragments having been corrected by extension, the limb was laid on its outer side, with the knee bent, upon an external pasteboard splint, moulded to the leg and foot, and strengthened by a temporary wooden splint. A porous cloth was applied over the tin to absorb the blood and serum which must escape from beneath its edges; and the whole apparatus was secured with a roller. At the conclusion of the dressing the pulse was 112.

He passed a restless night, though occasionally dozing, and the pulse next morning was 120. The bandage having been cut away sufficiently to enable the tin to be removed, the wound was found to have gaped so that the lint no longer covered the whole of it. Pieces of the cloth, which had become soaked with the exuded blood, were placed upon the exposed part, and also over the lint so as to make the crust more substantial, and the whole was freely treated with carbolic acid. The tin was then bulged out so as to be accommodated to the thickened crust, while overlapping the neighbouring skin to a slight extent; being retained in position by a couple of turns of bandage. A hot fomentation was then placed upon the inner aspect of the limb, and the whole leg enveloped in a large sheet of block-tin secured by looped bandages.

In the evening the pulse was 136, and on the following morning, thirty-six hours after the accident, it had risen to 168, and was very weak. He lay talking to himself in a rambling manner, unable to understand what was said to him. He was extremely restless, and had taken no food whatever since his admission. During the next night, however, he became composed, and took a little milk; and on the morning of the third day he was found to be again intelligent, while the pulse had

fallen to 140, and was of fair strength. The skin in the vicinity of the injury, both at the knee and ankle, was free from discoloration or swelling; but part of the large flap of skin over the calf was of purple tint, and had evidently lost its vitality. This dead part was touched with carbolic acid, to preserve it from decomposition, and convert it into a crust for the protection of the subjacent textures, and an additional piece of tin was applied to cover it. A good deal of brown transparent fluid escaped from beneath the crust.

On the fourth day the pulse was 120; he was quite bright and tranquil, and said he felt no pain. There was still no odour about the injured part, except that of carbolic acid. The discharge was much diminished, and was principally serous.

By the sixth day the pulse was as low as 108. He had a hearty appetite, and also took with avidity the six ounces of port wine allowed him during the twenty-four hours. His tongue, which had previously been dry, was moist. He had slept well at night, though waking occasionally with a scream. The discharge from beneath the crust, trifling in amount, was chiefly serous.

On the eighth day the splint was removed for the first time, and was covered with sheet-tin in order to prevent the discharge from softening the pasteboard. The leg had become slightly bent inwards through the yielding of the splint; and when it was now straightened, the upper margin of the crust became detached, exposing a deep granulating cavity. A bit of lint, dipped in carbolic acid, was applied lightly over this opening, and the tin was readjusted so as to cover it. Pressure in the neighbourhood of the injured part, about the knee, ankle, and calf, failed to induce the slightest increase of the discharge, which was thus shown to come merely from the surface beneath the crust, and was still for the most part transparent.

At the close of the second week his state was on the whole very favourable. His general health was much improved; and although he still suffered occasionally, especially at night, from restless movements of the limb, these had been much restrained by a new splint, extending from half-way up the thigh to the toes. The wound was certainly very large, measuring eight inches in length by six in greatest width; but it was healing round almost the entire circumference. In order to permit

cicatrization, which carbolic acid tends to check, the detached edges of the crust had been clipped away, and the exposed narrow ring of granulations was dressed with lint dipped in a solution of sulphite of potash—five grains to an ounce of water. The crust, however, was still touched daily as before with carbolic acid, while the tin still covered the whole of the injured part. By this means it was intended that cicatrization should be allowed to go on, and yet decomposition of the discharge be prevented; and this seemed to be to a great extent, if not entirely, attained.

There was, however, one unfavourable circumstance. The little sore on the outer side of the leg, which had been dressed separately without carbolic acid, and had for some time been observed to be increasing rather than diminishing, now assumed unmistakably the appearance of a mild form of hospital gangrene, and became blended with the main sore. For two days an attempt was made to correct the disease by touching the affected part with nitric acid; but on the eighteenth day it was clear that some more effectual measures must be adopted, as the skin in the vicinity had become insidiously undermined to a very serious extent. Accordingly I placed the boy under chloroform, and scraped away with a spoon all the soft grey sloughs, slitting up the skin in order to gain access to them, and in some parts clipping portions of it away, and then applied the strongest nitric acid thoroughly to the bleeding surface. As the disease extended up to the anterior edge of the crust, I thought it right to examine the state of the parts beneath, and as it was pretty loose I removed it. And now a sight presented itself which filled me with horror. There was, indeed, no appearance of hospital gangrene in the parts which the crust had covered, the granulations there having the florid aspect of perfect health; but in the large sore lay the lower fragment of the tibia, freely exposed to the extent of two inches and a half in length, bare and white like a macerated bone. At the upper end of this fragment, and apparently for a considerable distance from it, the bone was thus denuded round its entire circumference; and, judging from previous experience, there was reason to expect that, even if the patient should survive the profuse suppuration which was to be anticipated, about two inches of the whole thickness of the tibia must exfoliate, an amount of loss which in the

child's small limb, would of necessity render it utterly useless. The upper fragment was also bare for about half an inch just above its extremity, but the end itself was covered with prominent granulations.

Though despairing of any good result, I resolved to watch for a while the progress of events, prepared to amputate as soon as the boy's health should show signs of failing; and comforting myself with the reflection that he had been brought into a state greatly more favourable for the operation than on his admission. In order to keep down the amount of the discharge the sore was dressed with the sulphite-of-potash lotion, a poultice being applied to the part which had been treated with nitric acid. When the sloughs caused by the caustic separated a healthy surface appeared, which in the course of the next ten days was nearly healed. In other parts of the sore, however, grey patches occasionally showed themselves, assuming healthy characters after being touched with carbolic acid, which, when efficient, has the advantage over other caustics of being painless. But at length spots of hospital gangrene appeared in a form no longer amenable to this mild treatment, in spite of which they began to extend rapidly, and on the 26th of July it became necessary to put the child again under chloroform and apply nitric acid in the same thorough manner as before. This had the effect of producing a perfectly healthy state of the whole sore, which proceeded to heal with great rapidity; so that by the 8th of August it was found to measure an inch less in length and two inches less in greatest breadth than at the time when the crust was removed.

In the meantime his general health, instead of deteriorating, had improved, and he was evidently regaining flesh, while the discharge of pus was astonishingly little considering the state of the limb, being barely sufficient to soak the single layer of lint that covered the sore.

The explanation of this satisfactory state of things was afforded by an observation of much interest made at this period. Since the removal of the crust the granulations had been growing up on all sides about the bone, so that the bare part of the upper fragment was almost entirely covered in, and even the lower fragment, which projected beyond the level of the upper, was to a great extent embedded in the new growth. It had been

noticed before the end of this fragment was so much covered up, that granulations were sprouting from the medullary canal, showing that the bone was not dead in its entire thickness. Nevertheless, as the superficial parts had certainly lost their vitality, I had not doubted that a thin layer at least must exfoliate from the whole. Now, however, I observed that some of the surface which remained exposed had assumed a pink colour, implying that the layer of dead bone, whatever its thickness might have originally been, had become so thin as to be transparent, through absorption by new tissue growing in the interior. Further, on attempting to pass the eyed end of a probe between the tibia and the granulations which had enveloped it, I found to my surprise that the instrument could only be introduced for a very short distance, the granulations, with the exception of a narrow free border, being everywhere adherent. The new tissue outside the bone had coalesced with that within, after complete absorption of the intervening dead stratum. Hence the remarkable absence of discharge from around the bone.

During the following month I was absent from home, but was informed that the same process was for some time continued: the granulations gradually encroaching more and more on the exposed bone, and adhering to it as they advanced. The upper fragment was thus entirely covered without any exfoliation occurring, and the bare surface of the lower fragment was reduced to comparatively small dimensions. On the 10th of September the remainder of the dead part, being loose, was removed without difficulty as an exfoliation. It was about an inch in greatest length; but was of extremely irregular shape, full a quarter of the circumference of the tibia being deficient. At the upper end, where it had been most prominent and had become discoloured, it had nearly the full thickness of the dense tissue; but towards the lower end it became thinned away, so as to be in some places as delicate as tissue-paper. The outer surface presented near the margin an appearance of especial interest, being at some parts, even where the bone had considerable thickness, variously scooped and bevelled in a manner that admitted of no other explanation than that the granulations overlapping the dead bone externally had been engaged in its absorption. On applying a magnifier to these excavations in the external surface, they were seen to

present a peculiar velvety aspect, differing from the rest of the exterior, but resembling the internal parts of the exfoliation.

The only observation at all analogous to this with which I am acquainted is that of the effects produced upon the ivory pegs used in Dieffenbach's method of treating ununited fracture, the parts of the pegs driven into the bone having been observed, when removed, to have suffered diminution in size. This has hitherto remained as an isolated fact, and it has been regarded as an axiom in surgery that a piece of bone once dead must all come away as an exfoliation. Why it was that in the case before us the osseous tissue destroyed by external violence, aided by the action of carbolic acid, was so exceptionally affected by surrounding parts, the granulations in its vicinity discharging the office of absorbents of the dense tissue, instead of forming pus like those around an ordinary exfoliation, I will reserve for future discussion, when I shall have occasion to point out the great importance of the fact in its bearing both on pathology and practice. Meanwhile I may remark that it illustrates beautifully the function of absorption, which, even where solid substances are taken up, does not require any special set of absorbent vessels, but may be effected even by granulations, the most rudimentary of all tissues, each cell feeding upon any suitable substance in its vicinity.

We also see at once the value of the observation with reference to the treatment of compound fracture with carbolic acid; for it shows that in cases in which the bone is exposed, the acid may be applied so freely as to cause death of its tissue without necessarily inducing exfoliation.

The case was now reduced to one of simple fracture with a large granulating sore, and this was greatly diminished and healing rapidly, while the union of the fragments was becoming very firm; and the limb would doubtless soon have been entirely sound had it not been for that cruel scourge, hospital gangrene. This, however, had shown itself ten days before the removal of the exfoliation, not in the sore, but about an inch from its edge, as a pustule in the cicatrix, which on bursting disclosed a grey slough that soon showed its characters unmistakably, producing considerable destruction of the scar, although the original sore continued to heal kindly.

I will not enter into the history of this and numerous sub-

sequent attacks of the disease further than to state that they were partial in their effect, the unaffected parts still healing with rapidity, and that they continued to yield to the treatment with nitric acid; so that at one time the whole sore was very nearly healed.

But in the early part of October the disease assumed a more intractable form, and in spite of the most energetic use of nitric acid on several occasions, which produced illusory appearances of temporary improvement, by the 27th of the month the sore had become enlarged to nearly its original dimensions, while the limb had swollen greatly through inflammation caused by the irritation, and the boy's general health was rapidly giving way under the increased discharge and nervous excitement.

The question of amputation now again presented itself, but a good airy room in a different department of the hospital being happily now at my disposal, I determined to give the limb one last chance. Before he was taken to the new ward, nitric acid was once more thoroughly applied. His nurse was directed to change the poultice every three hours, and he continued to take wine and some tonic medicine. His general health immediately improved, and when the slough separated, the sore looked healthy. It was now dressed with lint dipped in a solution of sulphate of copper, five grains to an ounce of water, and over this a poultice, the whole being changed every three or four hours night and day; and under this treatment cicatrization proceeded rapidly. Yet when the scar had attained a certain width, a tendency to vesication again showed itself, threatening recurrence of the disease, and in order to prevent the newly formed epidermis from acquiring poisonous qualities as it seemed to do, I ordered the lint with the lotion, as well as the poultice, to be extended over the whole cicatrix. From the time this dressing was adopted the progress was uninterruptedly satisfactory till the 9th of January, when the sore was at length entirely healed, and he was allowed for the first time to put his foot to the ground. The contraction of the large cicatrix, involving at one part the gastrocnemius muscle, had caused some bending of the knee and pointing of the toes. The former has since become corrected spontaneously by his habitual attitude, sitting in bed with the legs extended before him. The pointing of the toes has also become diminished, and will probably soon pass

off entirely, without the division of the tendo Achillis, which I had in view. The tibia, which has long been firm, is of precisely the same length as the other, and the contour of the limb is natural. His general health also is excellent; but he was detained in the hospital till the 9th inst. (March 1867) on account of an obstinate eczematous eruption on the integument of the leg irritated by the long-continued poulticing.

CASE 6.—The following case terminated fatally, but from circumstances of an accidental nature; and I trust that the instruction to be derived from it will not be interfered with by the unhappy ultimate result.

John C——, aged 57, a labourer, was working in a quarry at Row, near Helensburgh, on the Clyde, at 9 a.m. on the 26th of October, 1866, when, striking with a crowbar an overhanging part, he brought down an enormous mass of stone weighing six or seven tons, which fell in large blocks on and about him. His right thigh-bone was broken in its lower third, and, as afterwards appeared, the end of the upper fragment was driven through the skin at the inner aspect of the limb a little above the knee. The right collar-bone was fractured at the same time, and he was severely contused in other parts. It was long before his only companion in the quarry could extricate him from his position, and the procuring of a conveyance involved further delay; so that a considerable period elapsed, during which he lost much blood from the thigh, before he could be taken to Helensburgh. Here he was placed on a litter, with a warm moist blanket round the limb, with the object, as he said, of checking the bleeding, which, however, it could not but tend to encourage. He was then conveyed by train to Glasgow, where he reached the infirmary six hours after the occurrence of the accident.

Dr. Archibald Cameron, the house surgeon, seeing the case to be a very grave one, at once sent for me, but without any delay introduced carbolic acid into the wound by means of a piece of lint held in a pair of dressing forceps, passing it about an inch in every direction beneath the integument, after squeezing out a considerable quantity of extravasated blood from the orifice, which was large enough to admit the tip of the finger.

On arriving, an hour after the patient's admission, I found him in a state of prostration sufficiently explained by the severity

of his injuries and by the blood lost to the circulation, including a large amount extravasated in the limb, and distending, not only the whole thigh, but the calf, the tenseness of which contrasted strikingly with the flaccidity of the other.

Under these circumstances decomposition of the blood effused among the tissues would have been necessarily fatal. And yet, considering the length of time that had elapsed since the receipt of the injury, and the fact that a reeking flannel had been for two hours in contact with the wound, and had already a somewhat offensive odour when removed from it, there seemed but a poor chance for the treatment with carbolic acid. On the other hand, taking into account the man's time of life and general condition, I believed that to amputate through the thigh infiltrated with blood would be certainly to kill him. And therefore, as it was impossible to say that the other treatment had no chance, while, if it should prove successful, it would have the immeasurable superiority of saving limb as well as life, I determined to persevere with it.

Having removed from the wound the dressings placed on it by Dr. Cameron, I forcibly squeezed out a further large amount of blood, and applied carbolic acid in lint and also mixed with blood, so as to provide for a crust of considerable thickness overlapping the skin by about half an inch every way. This was covered with a circular piece of tin, two inches across, well bulged out except a flat margin about a quarter of an inch wide, which rested on the surrounding integument. This tin cap was retained in position by a single turn of bandage tied round the limb.

The lower end of the upper fragment was much displaced downwards in the vicinity of the wound, but returned towards its natural position on extension of the limb. There still remained considerable depression anteriorly over the seat of fracture; but the lower fragment did not seem to project towards the ham so much as to forbid the use of the long splint. This I accordingly employed with two interior splints to support the muscles of the thigh, one of Gooch's material on the outer aspect, the other a large sheet of stout block-tin, embracing the anterior, inner, and posterior aspects of the limb to a little below the knee, padded in the first instance with a dry towel, for which a hot fomentation should be substituted when all tendency to

haemorrhage should have ceased. The object of having the tin extend round the back of the thigh was that it might prevent the discharges from soaking into the bed beneath; and in this way it proved extremely useful.

He passed an uneasy though not entirely sleepless night, suffering more from his shoulder and bruised side than from the thigh. Next morning his aspect was favourable, the pulse 76, and tongue natural; he took a little tea for his breakfast, but nothing solid. The tin cap having been removed, care being taken to avoid detaching the crust along with it, carbolic acid was applied to the surface of the latter. A hot fomentation cloth was then placed on the inner side and front of the thigh and gave him great comfort, and when the dressing was completed he was quite easy. The interior splints being kept in position by looped bandages, and the long splint by the usual folded sheet fixed by pins, along with the perineal band and handkerchief round the foot, the fomentations could be changed night and morning without any disturbance of the limb.

The following night he had a good deal of sleep, the thigh not causing him any inconvenience; and next day, the third after the accident, he took solid food with relish. His pulse was 72, and his tongue continued moist, though he was somewhat thirsty. The crust was touched again with carbolic acid, and covered with a circular piece of calico to prevent the tin cap from adhering to it. He still found comfort in the fomentations.

On the fourth day he made a substantial breakfast after a good night's rest, and was not thirsty. There was, however, now seen for the first time a slight blush of redness on the front of the thigh over the seat of injury. This was on the fifth day somewhat increased, and the thigh and calf were both more swollen. The tongue also was slightly furred at the base, and his appetite was not quite so good.

On the sixth day the dimensions and appearance of the limb were unaltered, but on the seventh both the redness and swelling were distinctly diminished.

By the end of the second week his appetite was improved and his pulse was 76; while there had not been a drop of discharge from beneath the crust, which had been still touched daily with carbolic acid, the fomentations also having been continued. The swelling, however, had not subsided, and the redness,

though varying in extent and degree, had never disappeared from over the seat of fracture. On the fifteenth day a defined prominence made its appearance at this part in a space about as large as the palm of the hand, a little further forward than the crust, and a sense of fluctuation was to be perceived in it. In the evening Dr. Cameron, on changing the fomentation, saw more pus than he thought could be accounted for by the superficial excoriation round the crust, and next morning, on removing the flannel, I found it soaked with similar discharge; a considerable quantity also lying between the tin splint and the limb. On raising the tin cap, the matter was seen welling out from beneath the lower edge of the crust. It was perfectly free from odour, confirming the conclusion I had previously arrived at that this abscess was not in any way caused by decomposition from atmospheric influence. The long period that elapsed before it made its appearance, together with the absence of any serious constitutional disturbance, clearly showed that the carbolic acid had effectually answered the purpose for which it was applied, the constant oozing of blood from the small wound having doubtless been in the patient's favour, by preventing decomposition from penetrating far into the interior before he came under treatment. We know that a mass of extravasated blood occasionally becomes the seat of suppuration without the existence of any external wound. A curious instance of this occurred lately in my practice, in a boy who fell down the hold of a ship upon his head, and, besides serious cerebral symptoms, exhibited at once a remarkable prominence of the right eyeball, evidently due to extravasation of blood into the orbit. There being no wound, I expected that the blood would be absorbed; but after the lapse of several days, the prominence of the eye showed increase rather than diminution, and the boy began to complain of supraorbital pain. Fluctuation then became perceptible, and pus was evacuated by incision, after which the eyeball gradually resumed its natural position.

Such I suppose to be the nature of the abscess in C.'s case, and previous experience made me fear that, if decomposition of its contents should occur, the irritation of the fetid pus might cause very serious consequences from rapid extension of suppuration among the imperfect and feeble products of the organization of the blood in the yet swollen limb.

Hence I had intended to evacuate the matter by aid of carbolic acid in such a way as to prevent decomposition. As the abscess was not near the surface at the part where it appeared to be pointing, I had reckoned on having plenty of time for my operations, and was greatly disappointed to find that it had discharged itself spontaneously.

Nevertheless, as the pus was proceeding from beneath the crust impregnated with carbolic acid and was still quite odourless, I did not altogether despair of attaining my object. In order to make the crust more effectual, I extended it for about three-quarters of an inch at the part from which the pus was escaping, by a piece of lint dipped in carbolic acid, which, when mixed with pus, forms a sort of curdy mass which answered pretty well for a crust. A considerable quantity of matter, of moderate consistence and greenish-white colour, was then pressed out from the limb. A new tin cap having been made, large enough to cover the whole of the extended crust, the fomenation was continued as usual.

Next day it was evident, from the sense of fluctuation, that reaccumulation had occurred in the abscess, but no further discharge had taken place. On removing the tin cap, however, pus was seen to well out from a new situation at the upper edge of the crust. A piece of lint dipped in carbolic acid was at once placed on this part, and the matter was pressed out and carefully collected measuring three ounces, of moderate consistence and yellowish-white colour, still without odour except that of carbolic acid. The crust having been somewhat extended at the situation of the new opening, the whole was freely treated with carbolic acid, the tin cap readjusted, and fomentation continued.

During the rest of the week that followed the first evacuation of the abscess the same treatment was pursued with the most satisfactory results. Some pus was usually seen on the fomenting flannel both morning and evening, and some was pressed out of the limb from the orifice last formed, but the amount rapidly diminished in quantity, and also became thinner and more transparent, while it continued free from odour. It may be worth while to mention in detail the quantities obtained from the limb in the morning of each of these days. On the seventeenth day it was an ounce and a half, somewhat thinner than before; on the eighteenth, two drachms and a half, de-

cidedly thinner; on the nineteenth, half a drachm, much thinner and more transparent; on the twentieth, a quarter of a drachm, similar in quality; and on the twenty-first, six drops only, and almost free from opacity. Finally, in the evening of that day no discharge was seen on the flannel, nor could any be squeezed out from the limb. Meanwhile the calf, which had increased markedly in circumference just before the abscess opened, steadily diminished, and in the thigh all swelling disappeared from over the seat of fracture, so that the end of the upper fragment, previously quite obscured, could be distinctly defined. His general health, too, had improved; his tongue had become quite clean, and he had acquired for the first time since his admission a genuine appetite, the pulse continuing about 72.

I suspect, however, that this success made us relax a little our vigilant care in guarding against decomposition. But be this as it may, the method which we pursued in order to avoid it was not, as experience has since shown, thoroughly trustworthy. Would that I had at that time known of the mode of proceeding which will be found described in a future section of this communication. Very different then might have been the issue of the case!

On the twenty-second day pus was again found in the flannel, and some bubbles of gas were observed to escape along with the two or three drops that could be squeezed from the limb, and these had a distinctly offensive odour. Judging it now useless to retain the crust any longer, I removed it, and found the original wound still sealed by the original clot, the openings by which the pus had escaped being new apertures in the skin overlapped by the crust. In the after-part of the day he had a good deal of uneasiness, and in the evening half an ounce of pus, with numerous air-bubbles, was pressed out of the limb by Dr. Cameron. After this the patient passed a comfortable night, and in the morning only two drachms of matter could be procured from the thigh, but this was thicker and more opaque than it had been, with decidedly offensive odour, and contained bubbles of gas; there was also pus in the flannel. There was, further, some return of swelling over the seat of fracture.

But though the plan of dealing with the abscess had failed to accomplish all that I desired, its essential object appeared to

have been attained. For during the week in which decomposition was prevented, the thigh had become so much consolidated and strengthened that all danger of serious consequences seemed to have been tided over. No extension of the suppuration took place beyond the trifling degree above described, and his constitution did not suffer. Any further use of carbolic acid being obviously uncalled for, the sore was simply dressed with a lotion, the lint being so arranged as to allow free escape for the pus, and afterwards, to promote this more effectually, a small perforated caoutchouc tube was introduced, a dry cloth being substituted for the fomentation. Under this management the discharge gradually diminished in quantity, and became again thinner and more transparent, and the swelling of the calf became steadily reduced.

Still the opening did not close, and on the 2nd of December, more than a fortnight having passed in this way, I introduced a probe, and found that it passed downwards to bare bone, including a considerable extent of surface in the lower fragment. Here, then, was presented the prospect of a tedious process of exfoliation; whereas if decomposition of the pus had not occurred, the granulations would probably have closed upon the dead bone, and absorbed it, as in the last case, and the fact that any part had lost its vitality would then never have been known. That there is a reasonable ground for this belief will, I trust, appear from the discussion in the succeeding section.

For a long time the progress of the patient continued satisfactory, the process of union of the fragments advancing steadily, till in the early part of February, the bone being firm, the splints were entirely discarded, and the case was reduced from one of fracture to one of limited exfoliation. It was satisfactory also to find that the knee-joint continued movable, so that I confidently anticipated recovery, with a perfectly useful limb.

At this period, however, a new symptom presented itself—viz., haemorrhage from the sinus. Mr. Hector Cameron, my present house surgeon, who saw the first appearance of bleeding, supposed it to proceed from the surface of the granulations; for it was then small in amount, and ceased spontaneously. Some days later, however—viz., on the 11th of February—a very profuse haemorrhage occurred, the blood soaking through the bed,

and dropping upon the floor beneath, before it was observed, and the gentleman who was summoned to see the patient in Mr. Cameron's absence, found him pulseless. He afterwards rallied to some extent, but remained utterly prostrated, and unable to retain the slightest nourishment. As the popliteal artery could be felt beating in the lower part of the ham, I hoped that the source of the blood might be some minor branch, which might possibly close. But it afterwards appeared that a circular opening existed in the main vessel, occasioned no doubt by the pressure of an irregular projection of the lower fragment. It would be irrelevant to relate particularly the history of his yet further exhaustion by recurrent haemorrhages after delusive temporary cessations, or of my attempts to restore him by tying the popliteal artery, and making arrangements for transfusion, to which he declined to submit. He died on the 25th of February.

The next four cases occurred in the practice of my colleagues in the infirmary, who have kindly placed them at my disposal.

CASE 7.—Mary M——, aged 62, was admitted under the care of Dr. Morton on the 13th of August, 1866, at 11 p.m., when she stated that about 5 o'clock in the afternoon of that day she missed her footing when going downstairs, and fell with violence, and on getting up found that her right forearm was broken and bleeding. A medical man was called in, who made various applications in order to stop the haemorrhage, but failed to do so, and she was advised to go to the infirmary. Mr. A. T. Thomson, the house surgeon (to whom I am indebted for notes of the case), on removing the bandage, from which blood was trickling, found both bones of the forearm broken a little above the wrist, and a detached fragment of the radius projecting from a wound about as large as a fourpenny-piece, on the outer aspect of the limb. Having extracted this fragment, he applied liquid carbolic acid thoroughly to the interior of the wound. This rather increased the bleeding, which, however, he arrested completely by plugging the orifice with a bit of lint dipped in the acid. Over this he placed a mixture of blood and carbolic acid, covering it with a piece of dry lint. He then put up the limb in two well-padded Gooch's splints, retained in position with a continuous bandage. The apparatus was left undisturbed for five days, when, on removal of the splints, it was found that the

piece of dry lint over the wound, though it had been saturated with blood, was quite dry, having become incorporated with the crust beneath. It was not interfered with except that the surface was touched with carbolic acid, and the splints were reapplied as before, the part being quite free from uneasiness.

On the twelfth day the splints were again removed and the crust was detached, when it was found that the piece of lint with which the wound had been plugged had become partly pushed out of the orifice. The plug also was now removed, when the surface beneath was observed to be granulating, but entirely free from pus. The sore was dressed with one part of carbolic acid to seven parts of olive oil applied on lint every second day, the use of the splints being continued till the 8th of September, when she was discharged, with the sore healed and both bones firmly united, two days less than four weeks after the accident.

This case is valuable as an example of a mode in which troublesome bleeding in compound fracture may sometimes be advantageously arrested. The entire absence of pus about the plug on the twelfth day after its introduction contrasts strikingly with the suppuration invariably caused within four days by a piece of lint inserted without carbolic acid into a wound.

CASE 8.—Samuel B——, aged 13, was admitted under Dr. Morton's case, on the 30th of August, 1866, with a compound fracture of the left femur, about the junction of the upper and middle thirds of the shaft, and a simple fracture of the right thigh in a similar situation. He stated that about four hours previously he was engaged in some work about a steam-engine, when he was struck by one of the balls of the "governor," and hurled with great force against an iron pillar. The men who brought him to the infirmary said that when he was raised from the ground a piece of bone was seen to protrude from a wound in the left thigh, but was restored to its natural position by a medical man who was called in to see him, and who applied a long splint and bandage to each limb. Mr. A. T. Thomson, on examining the boy, found a lacerated wound about three inches long at the upper part of the left thigh, running transversely from the middle of the inner side of the limb to its posterior aspect, and in this wound the upper fragment of the femur was visible, somewhat displaced, but not protruding. There was some bleed-

ing, but not to any serious extent. He sponged out the wound thoroughly with a solution of one part of carbolic acid in three parts of olive oil, and then covered its lips with a mixture of blood and the undiluted acid spread upon lint, and over this a piece of sheet-tin, retained in position by means of a looped bandage. He next corrected the faulty position of the fragments and applied lateral splints of Gooch's material to the thigh, maintaining gentle extension by means of plasters applied to the integument of the leg after the American plan, and fixed to the foot of the bed, a perineal band being attached to the bed-head. While the left limb was thus kept readily accessible for changing the dressings, the long splint was employed as usual for the simple fracture on the right side.

Next day the surface of the crust was touched with carbolic acid, and a hot fomentation applied to the limb.

On the third day the crust was removed through a misunderstanding, but it was resolved to follow out the treatment on the same principle, and with this view the wound was dressed twice a day with lint dipped in the mixture of carbolic acid and oil (one part to three), covered with the tin, as the crust had been before, while the fomentations also were continued. Meanwhile the limb remained free from pain, redness, or swelling, and there was a complete absence of constitutional disturbance.

On the sixth day, however, he was a little feverish, and remained so, though without any apparent local symptoms, till the twelfth day, when Mr. Thomson noticed that the central part of the wound, which had become covered with a whitish crust, was somewhat prominent, and, on careful examination, perceived a distinct sense of fluctuation. He therefore removed the white layer from that part, when eight ounces of perfectly odourless pus escaped. A probe introduced failed to detect any bare bone. Mr. Thomson now sponged out the cavity of the abscess with the mixture of carbolic acid and oil, and left in it a strip of lint dipped in the same, continuing the other dressings as before. The constitutional disturbance now at once subsided, and under the same dressing the cavity of the abscess quickly contracted, and in a little more than a fortnight closed entirely. Six weeks after the accident the wound was completely healed, and both the thigh-bones were firmly united, with the limbs of equal length. In another week he was able to stand.

This case, which, I cannot avoid remarking, reflects great credit on the house surgeon in charge, is interesting as another instance of the occurrence of abscess in compound fracture, independently of atmospheric influence. That it was so in this instance is clearly shown by the entire absence of constitutional symptoms for the first five days, the circumscribed character of the suppuration, and the odourless nature of the pus. The injured part suppurated, probably from the same cause as a severe bruise may without any breach of the integument. The satisfactory results obtained by treating the wound with carbolic acid diluted with oil, instead of the undiluted acid, will naturally suggest the inquiry whether this would not always be the better practice. And I may mention that my former house surgeon, Dr. A. Cameron, met with similar success in two cases in which he pursued the same treatment—one of them a compound fracture of the ulna at the elbow, the other a severe contused wound of the back of the hand communicating with a fractured metacarpal bone. But considering how much is at stake, and that the patient's life may depend upon entire destruction of the septic germs that lie in the wound, I am inclined to think it wiser to avail ourselves of the full energy of the pure acid, more especially since we have had sufficient evidence that any caustic effects it may have are not productive of serious consequences.

CASE 9.—William C——, aged 33, was admitted on the 29th of September, 1866, under the care of Dr. Eben Watson, with a compound fracture of the left tibia, produced by an omnibus passing over the limb at 8 o'clock p.m. The broken part of the bone was exposed in a wound six and a half inches in length, a little above the ankle. The skin in the vicinity was detached from the subjacent tissues for about two inches, and there was ecchymosis reaching some distance up the leg, with other evidence of severe contusion.

An hour and a half after the accident Dr. A. Forsyth, the house surgeon, from whose notes these particulars are obtained, sponged out the wound thoroughly with undiluted carbolic acid, and placed over it layers of calico soaked with the acid; and, in order to provide for a sufficiently substantial crust, spread over the calico some paste composed of starch moistened with carbolic acid, covering the whole with a piece of block-tin secured with

a bandage, the fracture being treated with a suitable apparatus. After the dressing, the patient, though unable to express his feelings, being dumb, appeared entirely free from uneasiness.

Next day the tin was carefully removed from the crust, the surface of which was touched with carbolic acid, and, the tin having been readjusted, hot fomentations were applied to the leg and foot. The pulse was now 96, the tongue clean, and appetite good. The same treatment was pursued till the thirteenth day, when the fomentations were discontinued, and the edges of the crust which were loose were clipped away, and lint moistened with water was applied to the granulating surface thus exposed, the remainder of the crust being still touched daily with carbolic acid. Meanwhile there had been no suppuration beneath the crust, and the patient had remained free from constitutional symptoms.

On the seventeenth day the crust, which had separated from the wound at its lower third, was removed, disclosing a healthy granulating surface, the bone being nowhere visible, while there was no appearance of pus, except a trifling amount towards the lower part. The sore, which was entirely superficial, was now treated like an ordinary ulcer, and healed quickly. The bone also united as in a simple fracture, and he was discharged eight weeks after the receipt of the injury, having been kept longer in the hospital than would otherwise have been necessary, on account of a head affection to which he was subject.

The above case, besides being a good example of the effects of the treatment of compound fracture with carbolic acid, affords an illustration of a practice which I have on several occasions found useful when there has been but little bleeding from the wound, a dough or paste composed of flour or starch, moistened with the acid, being employed in lieu of the compound with blood to render the crust sufficiently substantial.

CASE 10.—Thomas M'B——, a labourer, who gave his age as 52, but had the appearance of a much older person, was admitted at noon on the 2nd of January, 1867, under the care of Dr. G. Buchanan, having been knocked down an hour before by the shaft of a luggage wagon, the wheel of which passed over his left leg, producing a compound fracture in the lower third of the limb. Mr. James Robinson, the house surgeon, who has given me

notes of the case, found a wound from which blood was oozing, about an inch and a half in length, exposing part of the tibia, and communicating with the seat of fracture. The tissues were pretty severely contused. Undiluted carbolic acid was applied freely to the interior of the wound by means of lint held in a pair of dressing-forceps, and a crust was formed of blood mingled with the acid, covered with lint, over which a cap of tin was placed, well bulged out to correspond to the substantial crust, and large enough to overlap to a slight extent the sound skin in the vicinity. The fragments having been brought into proper position, the limb was put up with lateral wooden splints, with a hot fomentation. At the conclusion of the dressing the patient expressed himself as greatly relieved. The pulse was then 65.

Next day he was free from pain after a fair night's rest. The pulse was 74, and the tongue clean and moist. The surface of the crust was touched with carbolic acid, the limb being still fomented; and the same treatment was continued daily for the following fortnight, during which the limb was entirely free from pain, redness, or suppuration, while his constitution was quite unaffected by the injury, the tongue remaining clean, and the pulse varying only between 72 and 85.

I was present when the crust was removed, eighteen days after the accident. Not a drop of pus existed beneath it. On the contrary, the superficial sloughs of the cutis occasioned by the caustic action of the acid first applied remained still undetached. The exposed surface was treated with water dressing, and in two days presented the appearance of an ordinary granulating sore, which healed without interruption. Six weeks and three days after the receipt of the injury the splints were removed, the bones being satisfactorily united.

This is an excellent example of the effects of the carbolic-acid treatment in a compound fracture of the leg of average severity. No simple fracture could have caused less disturbance, either local or constitutional.

CASE 11.—The following case, though incomplete, is given on account of the conclusive evidence it affords regarding a complication of compound fracture of much interest both practically and theoretically—viz., emphysema of the limb in conse-

quence of air being introduced into the wound, and diffused among the interstices of the tissues by a pumping action of the fragments of the broken bone when freely moved through restlessness of the patient or carelessness of his attendants before he comes under the surgeon's care. Such a state of things may seem at first sight to render it impossible to prevent decomposition of the extravasated blood, since it would be out of the question to attempt to apply carbolic acid to all the emphysematous tissues. But I have long indulged the hope that, the air entering in small successive portions, its floating organisms might be arrested by the first blood with which they came in contact, and remain for some time confined to the vicinity of the external wound, in which case, by squeezing out as much blood as possible from the orifice in the integument, and introducing carbolic acid freely, we might get rid of all causes of decomposition in the limb, the mere atmospheric gases diffused more remotely, however abundant, being entirely innocuous. This hope, it now appears, was not ill-founded.

John D——, aged 55, a calico-printer, of intemperate habits, was admitted under my care in the Royal Infirmary at 6 p.m. on the 4th of April, 1867, having broken both bones of his right leg about an hour before by jumping out of a window into the street, from a height of between fifteen and twenty feet, while in a state of intoxication. He was carried upstairs to his lodgings, kicking about in his drunken frenzy. A cloth was then put round the leg, but no efficient means were employed to steady it, and he was conveyed to the hospital from a distant part of the city in a cab, moving the limb recklessly during the whole journey. His friends stated that he had lost a great deal of blood, and the cloth which was round the limb on his admission was saturated. Mr. H. Cameron, the house surgeon, found a wound about half an inch in length, situated over the spine of the tibia, at the junction of the middle and lower thirds of the bone, the fracture being half an inch lower down, and obviously communicating. The wound was bleeding very freely, and the leg was considerably swollen through extravasation of blood into it. On manipulation, Mr. Cameron found the tissues about the seat of fracture emphysematous, the characteristic crackling sensation being experienced fully four inches above the wound and two inches below it, and also on the opposite

side of the limb, over the fibula; and as a result of the handling, a frothy mixture of blood and air, in larger and smaller bubbles, escaped from the orifice. The fragments were much displaced, the foot being greatly everted.

Mr. Cameron, having squeezed out as much blood as possible from the wound, introduced melted crystallized carbolic acid in a piece of calico held in dressing-forceps, which he passed in various directions for more than two inches beneath the integument and about an inch and a half among the deeper structures of the limb, using three different pieces of calico soaked with the acid, and leaving the last in the wound as a plug to check the very free hæmorrhage, which the treatment had considerably increased. He then applied several layers of calico steeped in carbolic acid and smeared with blood, so as to make a pretty thick crust overlapping the skin by about half an inch, and adapted to the crust a cap of block-tin of slightly larger dimensions, pressing it down upon the skin by means of a looped bandage encircling the leg. Having next corrected the displacement of the fragments, he moulded a pasteboard splint to the outer side of the leg and foot, strengthening it with a temporary Gooch's splint, and laid the limb on its outer side upon a pillow with the knee bent. The patient now stated that the pain he had suffered was greatly relieved. His pulse was 100. Two hours later, as a good deal of oozing of blood was still going on, a folded cloth was placed upon the tin cap and pressed down upon it with a bandage. The limb meanwhile was considerably more swollen, from bleeding into its interior, kept up, no doubt, by the sudden jerking movements which in his unreasoning condition he could not be prevented from making. The pressure employed greatly diminished the external haemorrhage, but did not entirely arrest it; and when two hours more had elapsed Mr. Cameron asked my advice. I recommended the use of a well-fitting internal splint, to procure greater steadiness of the fragments, and so get rid of the irritation which perpetuated the bleeding. Mr. Cameron, however, on removing the compress, found that all tendency to oozing of blood had ceased. The patient was now sober, but continued very restless. The internal splint was therefore applied, and thirty drops of solution of muriate of morphia were administered.

During the night he suffered a good deal, and got no sleep

at all. Next morning, however, he complained rather of a general sense of weariness and sickness, the consequences of his debauch, than of pain; the pulse had fallen to 76; and he took his breakfast pretty well. The surface of the crust was touched with carbolic acid, and this was repeated in the afternoon, when a hot fomentation was applied to the inner side of the leg, and over this a sheet of stout block-tin, to serve, as in some previous cases, the double purpose of ensuring the efficiency of the fomentations, and acting as an internal splint. The limb was now quite easy. At night the pulse was still 76. He had made a pretty hearty supper, and felt only occasional twinges in the limb. The fomentation was changed, and the crust again touched with carbolic acid, and the opiate repeated.

He passed the following night like the preceding, without getting any sleep whatever; and in the morning his pulse was 90, although the limb was free from pain or inflammatory blush, and he made a hearty breakfast. Fearing the approach of traumatic delirium, I ordered a larger opiate to be given at night. Fifty drops of the morphia solution were accordingly administered; and after this dose he slept for about five hours. Nevertheless, he grew more restless, and was found in the morning with the leg fully extended and resting on the calf instead of on its outer side. His pulse continued at 90; and although the state of the limb and his appetite were all that could be wished, he exhibited in the afternoon unmistakable signs of delirium tremens, jerking out his tongue when asked to show it, twitching his hands in an excited manner, and declaring that his bedclothes were creeping away from him, while the restless movements of the limb were continued. I ordered a dose of castor oil, to be followed, as soon as it should have operated, by a drachm of the solution of muriate of morphia, to be repeated if necessary. He took the opiate about eight o'clock p.m., and soon afterwards dozed a little; and at eleven his pulse had fallen to 82. After this he fell into a sound sleep, from which he did not wake till 6 a.m.; and from this time forth he was perfectly tranquil and rational.

It is needless to enter into particulars regarding his subsequent progress further than to say that it has been in all respects satisfactory; and on the tenth day after the accident, when I saw him last, his pulse was 76, his appetite excellent, and he had

the appearance of a man in perfect health. The limb was still free from pain, while the swelling due to extravasation of blood had disappeared, and the skin was of natural aspect. After the second day from the accident, there had not been even any discharge of serum from beneath the crust, which had been daily touched with carbolic acid, the fomentations being also continued, as he found them comfortable.

I need not hesitate to say that all danger in this case is over; and that the compound fracture is already converted into a simple one under circumstances which, even for a simple fracture, would have been trying.

In revising the proof, after nine days more have elapsed, I may add that all has continued to go on well.

PRELIMINARY NOTICE ON ABSCESS.

I will now give a description of a new method of treating abscess, which has afforded results so satisfactory that it does not seem right to withhold it longer from the profession generally.

It is based, like the treatment of compound fracture, on the antiseptic principle, and the material employed is essentially the same—namely, carbolic acid, but differently applied in accordance with the difference of the circumstances. In compound fracture there is an irregular wound, which has probably been exposed to the air for hours before it is seen by the surgeon, and may therefore contain in its interstices the atmospheric germs which are the causes of decomposition, and these must be destroyed by the energetic application of the antiseptic agent. In an unopened abscess, on the other hand, as a general rule, no septic organisms are present, so that it is not necessary to introduce the carbolic acid into the interior. Here the essential object is to guard against the introduction of living particles from without, at the same time that a free exit is afforded for the constant discharge of the contents. The mode in which this is accomplished is as follows :—

A solution of one part of crystallized carbolic acid in four parts of boiled linseed oil having been prepared, a piece of rag from four to six inches square is dipped in the oily mixture, and laid upon the skin where the incision is to be made. The lower

edge of the rag being then raised, while the upper edge is kept from slipping by an assistant, a common scalpel or bistoury dipped in the oil is plunged into the cavity of the abscess, and an opening about three-quarters of an inch in length is made, and the instant the knife is withdrawn the rag is dropped upon the skin as an antiseptic curtain, beneath which the pus flows out into a vessel placed to receive it. The cavity of the abscess is firmly pressed so as to force out all existing pus as nearly as may be (the old fear of doing mischief by rough treatment of the pyogenic membrane being quite ill-founded); and if there be much oozing of blood, or if there be a considerable thickness of parts between the abscess and the surface, a piece of lint dipped in the antiseptic oil is introduced into the incision to check bleeding and prevent primary adhesion, which is otherwise very apt to occur. The introduction of the lint is effected as rapidly as may be, and under the protection of the antiseptic rag. Thus the evacuation of the original contents is accomplished with perfect security against the introduction of living germs. This, however, would be of no avail unless an antiseptic dressing could be applied that would effectually prevent the decomposition of the stream of pus constantly flowing out beneath it. After numerous disappointments, I have succeeded with the following, which may be relied upon as absolutely trustworthy. About six teaspoonfuls of the above-mentioned solution of carbolic acid in linseed oil are mixed up with common whitening (carbonate of lime) to the consistence of a firm paste, which is in fact glazier's putty with the addition of a little carbolic acid. This is spread upon a piece of sheet block-tin about six inches square; or common tinfoil will answer equally well if strengthened with adhesive plaster to prevent it from tearing, and in some situations it is preferable, from its adapting itself more readily to the shape of the part affected. The putty forms a layer about a quarter of an inch thick; it may be spread with a table-knife, or pressed out with the hand, a towel being temporarily interposed to prevent the putty from sticking to the hand or soiling the coat-sleeve. The tin thus spread with putty is placed upon the skin so that the middle of it corresponds to the position of the incision, the antiseptic rag used in opening the abscess being removed the instant before. The tin is then fixed securely by adhesive plaster, the lowest edge being left free

for the escape of the discharge into a folded towel placed over it and secured by a bandage. This dressing has the following advantages: The tin prevents the evaporation of the carbolic acid, which escapes readily through any organic tissue such as oiled silk or gutta-percha. The putty contains the carbolic acid just sufficiently diluted to prevent its excoriating the skin, while its substance serves as a reservoir of the acid during the intervals between the dressings. Its oily nature and tenacity prevent it from being washed away by the discharge, which all oozes out beneath it as fast as it escapes from the incision; while the extent of the surface of the putty renders it securely antiseptic. Lastly, the putty is a cleanly application, and gives the surgeon very little trouble; a supply being daily made by some convalescent in a hospital, or in private practice by the nurse or a friend of the patient; or a larger quantity may be made at once, and kept in a tin canister. The dressing is changed, as a general rule, once in twenty-four hours; but if the abscess be a very large one, it is prudent to see the patient twelve hours after it has been opened, when, if the towel should be much stained with discharge, the dressing should be changed, to avoid subjecting its antiseptic virtues to too severe a test. But after the first twenty-four hours, a single daily dressing is sufficient. The changing of the dressing must be methodically done, as follows: A second similar piece of tin having been spread with the putty, a piece of rag is dipped in the oily solution, and placed on the incision the moment the first tin is removed. This guards against the possibility of mischief occurring during the cleansing of the skin with a dry cloth and pressing out any discharge which may exist in the cavity. If a plug of lint was introduced when the abscess was opened, it is removed under cover of the antiseptic rag, which is taken off at the moment when the new tin is to be applied. The same process is continued daily till the sinus closes.

The results of this treatment are such as correct pathological knowledge might have enabled us to predict. The pyogenic membrane has no innate disposition to form pus, but does so only because it is subjected to some preternatural stimulus. In an ordinary abscess, whethter acute or chronic, the original cause that led to suppuration is no longer in operation, and the stimulus that determines the continued pus-formation is derived

from the presence of the pus pent up in the interior. When an abscess is opened in the ordinary way this cause of stimulation is removed, but in its place is substituted the potent stimulus of decomposition. If, however, the abscess be opened antiseptically, the pyogenic membrane, freed from the operation of the previous stimulus without the substitution of a new one, ought, according to theory, to cease to suppurate, while the patient should be relieved from any local or general disturbance caused by the abscess, without the risk of irritative fever or hectic.

Such, accordingly, is the fact. Abscesses of large size have, after the original contents have been evacuated, furnished no further pus whatever, the discharge being merely serum, which in a few days has amounted only to a few drops in the twenty-four hours. Whether the opening be dependent or not is a matter of perfect indifference, the small amount of unirritating fluid being all evacuated spontaneously by the rapidly contracting pyogenic membrane. At the same time, we reckon with perfect certainty on the absence of all constitutional disturbance.

As an illustration, I may mention the last case which has come under my care. It is that of a young woman, twenty-five years old, with psoas abscess, which had of late been rapidly on the increase, and caused a large swelling below Poupart's ligament, communicating with a fluctuating mass, dull on percussion, reaching to a considerable distance up the abdomen, the femoral vessels being raised over the communication between them. Six days ago I opened, in the manner above described, the swelling in the thigh at the anterior part of the limb where it was nearest the surface, giving exit to twenty-seven ounces of pus, thin, but containing numerous large curdy masses. I introduced a piece of lint, dipped in the carbolic acid and oil, into the incision; and this prevented any discharge from escaping during the next twenty-four hours, when, on removal of the plug of lint under an antiseptic rag, three ounces of turbid serum escaped. For the next three days there was scarcely any discharge, the deeper parts of the incision having cohered. On firm pressure, however, the product of seventy-two hours escaped, and amounted to four drachms of serum. Meanwhile the girl's general health, which had not been interfered with by the abscess, continued perfectly good, neither pulse, tongue, appetite, nor sleep having been disturbed.

In this case, though there is no deformity of the spine, there is great probability that caries of the vertebræ is present. But even though such be the case, there is good reason to hope for a favourable issue. Regarding caries as merely the suppurative stage of chronic inflammation in a weak form of tissue, I have been not surprised, though greatly rejoiced, to find that it exhibits the tendency of inflammatory affections generally—viz., a disposition to spontaneous cure on the withdrawal of irritation. Hitherto, in surgical practice, caries has had to contend against the formidable irritation of decomposing matter, which, under circumstances of weakness, is often sufficient to cause ulceration, even in the soft parts; yet, in spite of this irritation, caries is often recoverable in the child where the vital powers of all the tissues are stronger. If, therefore, this serious complication can be avoided, there seems nothing in theory against the probability that caries may prove curable in the adult. And even should portions of necrosed bone be present, as is not infrequently the case, our experience of the treatment of compound fracture with carbolic acid has taught us that dead bone, if undecomposed, not only fails to induce suppuration in its vicinity, but is liable to absorption by the granulations around it.[1]

Such were the hopes which I ventured to express several months ago to my winter class. Since that time I have opened numerous abscesses connected with caries of the vertebræ, the hip, knee, ankle and elbow, and in all cases I have found the discharge become in a few days trifling in amount, and in many it has ceased to be puriform after the first twenty-four hours. Finally, three days ago—viz., on the 4th inst. (July, 1867)—I had the inexpressible happiness of finding the sinus soundly closed in a middle-aged man, in whom I opened in February last a psoas abscess, proved to be connected with diseased bone by the discharge, on one occasion, of an osseous spiculum. For months past we had persevered with the antiseptic dressing, although the discharge did not amount to more than a drop or two of serum in the twenty-four hours, well knowing by bitter experience that so long as a sinus existed the occurrence of decomposition might produce the most disastrous consequences; and at length our patience has been crowned with success.

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[1] See p. 110.

Hence I no longer feel any hesitation in recommending the early opening of such abscesses, because, while they remain unopened, the disease of the bone is necessarily progressive, whereas when opened antiseptically, there is good ground to hope for their steady, though tedious, recovery.

The putty of the strength above recommended, though it generally fails to excoriate the skin, sometimes produces this affect when long continued. In such case it may be reduced in strength so that the oil contains only one part to five or six without disadvantage when the discharge is very small in amount.

The application prevents the occurrence of cicatrization in the little sore caused by the incision, and perpetuates a trifling discharge from it. Hence it is impossible to judge whether or not the sinus has closed, except by examining it from time to time with a probe, which should be dipped in the antiseptic oil, and passed in between folds of the antiseptic rag. This may seem a refinement, but if we could see with the naked eye a few only of the septic organisms that people every cubic inch of the atmosphere of a hospital ward, we should rather wonder that the antiseptic treatment is ever successful than omit any precautions in conducting it.

The putty used in treating abscesses has proved very valuable in simplifying the treatment of compound fracture, and enlarging the range of its applicability, and also in dealing with incised wounds on the antiseptic principle. But I must defer a notice of these matters to a future occasion.

DEMONSTRATIONS OF ANTISEPTIC SURGERY BEFORE MEMBERS OF THE BRITISH MEDICAL ASSOCIATION.

Collected Papers, vol. ii, p. 256—*Edinburgh Medical Journal*, vol. xvi, 1875-6, pp. 193, 481.

We must now choose, for a place in this selection, one of the dozen or more papers published by Lister while he was Professor of Clinical Surgery in Edinburgh (1869-77), the most brilliant period of his career.

One of the least known, because it was only published in full in the *Edinburgh Medical Journal*,[1] is an account of two demonstrations given to the British Medical Association which met in Edinburgh in 1875. It will serve our purpose well, and it brings before us incidentally a stirring and important occasion: the large theatre in the old Royal Infirmary occupied by four or five hundred members, mostly general practitioners, but with a good sprinkling of surgeons, the former with open minds but sceptical, the latter for the most part either simply incredulous, or strongly opposed to Lister and all his works.

By this time his reputation was firmly established in Scotland, Scandinavia and Central Europe, but in England his influence was not great far south of the Tweed, and in London it had done little more than ruffle the stagnant pool of old-fashioned surgery.

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[1] *Edinburgh Medical Journal*, vol. xxi, 1875-6, pp. 193-481.

Four years before, in a long address to the British Medical Association at Plymouth,[1] he had described in detail his practice at that time. No great modifications had been made in the interval. He now illustrated by actual demonstration the facts which he had then described. Many of the audience saw things they had never seen before: bloodless surgery, organizing blood-clot, free incisions into tuberculous and chronically inflamed joints, an open operation on an ununited fracture of the neck of the femur, excision of varicose veins, and the ligature of arteries in their continuity and in wounds with catgut ligatures cut short. They saw, and were duly impressed, and eagerly flocked to the Physiological Theatre two days later to see another demonstration of Lister's beautiful methods of bacteriological research. In this way the theoretical and the practical aspects of the antiseptic principle were forced upon their attention.

The reader will not fail to notice the dread that Lister still entertained of atmospheric germs, and the confidence he placed in the spray, although his own experiments made with the object of refuting or confirming those of Burdon Sanderson,[2] had convinced him that "Various as are the organisms which float in the atmosphere, they constitute but a very small proportion of the abounding particles of dust which a beam of sunlight reveals in an occupied room."[3] I think this is the first time he expressed a sort of forlorn hope that the spray might some day be abandoned, but, in his own practice, it survived for another twelve years, six at least after it had been discarded by most German and many British surgeons.

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[1] *Collected Papers,* vol. ii, p. 173; *British Medical Journal,* 1871, vol. ii, p. 205.
[2] *Quarterly Journal of Microscopical Science,* vol. xi, 1871.
[3] *Collected Papers,* vol. i, p. 284.

DEMONSTRATIONS OF ANTISEPTIC SURGERY BEFORE MEMBERS OF THE BRITISH MEDICAL ASSOCIATION.

DEMONSTRATION I.

GENTLEMEN,—I propose this morning and to-morrow morning to avail myself of such opportunities as happen to be at my disposal to illustrate before the British Medical Association the methods and the value of antiseptic treatment. The first case which I shall bring before you will show this treatment in its simplest form, and in one of its most striking instances—a case in which I propose to lay open the knee-joint. The patient (a man, 54 years of age) was under my care some years ago, with a very large effusion under the deltoid, in an acute form, attended with much fever. I opened it antiseptically, and the patient made a rapid recovery without suppuration. He has thus already had experience of the value of antiseptic treatment, and therefore trusts it implicitly for the management of what he at present suffers from, namely, painful effusion into the knee-joint. It is of twelve months' duration, and has resisted repeated blistering (about a dozen have been applied in all), and from the peculiar prominence that exists over parts of the articulation, I suspect suppuration is imminent. Now, if blistering failed in a case of this kind, without antiseptic management the surgeon would be be at a loss what to do. Dieulafoy's aspirator might sometimes prove serviceable, but those who have tried it must confess that they are often disappointed in consequence of the fine tube becoming blocked by portions of lymph. But by antiseptic means we are able to obtain, by incision and drainage-tube, a perfectly free exit for the fluid, and thus relieving the joint altogether from the tension due to effusion, permit the natural tendency to recovery to come into operation. I need hardly remark, that to do this without antiseptic treatment would be madness—

would be a thing which no surgeon would be justified in doing; to make a free incision into the knee-joint and to keep the wound open with a drainage-tube, would be an altogether unwarrantable procedure. We all know that the knee-joint has often been opened by free incision for the extraction of loose cartilages, and that in some such cases, the wound having healed by first intention, all has gone on well without any antiseptic treatment at all; though we know also that this is a very uncertain and dangerous practice. But though it is true that wounds of joints, whether accidental or intentional, may heal without disturbance under ordinary treatment, yet it is certain, that if such wounds were kept open without antiseptic means, disastrous consequences would be inevitable; by keeping the wound open we should take away the only chance there would be, without antiseptic treatment, of the case ending without disaster. But, Gentlemen, paradoxical as it may at first appear, with antiseptic treatment the more free the wound, and the more widely it gapes, the more certain you are to avoid inflammatory disturbance in the joint; simply for this reason, that you are the more certain of a free discharge of the plasma effused into the interior. And if you avoid all tension from this cause, and at the same time exclude putrefactive mischief, you have the joint left absolutely free from irritation. Before we bring the patient in, I may say that I shall make the incision pretty free as regards the skin, and carry it gradually down to the joint, so as to be able to see and secure any small artery that may be divided. For if you simply plunge the knife into the joint, and put in a drainage-tube, bleeding may take place into the articulation from some deep vessel, and lead to considerable inconvenience. Just as in Professor Andrew Buchanan's well-known experiment, hydrocele fluid is made to coagulate by the addition of a little serum from a blood-clot, so if a very little blood finds its way into the knee-joint, the liquor sanguinis effused from the synovial surface mixing with the globulin of the red corpuscles forms a coagulable fluid and undergoes coagulation, and you have the knee-joint filled with solid matter, which interferes with the rapidity of recovery, although in due time the accumulation disappears by absorption.

[The patient being now brought in, Mr. Lister proceeded]—Here, then, we have before us the distended knee-joint. You observe this peculiar limited special bulging, which, together

with the history, makes me suspect that the joint is on the eve of suppuration.

I have said that this case will be an example of the antiseptic treatment in its simplest form. The antiseptic will not be introduced into the joint; it will not be applied to the affected part at all. It will be merely employed externally to prevent the access of septic mischief while we provide exit for fluid from the interior. We shall first purify the skin with a strong (1 to 20) watery solution of carbolic acid, which is best for detergent purposes; water holding carbolic acid but slightly, and very readily giving it up to act upon anything else. Carbolic acid has a remarkable penetrating property. It blends with oily substances and animal matters, and penetrates the hair and hair-follicles, and therefore such a washing as I am now giving will render the skin absolutely pure, surgically speaking. This is a very great point.

In the next place, we shall have an antiseptic atmosphere provided by means of this spray-producer, which acts on the principle of Siegle's steam inhaler. High-pressure steam, issuing by a minute orifice from a boiler heated by spirit-lamp or gas, sucks up a strong solution of carbolic acid by a tube that dips into a vessel containing it, and, blending with it in about equal quantity, forms a 1 to 40 spray. We have lately very much improved our spray by a slight alteration of the apparatus. We used to have the tube which conveys the carbolic solution perpendicular to that for the steam, just as the air-tube is at right-angles with the water-tube in the common atmospheric odorator; and the result was a coarse spray with scattering drops, consuming a needless quantity of the solution, and causing needless irritation of the surgeon's hands and wetting of his sleeves; and, what was of more moment, inducing unnecessary irritation of the wound, and making around the trustworthy spray an area of uncertain extent completely valueless, because the solution in it was in the form of comparatively large drops with intervals of unaltered air. But, by placing the tube for the solution at an angle of 45° with that for the steam, and with its point ground off obliquely so as to be exactly in the axis of the steam-tube, we get a spray destitute of scattering drops, perfectly trustworthy throughout its visible extent, though little coarser than a London fog.

The slate on which I am now directing the spray is in an antiseptic atmosphere; yet so fine is the spray, that it scarcely moistens the surface. The face of one of my dressers is now enveloped by the cloud, which, as you observe, is capable of being inhaled without serious inconvenience. That we should be able to provide a respirable, yet reliably antiseptic atmosphere, is what I confess I never anticipated. The boiler has a safety-valve to prevent explosion, and a window to enable you to see when the water is becoming exhausted. A large spray-producer like this will go on working, with one supply of water in the boiler, for a couple of hours.

The part to be operated upon, then, being in an antiseptic atmosphere, if the finger is to be introduced into the wound (and I shall very likely have to pass my finger into the joint) you must take special care that it is an aseptic finger; and this is done by cleansing it with an antiseptic solution, making sure that it passes well into the folds of skin about the nail. And if I should have to introduce an instrument into the articulation, I must see that it is always pure when inserted. In order, Gentlemen, that you may get satisfactory results with this sort of treatment, you must be able to see with your mental eye the septic ferments as distinctly as we see flies or other insects with the corporeal eye. If you can really see them in this distinct way with your intellectual eye, you can be properly on your guard against them; if you do not so see them, you will be constantly liable to relax in your precautions. I have seen, for instance, a gentleman, anxious to carry out the antiseptic treatment completely, take out a large loose cartilage from the knee-joint under the spray, using at the outset instruments which had been purified by lying in a solution of carbolic acid; but in the course of the operation, I observed him take a pair of forceps which seemed better adapted for his purpose than any which he had so prepared, and simply dip them for an instant into the antiseptic lotion, and then plunge them into the interior of the joint. Now, Gentlemen, was that doing the treatment justice? Between the teeth of those forceps there were probably portions of dirt. Give the carbolic acid lotion time, and it would penetrate this dirt, greasy though it might be; but it cannot do so in a moment; and nothing was more likely than that some portion of this dirt would come off from the forceps and remain

in the joint, and induce putrefaction there. I have known of a gentleman with every anxiety to carry out antiseptic treatment, exploring the wound in a case of fracture of the skull, and, the probe happening to fall to the ground, it was taken up from the dusty floor, and immediately passed into the depths of the wound. Now, Gentlemen, that was but courting failure. What more likely than that some of the septic dust, which certainly was brought up adhering to the bloody probe, should pass into the wound without having been sufficiently acted on by the spray in the moment of transit, and, mingling with the blood in the interior, be there protected for the future by the blood-clots from the antiseptic influence of the dressings, and induce putrefaction If we could see the septic material upon the instrument as distinctly as we could see green paint in contrast with the red blood, then of course we should say, We must wash off this green poison; but because we cannot see it with the physical eye, we are always liable to make mistakes through neglect of using proper precautions; and I am more and more persuaded, the longer I practise antiseptic surgery, that the chief essential to success is a thorough conviction of the reality of the presence of the septic matter on all objects in the world around us. Through the kindness of the President of the Physiological Section, I hope to have the opportunity of demonstrating some facts which I believe will tend to convince you that the septic ferments are, like those of the alcoholic fermentation, living organisms—that they are analogous to the yeast plant. But whether you believe or do not believe that they are living, it is certainly demonstrated scientifically as it is certain we are here, that these ferments do exist. If we do not bear that in lively remembrance, we shall be constantly making mistakes.

[Mr. Lister then proceeded to perform the operation. Some small arteries, which bled in the incision, were secured with fine prepared catgut, and the joint having been opened, two drainage-tubes, each about ¼-inch in diameter, were inserted side by side; an obstructing band within the articulation being divided by a probe-pointed knife guided by the finger so as to permit them to be introduced fairly into the cavity. He commented on the various steps as he proceeded, urging again the absolute necessity of having all the instruments thoroughly

aseptic, and went on to say]—One learns after a while to do these little purifications instinctively, but at first it requires thought, intelligence, and constant care, particularly to anyone who has been in the habit of operating without having to attend to these minutiae. Would that we could get rid of all complications in the system! If we could dispense with the spray, no one would rejoice more than myself; but until somebody wiser than I am can supply some better means, we must continue to use it. There is, I find, considerable thickening of the textures in the vicinity of the joint, and this is the cause of the swelling which is still apparent, though the synovial capsule is now empty. The outer orifices of the drainage-tubes are made transverse or oblique, as required, in order that they may lie flush with the surface of the skin, and when retained in this position by means of the threads which you see attached to their margins, they discharge their functions perfectly.

The operation having now been performed, the next point is so to dress the wound as to make sure that nothing septic will get in before next dressing; this must be not a matter of hope but of certainty. The material which we have used for some time past is an open cotton cloth, with the fibres impregnated with a mixture of carbolic acid and common resin.[1] Common resin holds carbolic acid with extreme tenacity, and in consequence of this gives it off so slowly as to be unirritating to the skin; yet at the temperature of the human body it furnishes a sufficient supply of the acid for a trustworthy antiseptic dressing. But at the ordinary temperature of the air in this country, the antiseptic is evolved so slowly from the gauze that the fermentative energy of septic dust is not at once extinguished by falling upon it, as it is by mingling with a strong watery solution; and if the gauze were applied dry, some active septic particle adhering to its surface might enter the blood or serum at the outlet of the wound, and propagate putrefaction to the interior. There was a time when I used to have occasionally in my practice putrefaction which I could not explain, but which I afterwards saw must be due to this cause, and the difficulty

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[1] For details regarding the composition and mode of preparation of the antiseptic gauze, see *Lancet*, March 13, 1875; *Collected Papers*, vol. ii, p. 210.

was then at once overcome by dipping the lowest piece of gauze in a watery solution of carbolic acid. This solution which I am now using, having been mixed with blood from the wound, has a very dirty appearance. A surgeon, who went round my wards some time since, expressed astonishment that I should use dirty lotion to wash a wound, and to purify what I was placing upon it; but, Gentlemen, the wound, although aesthetically dirty, was surgically pure, and the lotion had not been made impure by being used for washing it. Even if it had been otherwise, we might have trusted the carbolic acid to purify it. Why then should we waste good lotion? I dip, therefore, in the lotion this piece of gauze that I place next to the wound, and thus make perfectly sure that nothing septic is applied to it.

It is most important that the spray be properly directed during the dressing. I have seen a surgeon expose a serious wound, involving injury to the brain, while the spray was only playing on the opposite side of the head. It were far better that the antiseptic method should not be employed at all than that it should be used imperfectly. For such attempts not only end in disappointment, but throw discredit on the system. Some people seem to say, "I have tried the thing and failed, and therefore, of course, the system is all nonsense." I have seen it fail in my own practice, but under such circumstances I have always thought there must have been some mistake on my part, and I have endeavoured to discover where my mistake lay. But that does not seem to be the way in which the matter is viewed by some of our professional brethren.

A small piece of gauze dipped in the lotion having been placed next the wound, the dressing on which we rely for excluding putrefaction is applied in the form of eight layers of the gauze, sufficiently broad, as you see, to cover the surrounding skin for several inches in every direction; and beneath the outermost layer is placed this piece of thin mackintosh cloth to prevent the discharge from going directly through the dressing; because, if a considerable quantity went through, strongly as the resin holds carbolic acid, it might be all washed out before twenty-four hours had elapsed, and then putrefaction would spread inwards to the wound. The dressing is secured by a bandage, for which strips of the antiseptic gauze prove very convenient. Now, Gentlemen, we are perfectly sure that, if we

have left nothing septic in the wound, we shall find no putrefaction when the dressing is changed to-morrow.

[The subsequent progress of this case has illustrated well the remarks made at the demonstration, with regard to the effects of a free opening, or the contrary, under antiseptic management. When I saw the patient on the following day, I learned that he suffered unusual pain in the afternoon after the operation, which became very severe during the night, and though somewhat less in degree at the time of my visit, was still very considerable. The temperature had risen on the previous evening to 102·4° Fahr., and was now 101·8°. Such a state of things would at one time have alarmed me, and would have made me fear that putrefaction had occurred. This, however, I felt confident could not have been the case, and another probable explanation suggested itself. The peculiar bulging above alluded to, situated over one of the pouches of the synovial capsule beside the ligamentum patellae, had tempted me to make the opening in that situation; but the bulging part collapsing on escape of the fluid, the only way in which I could ensure complete introduction of the drainage-tubes into the joint was by passing their ends under the ligamentum patellae; and I thought it not unlikely that they might have been compressed, and their function so interfered with. Accordingly, on changing the dressing, I found that the gauze presented a bloody stain, which appeared sufficiently accounted for by oozing from the surface of the wound, while the joint was fully distended. And it appeared that the disturbance to which the articulation had been subjected had led to unusually rapid effusion from the synovial surface, and this being unable to escape, had produced great tension, attended with pain and fever. I at once placed him under chloroform, and made a fresh incision at the outer side of the limb into the pouch above the patella, and introduced a drainage-tube larger in diameter than the little finger, after pressing out the clear serous and fibrinous contents of the capsule. This was of course done with antiseptic precautions, and a dressing like that employed the day before was applied. The result was that almost immediately after awaking from the chloroform sleep, he felt himself entirely relieved of his pain; and not only has that which was induced by the first operation left him, but he has entirely lost that which had annoyed him

for so long a period previously. The temperature in the evening was found to have fallen to 99° Fahr., and has since remained normal, and the discharge, which has continued to be merely serous, has so diminished in quantity, that when I last saw him (August 15) I substituted a drainage-tube of medium size for the large one, and was able to direct that an interval of three days should be allowed to pass before the next dressing. I must add that he has tested the limb, contrary to orders, by getting out of bed and resting his weight upon it, but without any of the pain which he formerly experienced on so doing. In all other respects he is in perfect health.

It happens, by a curious coincidence, that another patient requiring the same operation has since been admitted under my care in the infirmary; a man 26 years of age, who, six days before admission, observed a painful swelling in the left knee, without assignable cause, and both pain and swelling had since steadily increased. The skin, however, was free from redness, and, subacute as the case was, I hoped that entire rest, with efficient fomentation, would relieve him. On the contrary, pain continued to increase during the next five days, while the temperature rose above 100° Fahr.; and on the 11th inst. I introduced a large-sized drainage-tube into the joint by incision above the patella, at the outer side of the limb. With the serous fluid that escaped were mixed considerable portions of lymph, opaque, and in some parts of yellowish-white colour; and these portions proved on microscopic examination to be masses of pus corpuscles; so that it was clear that the case was just passing into one of that justly dreaded disease under ordinary treatment, suppurative synovitis. The result, as in the former case, was immediate and permanent relief from pain. His temperature next day was normal, and has remained so. The discharge, purely serous in quality, is quickly diminishing in quantity, and the patients eats and sleeps as in perfect health.]

The next patient I wish to bring before you is one who came under my care six weeks ago with an affection of the inner side of the ankle, which he attributed to a sprain two months previously, after which he had constant pain in the part, and increasing thickening of the textures. The outer side of the foot and ankle looked perfectly sound. We put up the limb in a side splint of poroplastic material, and used repeated blistering,

but without any advantage; pain continued to increase, and it was evident that, if left to run its course, it would end in caries of the tarsus. I therefore, fifteen days ago, made an antiseptic incision, expecting to open a joint, but hoping that I should not find pus. To make an opening into an articulation without the presence of pus would have been, without antiseptic means, an unjustifiable proceeding; but here, as I have said, I hoped not to find suppuration, because I knew that, if the procedure were antiseptically conducted, the opening into the joint would do no harm whatever, while I should be able in all probability to get great benefit through relief of tension by free incision; and if I should find that no pus had been formed, this would make the case much more hopeful, because it would show that the disease was not so far advanced as if suppuration had already occurred. I was gratified, therefore, to find, on cutting into the soft substance, which gave very much the same sense of fluctuation, before the incision was made, as if fluid had been present, that there was no pus—nothing but inflammatory degeneration of the soft parts, the lateral ligament between the astragalus and the navicular bone being entirely disorganized, so that when the finger-nail was applied the softened textures gave way with the utmost readiness, and the joint lay freely open before us, the cartilages happily appearing to be sound. I will now change the dressing, so that you may see the appearance of the part. While the bandage is being cut or removed, the patient, or an assistant, keeps his hand over the site of the wound, to prevent the dressing from rising *en masse,* and pumping in septic air. As I raise the folded gauze (exactly similar to that which I applied in the last case), I take care that the spray passes into the angle between it and the skin. And now, Gentlemen, I venture to say here is a novelty for such of you as have not practised antiseptic surgery. There is the blood-clot still lying in the widely gaping wound, purposely kept open by this drainage-tube, which I introduced down to the open joint when I made the incision fifteen days ago, and which has never yet been taken out.

I have not seen this wound myself since I made it. I am sometimes accused of taking a deal of unnecessary pains with my cases, and it is also said that any good results which I may get are due to my own personal care. If such were the case,

Gentlemen, if I obtained better results than other surgeons by the more careful use of the same means, that would indeed be something to be proud of. But it is not so. It is simply that we are working on a new principle. Mr. Rice, my house surgeon, who was trained first as a dresser and afterwards as a clerk under me, does these things exactly as I do them myself. If I were to go away for a week, a fortnight, or a month, as far as the antiseptic element is concerned, I should feel I had left my patients in perfectly safe hands. In this particular instance, Mr. Rice has had sole charge of the dressing after the first day, and here is the result. I am very glad to see, looking at the foot now for the first time for a fortnight, that the inflammatory thickening has almost entirely gone. I had of course made inquiry as to the patient's progress, and I had learned from his lips that the pain was greatly diminished, as the immediate result of the incision. I used to have a great horror of opening into the tarsal articulations in cases of this sort in consequence of the disastrous results which I have known to occur, through the spreading of suppuration among them. But if the skin is unbroken, so that the antiseptic system can be brought fairly into operation, there is no such danger. Here there has not only been no disturbance whatever from the operation, but we have obtained the benefit that we anticipated from free incision. The inflammation which previously existed has almost, if not entirely, disappeared.

And now let me direct your attention again to this remarkable appearance of the blood-clot lying in the open wound fifteen days old. If we had not used antiseptic means, that would have been impossible. Some people say, We can show you good results without antiseptic treatment. Of course, good results can be got by good surgery without antiseptic treatment; but I say this is an instance of something that could not possibly happen without it. When a blood-clot existed in an open wound under a moist dressing which was not antiseptic, it was absolutely certain to putrefy and disappear long before the lapse of fifteen days. Let us now see what change may have taken place in this clot. I see, when I raise the upper layer of it from the edge of the wound, that there is about an eighth of an inch of cicatricial margin; yet there is no pus—there is not even any granulation. How the tissue which is thus formed in an organiz-

ing blood-clot differs histologically from that of granulations, I have not had time to investigate. But that it differs from granulations functionally is certain, and that in two ways. First, it has not nearly the same tendency to contract that granulations have; and, secondly, instead of forming pus under the influence of the very slightest stimulus, as granulations do, this tissue resembles normal textures in requiring protracted stimulation to induce it to granulate and suppurate. Now, cicatrization in an open wound without granulation is something new; it never happened in the world's history without antiseptic means.

We may now dispense with the drainage-tube in this case; and having removed it from the tubular cavity in the coagulum in which it lay, I will cut out with scissors a piece of the tube of blood-clot. You observe blood oozes freely from it. What was once blood-clot bleeds when wounded. It has become organized and vascularized up to the surface.

If there had been a dressing of carbolic gauze applied next the wound, and changed daily, we should have had a very different appearance. It seems to be a difficult thing for me to write the English language so as to make my meaning intelligible. I find the opinion still often attributed to me, that carbolic acid stops suppuration by some sort of specific agency. On the contrary, I have pointed out, from my earliest experience in the subject, that antiseptic treatment threw remarkable light upon the subject of suppuration, by showing that an antiseptic itself, while it prevented putrefaction, stimulated to suppuration; so that you have what I have termed "antiseptic suppuration,"[1] if the antiseptic continues to act upon the tissues for a certain length of time. If we had not interposed this layer of prepared oiled silk to protect the wound from the stimulating action of the carbolic acid in the gauze, we should have had a granulating and suppurating sore long ago. The blood-clot itself in its superficial layers serves as an additional protection to that which lies beneath; but if the blood-clot, which must be regarded as a kind of tissue, is stimulated by an antiseptic, its superficial parts are converted in time into granulations which suppurate. The interposition of the oiled silk

[1] See *Collected Papers*, vol. ii, p. 152.

"protective" shields the clot more or less completely from this stimulating agency, and, provided that you can allow a considerable period to elapse between the times of changing the dressing, so as to avoid the frequent washing of the clot with the stimulating antiseptic lotion, you may often see cicatrization proceed to its completion without any granulation occurring. In the present case, it is five days since the dressing was last changed, and it might have been left longer without risk of putrefaction, the serous oozing being so extremely trifling. [The case had been dressed four times in all during the sixteen days that had passed since the incision was made, viz., on the day immediately following the operation (which, as a rule, should always be the case), and afterwards at increasing intervals, as the serous oozing diminished. But the deepest part of the dressing, consisting of the protective and the small piece of gauze immediately over it, had been left in place from first to last, to avoid as much as possible the stimulation of the clot. I may add, in preparing this paper for the press, that the case has continued to progress well. The patient told me yesterday (August 16) that the last trace of the jerking pain which he used to feel left him on the evening of the day of demonstration; and Mr. Rice informs me, that, on changing the dressing on the 14th, after an interval of six days, he found cicatrization almost complete. We may therefore say, without much risk of mistake, that this foot has been saved from amputation by antiseptic treatment.]

The next case is one of ununited fracture in the lower part of the femur of a year's standing, in a man 36 years of age. Twelve days ago, I cut down on the outer side of the limb, a very long incision being required. Finding the fragments overlapping about an inch, I removed portions with the gouge and hammer from the posterior surface of the upper fragment and the opposing part on the anterior surface of the lower one, so as to leave two fresh osseous surfaces in apposition. Without antiseptic treatment, this would have been a very dangerous operation. The risk of pyaemia would have been so great, that, in common with most surgeons, I should have regarded such interference as unjustifiable; but I think we may venture to say that, with antiseptic treatment in its present form, all such risk may be certainly avoided. It is now twelve days since the

operation. For the first few days blood and serum were effused very copiously, and we had an arrangement by means of which a large mass of gauze could be applied in considerable extent under the limb. But the time has come when it might be put up in a more permanent form. This plaster-of-Paris arrangement was applied yesterday, while the limb was kept well extended by the pulleys, the patient being under chloroform. I have here a limited space for the dressing, and therefore use a correspondingly thick mass of gauze. This you will find often a matter of importance, as in operating for strangulated hernia, where you have not much space between the wound and sources of putrefaction in the perineum. And so in the present case, the window left in the plaster-of-Paris is occupied by a very substantial mass of gauze. The discharge of the last twenty-four hours has caused, you see, merely a small brownish stain upon the gauze, the result of a slight amount of serum, tinged with the colouring matter of the blood. The ends of the wound were stitched up for about three inches at each side; those parts united by first intention, and are completely healed. The central part of the wound was left open for the orifices of three large drainage-tubes. And here again we see the persistent blood-clot. Two days ago, I took out for the first time the drainage-tubes, and they were, just as in the case you last saw, lying in tubular moulds in the coagulum. One of them was permanently removed; the other two were re-introduced after being considerably shortened by cutting portions off from the deeper ends. In taking out drainage-tubes you must be particularly careful to have the spray properly directed. For as the drainage-tube comes out, air must enter to take its place, and this air will be septic or not as the spray is or is not over the wound. Here we see the orifices of the two drainage-tubes, one of which may probably now be dispensed with altogether. As I remove them, you observe the tubular beds in which they lay. And here, as in the last case, we have as yet no suppuration whatever from the open wound.

The protective must never extend beyond the gauze; if it did so, by excluding the action of the carbolic acid it would allow putrefaction to spread in under it.

I should have liked very much to have shown you one other case, but as time does not permit this, I shall mention in brief

the main points of it. The case was one of chronic inflammation of the lower part of the tibia, which had induced great thickening of the bone, attended with severe and constant pain, in a girl 18 years of age. There was a small sinus present, but scarcely any discharge. Introducing the probe, I found it pass deeply into the substance of the bone. Supposing that there might be some small exfoliation present, I proceeded to explore the bone, detaching the periosteum from the surface, and making an excavation with a gouge and hammer. I found a peculiar state of things pathologically. The chronic inflammation, instead of producing merely a softened state of the bone, had led to a conversion of the osseous texture into granulations. We operated by the bloodless method, and found these granulations almost perfectly white. I proceeded to dig these out, and got into cavity after cavity. At one time I thought the probe had gone through the posterior surface of the tibia, but it proved to have passed into another cavity in the extremely thickened bone. At last I found that the soft material at the lower part of the excavation moved when the foot was moved; or, in other words, I had opened into the ankle-joint. The result of the whole procedure was a very large and complicated cavity, and it is to the mode in which this cavity has been filled up that I wish to direct your attention. Now, I desired that this should be done by means of organizing blood-clot. If this is done it saves a great deal of time as compared with granulation and healing from the bottom, and produces a more smooth and level scar. As for a long time past I have done, I systematically placed the protective right across from one lip of the wound to the other, and then stretched the small piece of moistened gauze over, so as to keep the protective flat, in order that the blood-clot might accumulate under the protective, and so fill the wound. But we forgot to arrange the limb in proper position. It was allowed to lie resting on its posterior surface, and on changing the dressing next day I found that a large portion of the blood had drained out of the cavity. The deepest recesses of the excavation in the bone were indeed filled with clot, but a great cavity still remained. Well, there was an observation made by my colleague Mr. Chiene not long since that gave me a hint as to how to do in this case. He observed that, having systematically arranged for the formation of blood-clot in a

hollow wound, a portion of the blood in his case, as in mine, trickled out, and the blood-clot only partially filled the wound. After the lapse of sixteen days, Mr. Chiene proceeded to ascertain by scratching with the point of a knife whether the blood-clot was organized. He found it was, for blood was effused from the vessels of the tissue into which it had become converted. Dressing was applied as before; and the remarkable thing is, that this secondary blood-clot, formed on the top of the first, became also organized like the first, producing living vascularized tissue level with the surface of the skin.[1] That observation gave me the hint how to deal with this case; for it showed that if the blood-clot is insufficient in the first instance, we may supplement it by letting fresh blood into it at a later period; and if the secondary clot became organized in Mr. Chiene's case, though formed so late as sixteen days after the operation, still more might such an occurrence be expected if the second bleeding took place at an earlier period. Accordingly, three days after this operation had been performed, I took a sharp knife and made a few slight incisions in the sides of the wound. A considerable quantity of blood poured out, and the limb being kept on its side, to prevent it from escaping, the result is that, twenty-two days after the operation, and nineteen days after this secondary procedure, I could show you still a portion of the secondary blood-clot visible, while the greater part of it has given place to granulations. [It may be added, that the patient has lost all her pain from the time of the operation, and that here, as in the case of disease of the foot and in the ununited fracture, there has never been the faintest inflammatory blush around the open wound.]

DEMONSTRATION II—PART I.

GENTLEMEN,—The first patient I wish to show you to-day presents an illustration of the effects of ligature of an artery in its continuity by means of prepared catgut applied antiseptically. The opportunity of showing him to you, I owe to my colleague Mr. Annandale, under whose care he has been.

The case was one of aneurysm of the upper part of the

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[1] See *Lancet*, July 10, 1875.

femoral artery; but as it would not be right for me to anticipate Mr. Annandale in the publication of its details, I shall merely mention to you the main point that I wish to illustrate. The external iliac artery was tied under spray, the operation (at which I happened to be present) being performed with strict regard to antiseptic management, while the important matter of the use of drainage-tube was not neglected. The operation was performed on the 23rd of June, and the wound was absolutely skin-whole in fifteen days, without the occurrence of any suppuration at all.

[Mr. Furneaux Jordan, of Birmingham, was now kind enough to come forward and examine the patient, verifying the fact that there was no pulsation in the artery at the groin. Mr. Lister then proceeded.]

The immediate object of the operation has therefore been attained—the vessel has been permanently obstructed at the part tied; and this has been done without the occurrence of any suppuration, and by a mode which, I think we may venture to say, involves no danger whatever, provided it be properly carried out. The two great risks of an operation like this are, of course, secondary haemorrhage, and diffuse suppuration in the cellular tissue around the peritoneum; and both of these are securely guarded against by proceeding in this manner. I believe myself that this is a pretty perfect method of obstructing a vessel in its continuity; I do not see that we can wish to have it improved upon. I therefore regret extremely to find that it is still distrusted in various quarters, even by those who use catgut for the ligature of arteries in ordinary wounds. They do not trust it for tying arterial trunks in their continuity. I regret this the more, because I feel it is to a certain extent my own fault. When I first published on the subject, I was not aware myself of the proper mode of preparing the catgut. I had prepared it right, but by a mere accident. I described the mode of preparation in the *Lancet*,[1] as steeping the catgut in a mixture of carbolic acid and oil. It so happened that the carbolic acid which I used was liquid carbolic acid, so called—that is to say, crystallized carbolic acid, liquefied by the addition of water. Now, this water makes all the difference in the world. When

[1] See *Lancet*, April 3, 1869; *Collected Papers*, vol. ii, p. 86.

oil is added to this liquid carbolic acid, a considerable portion of the water is deposited in the form of very fine particles, which are suspended in the oil; and it is this mixture—this emulsion if we may so call it—of oil and water which causes the remarkable physical change in the animal tissue of which catgut is composed, that alone renders it fit for our objects. The tissue of the catgut in the ordinary condition is utterly unfit for surgical purposes; as slippery, when moistened, as a piece of intestine in a dead-house—when you tie it in a knot, it slips with the utmost ease. But after it has been steeping in the emulsion of carbolic acid, water, and oil for a certain length of time, it undergoes a physical change, which I am quite at a loss to explain. As the tissue lies steeping in this mixture, the first effect is to moisten it somewhat; then, as time passes, after about a week, you find that, instead of becoming softer, more swollen, and more opaque, as you would expect, it is, on the contrary, growing less opaque and beginning to shrink; and in about three months, though still softer than dry catgut, it is comparatively firm, and quite transparent. Now, if you take a fresh piece of dry catgut and put it into this same sample of the preparing liquid, you will find the second piece become in the first instance partially moistened like the first; a fact which renders it inexplicable to me, why the former piece should have undergone what looks like a partial drying. But whatever the explanation, the all-important fact is this, that after the catgut has been thus partially dried, so to speak, in this moist liquid, it is now no longer liable to be made slippery by being steeped in water or the animal juices at the temperature of the body: it is indeed rendered softer and somewhat opalescent, but a reef-knot tied upon it holds better than one on waxed silk. I repeat, when I first published on the subject, I was not aware of this circumstance. I had got the catgut properly prepared, but it was by mere accident that the water which is essential to the process was present in the mixture that I used; and, ignorant of its importance, I omitted to mention it in the description which I gave of the mode of preparation; whereas mere steeping of catgut in a solution of dry carbolic acid in oil, though it of course makes it antiseptic, leaves it perfectly unfit for use as regard its physical properties. When I found out my mistake, I sought to remedy it by insisting, in subsequent publi-

cations, upon the importance of the presence of the water in the preparation of the catgut; but I never stated, as I now do, that I had originally described an untrustworthy method. I very much regret this bad result of what turns out to have been premature publication; and I earnestly hope that this public confession of my mistake will have the effect of preventing any further bad consequences from it.

The catgut does not spoil by being kept a long time in the preparing fluid of oil, carbolic acid, and water. Here is some that was put in six years ago last month. It is now just as good as ever. Thin as it is, I cannot break it with any reasonable force. If you were going to tie the external iliac, you would use a thicker piece than this; partly, in order that it may stand any strain to which it could be reasonably subjected in the act of ligature, and partly that, a longer time being required for the absorption of the more substantial material, it may remain longer as a mechanical barrier to the force of the circulation.

[In this point of view there is another important advantage possessed by catgut properly prepared, viz., that it is much less rapidly absorbed than that which has been for a shorter time in the preparing liquid.

I would strongly advise any surgeon, who proposes to ligature an artery in its continuity with catgut, to test for himself the quality of the article; since those who sell it are tempted, if their stock of old catgut has run out, to supply that which has not been long enough prepared. In order to ascertain if it is trustworthy, a piece should be steeped for an hour in water about the temperature of the body, as in a vessel at a suitable distance from the fire. If then a reef-knot tied upon it does not slip, it is fit for use. And it will be well for the surgeon to keep a stock of the material for special purposes like these, testing it in the first instance in the manner described, after which he will be sure that, being still kept in the preparing liquid, it will be at least equally good at any subsequent period. If these points are attended to, there will be no further complaints about untrustworthiness of the catgut.][1]

[1] When it is requisite that the cord should be able to withstand all the strain to which the human hands can subject it, as, for example, if it be used for the pedicle in ovariotomy (in which case, I may remark, the pedicle would have to be well

I have now, Gentlemen, to bring before you two cases illustrating a somewhat interesting example of the usefulness of the catgut, namely, for the arrest of haemorrhage from a wounded vein.

Nineteen days ago I removed this patient's mamma, and at the same time cleared out the entire contents of the axilla, thus taking away, along with the fat, a number of scirrhous lymphatic glands, one of which lay immediately beneath the clavicle. In performing the operation, you may cut freely enough on the side towards the chest; but towards the axillary vessels, the glands, with the loose tissue about them, should be detached with the fingers, and any considerable-sized venous branch tied before it is cut.

[If the incision is carried parallel to the margin of the pectoralis major to near its insertion into the humerus, and the integument is raised a little from the edge of the muscle, and also freely dissected backwards to the fold of the latissimus dorsi, there will generally be obtained satisfactory access for dealing in this manner with glands situated even at the apex

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subdivided), the material must be of specially strong quality to begin with. Catgut consists of the peritoneum, together with muscular fibres, of the small intestine of the sheep; and the common kinds are either the entire tube of the gut, or longitudinal strips (according to the thickness required) simply twisted, dried, and subjected to sulphurous-acid vapour, or other chemical agents. But for special purposes, as, for example, the manufacture of fiddle-strings, the cord is made of several narrow strips twisted together, and is then very much stronger. Such catgut can be obtained of the musical-instrument makers, but is of course then unprepared in our sense, unfit for surgical purposes, and must be kept in the preparing liquid for a due length of time. For the sake of those who wish to prepare catgut for themselves, I may repeat here the proportions which I have found the best for the purpose. Add one measure of water to ten parts by weight of crystallized carbolic acid, mix and add one measure of the mixture to five measures of olive-oil, in a suitable jar or wide-mouthed bottle; then at once introduce the catgut, the hanks being opened up to allow access of the liquid to them; cover, and set aside in a cool place. Some water is gradually precipitated to the bottom of the vessel, and it is necessary to prevent any part of the gut from coming in contact with this precipitated water. A simple way of ensuring this is to put in as many marbles as will cover the bottom of the vessel.

of the axilla; the pectoralis being drawn well forward, when necessary, by means of a copper spatula. If, however, the space thus obtained is not sufficient, whether for the removal of glands in that situation, or for the arrest of haemorrhage there, the skin should be at once dissected up from the pectoralis, and the muscle divided transversely from the margin towards the collar-bone to any degree that may be requisite. When I first adopted, seven years ago, the practice of systematically clearing out the contents of the axilla, I divided both pectoral muscles in all cases (the pectoralis major only partially), and though I have since found that this is not generally necessary, yet the experience of the earlier cases was valuable, by showing that the division of the muscles, though it appears a severe procedure, does not seriously complicate the operation, either as regards its performance or its ultimate results. The arm being kept bound to the side, the divided muscles unite quickly, and the patient gives, in time, the best evidence that their functions are not materially impaired by being able "to do her back hair."]

In the present case, one of the glands was so very close to the vein that, as I was endeavouring with the fingers to detach it, a venous branch broke at its origin from the axillary, the result being an aperture in the venous trunk about an eighth of an inch in diameter. I seized the opening in the vein with catch-forceps, and put a catgut ligature upon it, but the thin slippery tissue of the venous coat slipped from the grasp of the knot. I made a second similar attempt, and again the same thing occurred. What was now to be done? Without antiseptic treatment I should have been a good deal at a loss. To have obstructed the main vein of the limb by tying it across like an artery, would have been most undesirable; and to have introduced a pad of lint into the wound, to compress the orifice, would have been very unsatisfactory practice.

I did, however, what I had long contemplated doing, if such a circumstance should arise. All flow of blood being temporarily stopped by the pressure on the vein to the distal side, I threaded a fine sewing-needle with the finest catgut, and passed it through the coats of the vessel at opposite points of the wound, and at a short distance from its edges, and then, cutting off the needle so as to leave two threads in its track, tied one thread round each half of the wound. The purchase thus secured upon

the venous texture prevented the ligature from slipping, and the bleeding was permanently arrested. The healing of the wound has proceeded undisturbed, and cicatrization is, you observe, already almost complete. Here the small part that remains unhealed being entirely superficial, it is no longer needful to use the spray in changing the dressing.

Another reason that has made me bring this patient before you is, that you may see how the drainage of the axilla was provided for, and this I believe to be a matter of great importance. In all previous cases of this kind, when it was necessary to clear out the axilla, my practice had been to extend the transverse incision made for removal of the mamma, and introduce a drainage-tube at the outer angle of the wound. But if this is done, it will sometimes happen, if the patient be stout, that in spite of the presence of a substantial pad of folded gauze between the arm and the chest, the skin of the fat side and that of the fat arm will come in contact with each other, and the drainage-tube will become obstructed, leading to tension in the axilla, and, it may be, inflammatory suppuration. But here, for the first time, I have got over this difficulty completely, by making a special perforation for the drainage-tube so far back as to be out of the way of the pressure of the arm. Here, you observe, is the place where the tube was inserted, viz., in the angle between the arm (as it lies against the side) and the back. Thus, while you avoid a needlessly long incision, you have the most complete possible drainage, and the result, as you see here, has been very rapid healing. We all know that wounds after removal of the mamma may heal quickly, and sometimes without suppuration, without any antiseptic treatment at all. But this, I suspect, could not have been an instance of that kind. A large amount of skin implicated in the disease had been removed, so that, notwithstanding the use of button-stitches,[1] tension was great; and if we add to this the presence of the large hollow wound in the axilla, it is not at all likely that under any treatment not antiseptic, healing would have occurred without suppuration, as it has done here.

The other patient whom I wish you to see, as an illustration of the arrest of venous haemorrhage by means of catgut, will

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[1] See *Lancet,* June 5, 1875; *Collected Papers,* vol. ii, p. 241.

now be brought in. She had long suffered from varicose veins, which you see conspicuous in the leg, even in the recumbent position in which she is; and I was asked to see her on account of haemorrhage that had occurred from a tumour about as big as an orange, which had formed in the ham, the most prominent part being formed of blood-clot. It was evidently composed of a mass of greatly distended veins, one of which had given way by ulceration. The case seemed urgently to demand interference, and I resolved to remove the mass—a thing which I should have hesitated in doing without antiseptic measures, as I felt sure that I should open into large varicose veins. Such proved to be the case, as you see from this preparation of the part removed. On section, the most prominent portion is shown to be composed of coagulum, while the deeper surface presents numerous large vessels. They have shrunk a good deal since they were removed, but when the operation was performed they were almost as thick as my little finger. And now we have to speak of how the veins, which lay open in the wound, were dealt with. Some of them presented transverse orifices, but others had been divided more or less longitudinally. I tried, by detaching the veins from the surrounding parts, and clipping away some portions, to get the vessels to present themselves in transverse section, so that I might tie them with catgut in the ordinary way; and in most instances this was satisfactorily accomplished. But there was one large vein presenting a longitudinal slit about five-eighths of an inch in length, so connected that I could not readily deal with it as with the others. I therefore adopted a practice which will, I believe, prove a valuable addition to our resources, in wounds of large venous trunks. Using a very fine sewing-needle and finest catgut as before, I sewed the two lips of the wound together by continuous or glover's stitch; leaving the calibre of the vessel intact. Now, I do not think any man would have been justified in doing that with ordinary silk or cotton without antiseptic measures. To do so would have been to run imminent risk of suppurative phlebitis and pyaemia. But by proceeding antiseptically we incurred, as I believed, no such danger, and the result is, as you see, so far satisfactory. It is now three days since the wound was dressed last, and five days after the operation. The discharge of the three days has caused, you observe, a merely

trifling serous stain upon the gauze. And there is entire absence of any inflammatory disturbance. In performing the operation, the skin having been very thoroughly washed with 1 to 20 carbolic-acid lotion, I took care to cut wide of the tumour, so as to keep clear of the putrefactive material on the exposed clot; but though a considerable portion of skin was thus taken away, I was able, by dissecting up the integument a little at each side, to free it so that its edges could be brought together closely by suture, except at the spot selected for the insertion of a small drainage-tube. You see the blood-clot still lying at this spot, while the stitches retain their places without any suppuration about them. [Healing afterwards proceeded to its completion, without gaping of the wound or any other untoward circumstance. It should be mentioned, that bloodlessness of the operation was provided for by encircling the thigh with a constricting elastic band, after emptying the limb of its blood by keeping it elevated for a few minutes in the vertical position.]

ON THE NATURE OF FERMENTATION.

The Introductory Address delivered in King's College, London, at the opening of the Session, October 1, 1877.

Collected Papers, vol. i, p. 335—*Quarterly Journal of Microscopical Science*, April, 1878.

In 1877 Lister—now in his fifty-first year—left his strong position in Edinburgh to embark on a difficult campaign in London. The following Introductory Address to the course of Clinical Surgery at King's College was his first public pronouncement—his battle-cry, as it were. In his audience were not only students, but many doctors and some followers of pure science.

It thus appropriately finds a place in this volume. It has the further advantage of being the most readable of his publications on bacteriology, and of giving his matured views at this time—views, moreover, which he had no reason to modify afterwards. The subject was more elaborately dealt with in a long-promised address to the Pathological Society,[1] in which he fully explained his bacteriological apparatus and how he used it.

He had been working at bacteriology for seven years or more, and had gradually built up a very complete method for isolating and cultivating different strains of organisms. His *Bacterium lactis* was the first organism to be submitted to this accurate scrutiny. Those who are interested in the subject should at least study the address to the Pathological Society. The earlier papers have much historical value. The full recantation in the Pathological Address of a mistake which he made and published in 1873 is characteristic of Lister's honesty. It will be observed that the organism to which he gives the name "Granuligera" is the genus streptococcus which had then scarcely been recognized and, of course, not subdivided into species.

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[1] *Collected Papers*, vol. i, p. 353; *Transactions of the Pathological Society*, 1878, vol. xxix, p. 425.

ON THE NATURE OF FERMENTATION.

GENTLEMEN,—In making my first appearance as a teacher in King's College, I cannot refrain from expressing my deep sense of the honour conferred upon me by the invitation to occupy the chair which I now hold; and, at the same time, my earnest hope that the confidence thus reposed in me may not prove to have been misplaced.

In considering how I could best discharge my duty as the person selected to deliver the Introductory Address of the Medical Session, I have felt that two courses were open to me: either to spend the short but important time at my disposal in an endeavour to convey to the student some sense of the exalted privileges, and correspondingly high responsibilities, of the beneficent calling to which he proposes to devote himself, or to treat on some special subject, in the hope that I might say something which should have interest, and, if possible, even instruction, not only for the student, but also for the eminent men whom I have the honour to see around me. The latter is the course which I have decided to follow, and the subject which I have selected is a short account of an inquiry in which I have been engaged in the interval between the cessation of my official duties in Edinburgh and their commencement here. The object of that investigation was to obtain, if possible, some more precise and definite knowledge of the essential nature of a class of phenomena which interest alike the physician, the surgeon, and the accoucheur. I allude to the changes in organic substances which are designated by the general term *fermentation*.

In medicine, the large class of diseases termed zymotic derive their name from the hypothesis that their essential nature is fermentative. In obstetrics, puerperal fever, the most frequent cause of disaster after childbirth, is now regarded by many of the highest authorities as likewise due to fermentative disorder; and, in surgery, among the various causes which may disturb a wound, we know that by far the most frequent in operation, and the most pernicious in its effects, both upon the wounded part

and upon the constitution, is putrefactive fermentation. If this be so, it is clear that to understand the nature of fermentation must be a matter of the very highest importance, with a view to curing or preventing the various evils to which I have alluded.

What, then, do we mean by fermentation? I shall best approach the answer to this question by giving an example. Rather more than a week ago, I witnessed in the north of Italy the time-honoured practice of treading grapes in the wine-vat. I was told that the juice would within twenty-four hours boil, as it was said, over the vats into which it was introduced; in other words, that the sugar of the grape-juice would within that short time be so converted into alcohol and carbonic acid that the carbonic-acid gas, by its evolution, would cause sufficient frothing to produce the effect to which I have referred. This conversion of the sugar of the grape into alcohol and carbonic acid is accompanied by the development of a microscopic organism, the yeast plant, or to continue the old nomenclature, *Torula cerevisiae*, consisting of microscopic cells multiplying by pullulation, as indicated in this diagram (not here represented). Now, it is, I believe, universally admitted that the alcoholic fermentation of grape sugar is due to the growth of the yeast plant. M. Pasteur thinks that he has traced the origin of the yeast plant in the juice of the grape to a minute fungus adhering to the outside of the skin of the grape.[1] Be this as it may, it is admitted on all hands that the alcoholic fermentation is caused by the growth of the yeast plant. So long as the juice of the grape is protected by the skin of the berry, no fermentation occurs; but, as soon as it escapes from that protection, the organism, by its development, induces the fermentation. Nor is it by any means exclusively in the natural juices of fruits that such fermentation occurs. Any sugary solution, provided it contains, besides the sugar, other ingredients requisite for the nutrition of the yeast plant, will serve as pabulum for the organism, and in that case the yeast plant will give rise to the fermentation. Here is a glass containing such a liquid, termed Pasteur's solution, because it was devised by M. Pasteur for the very purpose of affording nourishment to the yeast plant and other minute organisms. This was prepared on August 7 in a

[1] *Vide* Pasteur, *Études sur la Bière*, pp. 150 *et seq.*

flask purified by heat, covered over with a cap of pure cotton-wool,[1] which allows the entrance of air, but does not permit the access either of the yeast plant or of any other form of dust. The Pasteur's solution, containing, besides sugar, ammoniacal and earthy salts for the nutrition of the fungus, was heated to about the temperature of boiling water, so as to destroy any organisms that might exist in the water. The result is, that it continues perfectly unchanged, just as it was on August 7; but, if we were to add to it a little of the yeast plant from fermenting grape-juice, we should find that, at the temperature of summer weather, it would very soon be in a state of free fermentation at the same time that the yeast plant would multiply. This, then, is a typical instance of fermentation. We have an active agent termed the ferment, which ferment is capable of self-multiplication. That I believe to be the essential property of a true fermentation. Now, in this particular case, I have already said it is admitted on all hands that the yeast plant is the cause of fermentation. Persons may differ as to how the development of the yeast plant gives rise to the resolution of the sugar into the alcohol and carbonic acid gas; but all now agree that, somehow or other, the organism causes the fermentation. Now, is it the case that all true fermentations are caused by the development of organisms? That, gentlemen, is the question which it is desirable that we should be able to answer.

Take, for example, the case of the putrefactive fermentation of blood. We all know that, if blood be shed from the body into any vessel without special precautions, in a few days it putrefies. The bland nutrient liquid, soon after leaving its natural receptacle, becomes foul, acrid, and poisonous; a change fully as striking as that which grape-juice undergoes in the alcoholic fermentation. Here, on the other hand, we have a vessel (a liqueur-glass) into which blood was received with special precautions. In the first place, the glass, covered, as you see, with a glass cap and a glass shade, with a view of preventing the access of dust, and standing upon a piece of plate-glass,

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[1] The cotton-wool was rendered free from living organisms by soaking it with a solution of carbolic acid in one hundred parts of anhydrous ether and allowing the ether to evaporate, leaving the carbolic acid behind in the cotton.

had been heated to about the temperature of 300° Fahr., and cooled with an arrangement which ensured that the air which entered during cooling was filtered of its dust; so that we were perfectly sure that the glass contained at the outset no living organisms.

Then, in the second place, the glass had been charged from a flask like this, provided with a spout. It contains, as you see, a glass tube introduced into it; it is stuffed well with cotton-wool between the neck of the flask and the tube, there is a piece of cotton-wool over the end of the tube, and another piece is tied securely over the spout of the flask. The flask so arranged was heated just as the glass had been heated. It is not necessary to have the temperature so high as to singe the cotton. Heat far short of this is adequate, according to my experience, to make perfectly sure that you destroy all living organisms. The flask having been thus prepared, the jugular vein of an ox was exposed and divided, with precautions against the entrance of anything putrefactive,[1] and, the cotton cap having been taken off from the end of the tube, the vein was slipped over the tube and securely tied on, and then the hand of the assistant, who previously restrained the flow of blood, being relaxed, blood was permitted to flow into the flask. Then, before coagulation had time to take place, this and various other similar glasses were charged after the removal of the cotton cap from the end of the spout. Now, the first thing that may strike you is the remarkable fact that this blood-clot has not undergone any contraction. One of the earliest things that your professor of physiology will have to teach the junior students will be that blood, after coagulation, contracts; that the fibrine of the coagulum shrinks and the serum is pressed out. But here no such thing has taken place. There has been no shrinking of this clot, no pressing out of the serum, and I venture to say that there is no one here—at least I think it is unlikely that there is anyone here except myself—who has seen such a phenomenon, illustrating how, when the most familiar objects are placed under new circumstances, the most unexpected results may arise. Now,

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[1] This was secured by washing the skin and the instruments with a strong solution of carbolic acid (1 to 20), and performing the operation under a carbolic spray.

this is a matter of very considerable interest with reference to the behaviour of blood-clots inside the body in wounds and so forth. However, that is not a point on which I wish to dwell on the present occasion.[1] The point to which I wish to draw your special attention is, that this blood, although it has been six weeks in this glass, without any close fitting of the glass shade or the glass cap, and therefore with free opportunity for the access of the gases of the atmosphere, has not putrefied. The air in the glass shade is perfectly sweet, perfectly free from odour.

Now, gentlemen, this, without going further, is a very important matter. It proves that the blood has no inherent tendency to putrefaction. It further proves that the oxygen of the air is not able to cause the blood to putrefy, as used to be supposed. There was a time—the effect is still seen to a certain extent—when the dark venous colour of this blood-clot gave place to the crimson colour of arterial blood in a gradually deepening band from above downwards. We still see some of the red colour remaining, though now the converse effect has begun to take place. That florid redness, gentlemen, showed that the oxygen of the air was in reality acting upon the blood, yet it did not putrefy. Now, if I were to take a little morsel of already putrefied blood, say, upon the end of a needle, and touch with it this clot of blood, putrefaction would, in the course of a very short time, spread throughout the mass. Exactly as in the case of alcoholic fermentation under the influence of the yeast plant would the fermentation spread.

Putrefaction, then, is a fermentation, a true fermentation, characterized by the power of self-multiplication of the ferment. Then, gentlemen, if we examine microscopically, we find in the putrefying blood, as we found in the fermenting grape-juice, microscopic organisms, termed *bacteria* from their rod-shape, which we have represented in this diagram on the same scale as we had the yeast plant; of different sizes, but all very much more minute than the yeast plant, and commonly endowed with a remarkable power of locomotion. I say that, in the putrefying

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[1] I desire to guard myself against being supposed to express any opinion here as to the cause of this phenomenon.

blood, we find these organisms developing *pari passu* with the fermentation.

Now, the question is, Are these bacteria the cause of the putrefactive fermentation, or are they merely accidental concomitants? These are two views which are entertained at the present day by men of high eminence. It may be said, "Why should there be any doubt that the bacteria are the cause of the putrefactive fermentation, any more than there is a doubt that the *Torula cerevisiae* is the cause of the alcoholic? Well, one reason I believe to be that the bacteria are so exceedingly small. They are not so easily defined as the yeast plant. We cannot get them in a mass as we can get a mass of yeast; at least without a great deal of trouble; and, besides that, they occur very similar in appearance in a great number of different fermentations. There is, therefore, so far some colour for doubting whether bacteria are the cause of a special fermentation, like this putrefaction. Then there is another ground justifying such a view; for certain it is that organic substances are liable to extremely remarkable alterations, decompositions, under the influence of agents which are endowed with no life at all. As good an example of this as we can take is what occurs in the bitter almond when it is bruised with water. You all know what takes place under those circumstances; that there is prussic acid developed, and essential oil of almonds, with other materials. Now, these did not exist beforehand in the almond, but they are the result of the mutual action upon each other of two constituents, neither of which was hydrocyanic acid nor oil of bitter almonds, &c. These two constituents are termed emulsin and amygdalin. Amygdalin can be got from the almond in the form of definite crystals; and emulsin, though not a crystallizable substance, but a variety of albumen, can also be obtained separate. Till these two materials are in a state of solution in water, they do not act upon each other at all; but, as soon as they are in watery solution, the emulsin so acts upon the amygdalin that the latter becomes broken up into the constituents to which I have referred. This is an exceedingly remarkable fact. Undoubtedly, the emulsin is dead; there is nothing living about it. It is not an organism. It is obtained by a process of alcoholic extraction, and so forth. It is thoroughly a chemical substance, a merely dead substance, if we may so speak, and

yet it does produce this remarkable effect upon the amygdalin. But, when we come to consider this case, we find that the process, remarkable as it is, lacks the true character of genuine fermentation, that of the faculty of self-propagation of the ferment. Liebig himself, who was the great advocate of the doctrine of so-called chemical ferments, and who, along with Wöhler, discovered this action of emulsin on amygdalin, pointed out, and showed by irrefragable evidence, that the emulsin does not undergo any multiplication; not only so, but that, after a while, the emulsin loses the property of acting on the amygdalin; but, for a considerable time, it continues to act upon it without undergoing apparently either increase or diminution of its bulk. It may be called a resolvent, the amygdalin being the resolved material.

There are other cases equally striking that might be mentioned, not only in the chemistry of vegetables, but in the chemistry of our own bodies. There exists, for instance, in the saliva a material called ptyalin, which has a remarkable power of acting upon starch, so as to convert it into soluble compounds. In the gastric juice there is a material called pepsin, which has an equally remarkable property of acting on albuminous materials, fitting them for solution in digestion. But here again we find, when we come to consider the matter, that there is no evidence whatever that either pepsin or ptyalin is capable of self-multiplication. Each is secreted for the purpose and in the quantity in which it is required, but it has no faculty of self-propagation; and I believe, if you search through the whole range of organic chemistry, you will not find a single established instance where any ferment, so called, destitute of life has been proved to have the power of self-multiplication. At the same time, gentlemen, it may be admitted that the thing might be theoretically possible. It is conceivable, for instance, that a resolvent, if we may so speak, of comparatively simple constitution might, by its action upon a resolvable compound, resolve it into substances, one of which should itself be the resolvent, and, if that were so, the process might go on *ad infinitum*. That is conceivable; and accordingly, although we have no instance of the kind on record, yet we have persons in high authority, as teachers both of physiology and of pathology, maintaining the view that in putrefactive fermentation, for

instance, the bacteria are probably mere accidental concomitants; that the real essential agent in the putrefaction is not an organism at all, but some so-called chemical ferment destitute of life. And so long as we have authorities maintaining such a view, it is necessary to test its truth or falsehood by searching inquiry; and such has been the object with which my investigations of the last two months have been conducted.

As regards the putrefactive fermentation, we have already evidence in the flask and in the glass that I have shown you (the flask also has no putrefactive odour emanating from it), that blood has in itself no inherent tendency to putrefy. It must receive something from without, and that somethting is not mere oxygen or any other atmospheric gas. I have now to point out to you that the addition of water is not of itself sufficient to induce this fermentation. Blood and water constitute a mixture highly putrescible, very much more so than blood itself. But in this flask we have had mixed with water the contents of one of the liqueur-glasses of unputrefied blood like that before shown to you. The water, however, had been previously boiled, so as to kill any organisms in it; boiled and cooled under the protection of a cotton cap, and then, the cotton cap being raised, careful provisions (into which I must not enter) against the entrance of dust being taken, the clot was spooned into the water; a fresh cotton cap, perfectly pure, was put on, and so we got, I believe for the first time, a permanent cold watery extract of blood, and here it retains the same brilliant clearness that it had in the first instance, more than a month ago. Mere water, therefore, is as inadequate to induce the putrefactive fermentation of blood as are the gases of the air.

But the fermentation which I have been especially investigating has not been the putrefactive, but one which seemed to me more convenient for the purpose, the lactic fermentation, by means of which milk sours and curdles, through conversion of the sugar of milk into lactic acid. This is a curious instance of a chemical transformation. The composition, as regards the proportions of the three elements, carbon, hydrogen, and oxygen, remains identically the same; but those of you who are chemists understand what I mean when I say the atomic weight of the lactic acid is one-fourth of the atomic weight of the sugar of milk. Each atom of milk-sugar is resolved into four simpler atoms of

lactic acid. Now, it may be naturally supposed, if you observe what happens in a portion of milk obtained from a dairy, that there is an inherent tendency in the milk to this souring and curdling. If you get milk from a dairy and keep it long enough, it is certain to turn sour and curdle; then, after a while, there comes a certain mould upon the surface, the *Oidium lactis,* which constitutes the sort of bloom there is upon a cream cheese; then comes on, often simultaneously with the growth of this mould, the butyric fermentation, in which butyric acid is produced; and afterwards, if you keep the milk long enough, it will probably putrefy. When you see, time after time, specimens of milk, taken from various dairies, undergo this succession of alterations, you may be tempted to suppose that these were changes to which the milk was disposed from its own inherent properties as it comes from the cow's udder. The late eminent Professor of Chemistry in this College, Professor Miller, in his excellent work on Chemistry, states that the ferment of the lactic-acid fermentation is the caseine of the milk. I am bound to say, however, in justice to Professor Miller, that he also adds that M. Pasteur has expressed his belief that there exists an organic living ferment which produces this fermentation; but Professor Miller does not profess to decide between these two opinions. On the contrary, his first statement, that the caseine is the ferment, might lead you to suppose that he is inclined to the former view.[1] If this were the case, as there is caseine always in the milk, there should always be the lactic-acid fermentation. But it was pointed out long ago by M. Pasteur that, if you examine any specimens of souring milk with the microscope, you find little organisms.[2] These, when you come to look at them carefully, you see to be obviously of the nature of bacteria. Bacteria may either have the faculty of motion or they may not. This particular bacterium is a motionless bacterium, so far as I know; still it has the essential nature of a bacterium: a microscopic fungus, multiplied by fissiparous generation, the lines of segmentation being transverse to the longitudinal axis

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[1] *Vide* Miller's "Elements of Chemistry," third edition, vol. iii.

[2] *Vide* "Mémoire sur la Fermentation appelée Lactique," *Annales de Chimie et de Physique,* 3me série, tome lii, 1858.

of the organism. I have ventured to give to this little organism the name *Bacterium lactis;* for, gentlemen, no doubt there are different kinds of bacteria. The circumstance that they are minute must not make us shut our eyes to this truth. You sometimes hear bacteria spoken of as if they were all alike. The fact that some do not move and others do, is one indication of a difference between them. Another indication of a difference is, that some bacteria will thrive in a medium in which others cannot live. For instance, the *Bacterium lactis* refuses to live at all, according to the more careful experiments I have been lately making, in Pasteur's solution; the very fluid provided by Pasteur for bacteria, torulae, and other fungi to live in, is a medium in which the *Bacterium lactis* refuses to grow at all; although many bacteria grow in it with rapidity. That is clear evidence that this is a different kind of bacterium from those which both thrive and move in Pasteur's solution. You will observe, also, it is somewhat peculiar in the form of the segments; they are oval, and not so rod-shaped as bacteria generally. These you will always find in milk when it is souring.

But, gentlemen, neither the souring of milk nor the organism which is found associated with that change is the result of any inherent tendency in the fluid. This is a flask of boiled milk prepared on the 27th of August, with the same arrangements for ensuring purity of the vessel and excluding dust that we had in the flask of Pasteur's solution. It has not coagulated; it has undergone none of the changes to which I have alluded. There has been no butyric fermentation, no *Oidium lactis* has formed upon it, no putrefaction has occurred. This milk is as sweet as when it was first prepared; and if you were to examine it with the microscope, you would find in it no organism of any kind. From this same flask, with precautions with which I will not detain you, I have charged various purified liqueur-glasses. This one has been charged for more than four weeks, yet the milk remains fluid, you observe, although there is abundantly free access of air to it. The oxygen of the air and the caseine which still exist in the boiled milk have together been unable to bring about the lactic fermentation. As regards boiled milk, this is sufficient evidence that the lactic fermentation is not something to which the liquid is spontaneously prone; it requires something to be introduced into it from without. For you must not suppose

that the boiling has rendered the milk incapable of souring. All that it requires is the introduction of the appropriate ferment. If you were to touch the edge of the milk in this glass with the point of a needle dipped in souring milk from a dairy, within two or three days the whole would be a sour clot, showing both the proneness of boiled milk to souring and also the genuine fermentative character of that change as indicated by the faculty of self-multiplication of the ferment. And on microscopic examination you would be sure to find the *Bacterium lactis* present throughout the mass.

But though the ferment which occasions the souring of milk is present in the milk obtained from any dairy, it appears to be by no means common in the world in general. Suppose you take a series of glasses of boiled milk like these, and introduce into them a series of drops of ordinary unboiled water, you will get fermentation in them. If you put into each, for instance, a drop as large as a quarter of a minim, you will have a fermentation in every one, and an organism in every one; but you will neither have, according to my experience, the lactic-acid fermentation nor the *Bacterium lactis*. You will have bacteria of other sorts; fermentations of other kinds. Again, suppose you take a series of such glasses, take off the glass shades and the glass caps, in different apartments or at different times, and expose the milk to the air-dust for half an hour; you will get fungi and bacteria of various sorts, but, according to my experience, you will not get the *Bacterium lactis;* nor will you get the lactic fermentation. And thus it turns out, so far as boiled milk is concerned at all events, that the ferment that brings about this particular fermentation is a rare ferment. So far from boiled milk being spontaneously prone to the change, it requires something to be introduced from without which is a rarity both in ordinary water and in ordinary air.

But then, it may be urged, indeed such arguments have been used, this may be very true for boiled milk, but how about unboiled? "May it not be that, by boiling the milk, you have destroyed certain chemical ferments, purely hypothetical we must admit, but which we think likely to exist?" For, according to the views of some persons, it may be that in the unboiled milk there may exist certain chemical substances prone to evolve into organisms by spontaneous generation, and prone to produce these

and other fermentations, but which, by the act of boiling, we deprive of this tendency. Therefore, with a view to meeting this objection, the first part of my investigation was devoted to endeavouring to see whether or not milk, as it comes from the cow, really does or does not contain materials tending to the development of organisms or to fermentation of any kind.

An exceedingly simple experiment will probably serve to convince you to a considerable extent with regard to this matter. If you go to a dairy where there is also a cow-house, take a couple of clean bottles, and fill one with milk from a pan in the dairy and the other with milk direct from the cow in the cow-house, the milk obtained from the dairy will be certain to sour, but that which you get direct from the cow will very probably never sour at all. It will probably acquire a nasty bitter taste, and will not contain the *Bacterium lactis* or the *Oidium lactis*, but some other kinds of fungi. That very simple experiment is enough to show that the lactic-acid fermentation is not a change to which unboiled milk is spontaneously prone. And it occurred to me that, if all organisms and fermentations which occur in milk really depended on accidental introduction from without, by performing the experiment with a number of purified glasses and taking the milk in small quantities into each, we might by thus subdividing elude the foreign element and get the milk, in some of the glasses at least, not only without the lactic-acid fermentation or the *Bacterium lactis*, but without any fermentation or any bacterium, or any sort of organism. Accordingly, I prepared little glasses like these; little test-tubes with test-tube caps, arranged upon a stand made of pieces of glass tube and silver wire. The stand containing the test-tubes was placed under a glass shade on a plate of glass and purified by exposure to 300° Fahr. in the hot box. Milk having been received from the cow into a purified vessel, some of the milk was then, by means of a syringe attached to this pipette (the pipette having been also previously purified), drawn up into the pipette, and then, by means of the syringe, each little cap being in succession raised, a few minims of milk were introduced into each of the glasses, the caps being immediately re-applied. The result was, every one of the milks underwent fermentation, and every one of them contained organisms, some of them as many as three different species. The great majority of those twelve glasses

presented little orange specks, such as were never seen, I suppose, in any milk before; and, on examining these, I found them to be little organisms belonging to a group to which I have ventured to give the name Granuligera, because they consist of granules, different from bacteria in this respect, that you might suppose them not to be organisms at all till you had the opportunity of seeing them undergoing multiplication by fissiparous development, in a manner, however, differing from the transverse fissiparous multiplication of bacteria, in being crucial.[1] But, besides the granuligera, there were among the contents of these test-tubes bacteria of different kinds, to judge by form and size, and in one of them was a toruloid organism, and in two others two species of filamentous fungi, one of which was of the most exquisite delicacy, though in general type of the same sort of arrangement as the common blue mould or the *Oidium lactis*. The size of the filaments was so exceedingly small that twenty of them would lie abreast in a single human red corpuscle; they were smaller in diameter than even the *Bacterium lactis*, smaller than the great majority of bacteria. I doubt if any such exquisitely delicate filamentous fungus has ever been seen before even by a professed botanist like my colleague Professor Bentley. But there was no *Bacterium lactis*, and there was no lactic-acid fermentation.

What inference were we to draw? Was I to suppose that, although the lactic-acid ferment had been excluded, it was impossible to exclude others; that others were present in the milk as it existed in the cow's udder; or was it that I had not been sufficiently careful? The latter was the view I was disposed to take. The experiment had been performed in the cow-house, where certainly the air might be supposed to be reeking with organisms. I therefore performed the experiment a second time, and this time in the open air. It must be confessed it was not far from the cow-house, and it was a fine day at the very time of the year in which organisms most abound. On this occasion, I used twenty-four of the little covered test-tubes; those which you see before you. The result was that this time, again, every glass had organisms developed in the milk which

[1] *Vide Transactions of the Royal Society of Edinburgh*, vol. xxvii, p. 319; *Collected Papers*, vol. i, p. 281.

it contained. At the same time, every glass seems to be different from all the rest. Such fermentations as there are here, I venture to say, were never seen in milk before. I have brought before you a diagram, showing some of them on a large scale. I want particularly to direct your attention to these strange scarlet spots which occurred in almost all of them. They began as tiny scarlet dots, which spread as fermentative changes capable of self-multiplication in the substance of the milk. Here is one glass that is green, and here is another of an orange-yellow colour. Here are two that have two kinds of filamentous fungi. I have not examined them microscopically, but I shall very likely find there are some species that have not been described.

I felt little doubt that these organisms had got in for want of sufficient care on my part. But how are we to explain these unheard-of appearances? Simply thus. If the *Bactarium lactis* had been here, it would have taken the precedence of all other organisms in its development, and the changes which it would have induced would have made the milk an unfit soil for these other numerous species. And the novelty of the appearances depended not on the presence of an unusual variety of organisms, but merely on their having enjoyed an unprecedented opportunity for coming forward. Under ordinary circumstances they would have been smothered—killed—by the effects of the *Bacterium lactis* and the other ferments that commonly develop in its wake. Such being my belief, I determined to make one more attempt. This time I used again the original twelve glasses, but charged them with greater care. I mentioned that a large proportion of these glasses of the second experiment had scarlet spots; and in the former experiment in the cow-house the great majority had orange spots, and those, as we have seen, were composed of heaps of granules. It occurred to me that one cause of failure might be this. Suppose one single group of such granules to exist, and to become disturbed and broken up in the process of transference to the glasses, it might vitiate the whole specimen of milk; therefore, instead of drawing up the milk into the pipette with a syringe and then expelling it, I determined to have it introduced as direct as possible into the little glasses. With this object I employed these two glass tubes, connected together, as you see, with a short piece of india-rubber tubing,

the wider tube being for the purpose of receiving the milk, the narrower to conduct it into the glasses. The glass tubes had been purified by a high temperature, and the piece of india-rubber connecting them, as it would not bear a very high temperature, had been boiled for half an hour. The same cow was taken out again into the open air, and this day the elements were in my favour. It had been a drizzly morning, and I might fairly hope that some of the multitudes of organisms existing in the little orchard might have been washed down and that the air might thus have been somewhat purified. I was also more careful in this respect. I got the dairywoman to milk the cow without drawing the hand over the teat, performing the operation by an action of the fingers in succession, so that the end of the teat should always be exposed. Her hands were washed with water, and the cow's udder also, and she having squirted out a little milk to wash away any organisms from the orifice of the duct, the glass cap which protected the larger tube from dust was removed and the end of the tube was held in the immediate vicinity of the teat; a few drachms were introduced, then the cap was readjusted, and then these little glasses were filled by the simple expedient of alternately relaxing and compressing with the finger and thumb on the caoutchouc, so that there was as little disturbance as possible of the organisms that might be supposed to be introduced in spite of my care. It is six weeks since this was done. At first sight, you might suppose, contrasting these appearances with those of the other tubes which were charged only three days earlier, that the milks of this last experiment were all pure. The truth is, all but two have organisms in them; but I may mention that all four had obviously organisms in them before I went for my trip on the Continent three weeks ago. On my return I found that in the course of the three weeks that had elapsed, two others had gone; but they already showed organisms which, though very pale and insignificant, were quite easily seen by a magnifier in such considerable mass that I felt sure they must have already been growing for a considerable time; and, therefore, in all probability those that still seemed to the naked eye and to the magnifier free from organisms were really so. Accordingly, two days ago I drew out milk from one of those that seemed to be still pure, and I had the great satisfaction of finding it not only perfectly fluid

and tasting perfectly sweet, with a perfectly normal reaction, purpling both blue litmus paper and red litmus paper—the normal reaction of perfectly fresh milk—but under the microscope I could not discover any organism of any kind whatever. Therefore, I think we are justified in saying that in unboiled milk as in boiled milk, provided, of course, the cow be healthy, there does not exist any constituent having the power of giving rise to organisms or producing the lactic or any other fermentative change.

This, gentlemen, was the first step of the investigation: to the second I must beg your special attention, because I believe you will agree with me that it is by far the more important step of the two.

The object of the second part of the investigation was to find absolute evidence, if possible, whether the *Bacterium lactis* was or was not the cause of the lactic fermentation. It occurred to me that, if we could estimate with some degree of accuracy the number of bacteria present in a given quantity of souring milk, and then if we were to dilute the milk with a proportionate quantity of boiled water, we might have the diluted milk so arranged that every drop with which we should inoculate a series of glasses of boiled milk might contain, on the average, one bacterium; and if we should do so, as it would be practically certain that the bacteria would not be distributed with absolute uniformity, we should expect that we might have, as the result of these various inoculations, some glasses with the *Bacterium lactis,* and some without it; and, if it should turn out that all those glasses which contained the *Bacterium lactis* underwent lactic fermentation, and, on the other hand, those glasses which were free from bacteria had no fermentation, that would prove the point; as, I think, you will agree with me, when we come to discuss the matter at a little more length after we have all our facts before us. Well, how were we to determine the number of bacteria existing in the liquid? This was done in a simple manner. A circular covering glass, just half an inch in diameter, was used. Of course, we know how many square thousandths of an inch there are in the area of this little glass. We also know by the micrometer how many thousandths we have across the field of our microscope, and, therefore, by calculation we know how many square thousandths there are in our field,

and thus we can tell how many fields there are in the covering glass. To measure the liquid, I used this little syringe, with the piston rod in the form of a screw, on which revolves a disc, graduated for 100ths of a minim; by which means you can, with perfect precision, emit 1-100th of a minim, or 2-100ths, or any number you choose. I found that 2-100ths, or 1-50th, exactly occupied the covering glass; so that, when it was put down upon a glass plate, with 1-50th of a minim interposed, the rim of fluid round about the covering glass was not one-quarter of the diameter of the field, using the highest magnifying power; so that practically the liquid was all under the covering glass. I knew, therefore, that there was 1-50th of a minim under the covering glass. If, then, I counted how many bacteria there were in a field, taking a number of different fields and striking an average, I could ascertain how many bacteria there were on the average in a field; therefore, by calculation, how many there were under the covering glass; or, in other words, how many there were in the 1-50th of a minim; and, consequently, I knew how much boiled water I ought to add in order that the drop, of whatever size I might wish it to be, should contain, on the average, one bacterium, and one only. This being done with a particular specimen of souring milk, I found that it was needful to add no less than one million parts of boiled water to the milk to ensure that there should be rather less than one bacterium, on the average, to every drop. Then with drops of that size I inoculated five glasses of boiled milk, and the result was that out of the five only one curdled; but one did curdle and soured, and that one had the *Bacterium lactis* in abundance; the others did not curdle, underwent no fermentation whatsoever, and had no bacteria in them. You may say, perhaps, "How was it that there were none of these numerous different organisms and fermentations that you have been showing us?" Simply for this reason, that although many other kinds of organisms undoubtedly did exist in the milk, yet most of them were in exceedingly small proportion to the *Bacterium lactis,* so that you might have searched, perhaps, for a whole day, with the high power of the microscope which it was necessary to use, and never discovered one. We are apt to forget how difficult it is to find these minute objects with high powers of the microscope, unless they are very numerous indeed. Therefore, when

we came to dilute the milk with a million parts of water, the chances of getting anything but the *Bacterium lactis* were exceedingly small. It was with reference to the *Bacterium lactis* that the dilution had been made, and not with reference to these other organisms relatively so rare. It happened that we saw in the souring milk before making the dilution that there was another kind of bacterium present, a moving kind different from the *Bacterium lactis;* it was in every field, but not nearly so numerous as the *Bacterium lactis,* and, consequently, it did not occur in the one milk that curdled.

Now, therefore, we had every reason to hope that we had got the ferment pure, and thus we had the opportunity of performing other experiments; and the last experiment that I shall mention is this. Having induced the lactic fermentation in another glass of pure boiled milk by means of our presumably pure ferment, and estimated the number of bacteria per minim, I diluted with boiled water accordingly and then proceeded as follows :—These five covered test-tubes which you see before you, containing boiled milk in their lower part, were inoculated each with a drop calculated to contain two bacteria; these other five similar test-tubes were inoculated each with a drop calculated to contain one bacterium; these five liqueur-glasses were also inoculated with drops each calculated to contain one bacterium; and one other liqueur-glass with a drop calculated to contain four bacteria. The result was that the specimen with the drop calculated to contain four bacteria soured and curdled in a few days; and all these five calculated to have two bacteria to a drop curdled also in a few days. The milk, you see, is perfectly solid. You will also observe that no change has taken place except the lactic fermentation, no *Oidium lactis* has grown, and no other alteration has taken place; it is as pure in whiteness as when it was first coagulated. I may here mention that, although all these glasses of milk coagulated, they did not do so at the same time. There was a time in the twenty-four hours during which the coagulation went on, in which I hoped that some of them were going to be permanently fluid, implying, as you would expect, that the particles of the ferment were not uniformly distributed; some had more than others, though each happened to have at least one. But, of the five test-tubes calculated to have only one bacterium on the average to each inoculat-

ing drop, three have remained fluid, and so have two of the liqueur-glasses; so that, of the ten calculated to have on the average one bacterium each, exactly five, it so happens, have remained fluid without any curdling. I may consider myself somewhat fortunate that I have succeeded in bringing these specimens all the way from Edinburgh in this condition. I will now deprive this one of the protection in which it has hitherto lived. [Mr. Lister, having removed the glass shade and glass cap from one of the liqueur-glasses, proceeded to drink part of its contained milk.] It is perfectly sweet. It has a slight flavour of suet, which M. Pasteur has described as resulting from the oxidation of the oleaginous material of the milk. If any gentleman likes to taste it after the lecture, he can do so.

Let me note this curious circumstance, that, of those specimens which did coagulate, those in the tubes coagulated considerably earlier than those in the more open vessels. At first, it seemed as if, for some strange reason, those in the open vessels were going to remain permanently fluid—even that which had, according to the calculation, four bacteria to the drop. I presume this is to be explained on the same principle as Pasteur has explained a corresponding fact with regard to the yeast plant. He has shown that, if a saccharine solution be put in a very thin layer in an open vessel with yeast, the yeast plant develops very rapidly, but very little fermentation occurs; on the contrary, if it be put into a deep vessel, the development of the yeast plant does not go on so rapidly, but more fermentation results. He explains the fact in this way: that the yeast plant requires oxygen for its nutrition; if it gets it easily, as it does in a shallow vessel in the air, it produces comparatively little effect in breaking up the sugar into its constituents, and vice versa. So here, in the test-tubes the carbonic acid accumulated, supposing any to exist, as in a well, and the *Bacterium lactis* had but little opportunity for getting oxygen. Accordingly here, just as in M. Pasteur's experiments with a sugary solution with yeast in a deep vessel, the *Bacterium lactis* produced more rapidly its fermentative effect.

But this, you say, is assuming that *Bacterium lactis* is the ferment. Now we are coming to that point. But first I have to mention an additional fact. For the satisfaction of others rather than for my own, I went through the laborious process

of investigating portions of the contents of all these vessels; and I found that, in every one in which the lactic-acid fermentation had taken place, where there was curdling and souring, the *Bacterium lactis* was present; and in no instance in which there was no lactic fermentation was any bacterium of any sort to be discovered. I believe that fact demonstrates that the *Bacterium lactis* is the cause of this very special lactic fermentation. Let us assume for a moment that there did exist some other material besides the *Bacterium lactis* in the milk capable of causing the fermentation; that the lactic ferment was not the bacterium at all, but some chemical ferment. First of all, you will please to observe that we have from this experiment absolute evidence that the ferment, of whatever nature, is not in solution, but in the form of suspended insoluble particles. If the ferment had been in solution, every equal-sized drop of the water of inoculation would have produced the same effect. The fact that some drops were destitute of the ferment proves that that ferment was not in a state of solution. That is absolutely demonstrated. Now, suppose we admit, for the sake of argument, that the lactic-acid ferment consisted of particles of some non-living substance, capable of self-multiplication as rapidly as the bacterium, but not living; a strange hypothesis, no doubt—but suppose we assume it. Suppose we admit that the true lactic ferment and the *Bacterium lactis* were merely accidental concomitants of each other, it would be absolutely inconceivable that these two accidentally associated things should be present in exactly the same numbers. And yet, according to the hypothesis, such would be only another mode of stating our observed fact, which amounts to this, that wherever there was a fermentative particle there was a bacterium, and wherever there was a bacterium there was a fermentative particle. But, suppose you admitted that—that there were exactly as many of the *Bacterium lactis* as there were of the hypothetical true fermentative particles—suppose you admitted that inconceivable thing, I say it would be again inconceivable that, if mutually independent, they should accompany one another in pairs, that invariably where there was *Bacterium lactis* there should be a ferment particle, and where there was no *Bacterium lactis* no ferment particle. That would be a thing as inconceivable as the other. Therefore, we have two inconceivables, one of which would have been

sufficient to show that we cannot admit any other hypothesis than that *Bacterium lactis* is the cause of the lactic-acid fermentation.

But the experiment tends to even more than this. Where we find the effect so exactly proportioned, as regards the number of glasses affected with fermentation, to the adult bacteria that we count, we are led to infer that this particular bacterium, at all events, has not any spores—that there are no spores existing in addition to the bacteria. People seem often to assume that bacteria must necessarily have spores or germs. It seems to me an unlikely thing that they should. They are, as it were, a generative apparatus *per se,* they are constantly multiplying; why should they have spores? I do not say that bacteria may not have spores. There are very different kinds of bacteria; some may have spores, and some may not; but this sort of result seems to indicate that this particular bacterium has no spores; at least, in the condition in which it exists in souring milk; because, if we had, besides the bacteria that we can count, spores of bacteria disseminated through the liquid also, we should have the effect more than in proportion to the bacteria that we have counted. The only fallacy here is that it may be that the bacterium has not been diffused uniformly through the milk. Therefore, I do not say that in this case it is absolutely proved. But, at all events, this experiment gives us a line of inquiry, by means of which we may probably settle that point with regard to any individual case of bacterium. This, however, is a point I do not desire now to insist on; but what I do venture to urge upon you is, that you will seriously ponder over the facts which I have had the honour of bringing before you to-day; and, if you do so, I believe you will agree with me that we have absolute evidence that the *Bacterium lactis* is the cause of the lactic-acid fermentation. And thus I venture to believe that we have taken one sure step in the way of removing this important but most difficult question from the region of vague speculation and loose statement into the domain of precise and definite knowledge.

AN ADDRESS ON THE PRESENT POSITION OF ANTISEPTIC SURGERY.

DELIVERED BEFORE THE INTERNATIONAL MEDICAL CONGRESS, BERLIN, 1890.

Collected Papers, vol. ii, p. 332.

What shall be chosen for the last of this somewhat disconnected series of papers? With reluctance we pass over the address on Primary Union given to the International Medical Congress of 1881 in favour of that delivered nine years later at the meeting of the same Congress in Berlin. The two hang more or less together, but the latter is shorter, and gives Lister's final views on the theory and practice of aseptic surgery.

One of the most unreasonable objections urged against the antiseptic system was that Lister, especially at first, modified his dressings so frequently. Surgeons seemed to assume that the discovery of a new principle ought at once to lead to a perfect method of applying it. Probably most of them mistook the method of application for the principle, if indeed they appreciated that any new principle was involved; but there was, of course, always a thoughtful minority who never fell into this error.

A much greater difficulty arose when it became clear that the original bald statement of the germ theory as

applied to septic diseases was in some respects deficient and in others not true. When, for example, the air was proved not to be the most important vehicle for septic germs, and when bacteria were found in unopened abscesses, and harmless cocci in wounds that were following a "typically aseptic course," misgivings were felt even by thoughtful men; and when Metchnikoff's discoveries were announced and "aseptic surgery" found wide acceptance, it looked to some as if the last planks of Lister's original platform had fallen away.

In the following address we have his own explanation of his early successes and misconceptions and of the many changes in his methods in the light of the accumulated knowledge of the preceding quarter of a century —a short time for so portentous an advance.

He had been thinking over what he should say to this world-congress for months, and especially brooding over it for a few days at St. Margaret's Bay before starting; but for all that the final draft was finished—if it ever was finished—in a desperate hurry; writing, writing in the train between Calais and Cologne, in the hotel garden at Cologne, and half through the night at his Berlin hotel. It was delivered on a broiling August afternoon, in a huge circus lighted by gas and electric light, and with little or no ventilation, but it seems to have been well received by what he called "a kind audience." It was appropriately followed by an address from Koch on the progress of bacteriology, spore formation and vaccine therapy. The mere mention of Koch's name and of the subjects he was discussing are enough to remind us of the ground that had been traversed since Lister published his first paper on Antiseptics in 1865.

AN ADDRESS ON THE PRESENT POSITION OF ANTISEPTIC SURGERY.

Delivered before the International Medical Congress, Berlin, 1890.

Mr. President and Gentlemen,—At the International Congress in London, 1881, Robert Koch demonstrated in King's College his then new method of cultivating microbes upon solid media. The illustrious veteran Pasteur was present at the demonstration; and at its conclusion exclaimed, "C'est un grand progrès, Monsieur." How vast have been the extensions of our knowledge which have resulted from that great step in advance! Of these none perhaps have been more striking than Koch's own brilliant discovery of the cholera microbe—picked out with unerring precision by his beautiful method from among the multitude of bacteric forms that people the intestinal contents, and grown and studied with as much definiteness as if it were a cabbage or a rose.

But while we have during the last nine years learned so much more of the nature and habits of the micro-organisms which invade our bodies, a new and surprising light has been thrown within the same period upon the means by which the living animal defends itself against their assaults. This we owe to the eminent naturalist Metchnikoff, who, having long carefully studied intracellular digestion in the amoeboid cells which form the main mass of the bodies of sponges and other humble organisms, was prepared to observe and rate at its true value an analogous process in the wandering leucocytes of vertebrata. He found that these migratory cells, with whose amoeboid movement we have been long familiar, feed also like amoebae, and while almost omnivorous in their appetites, have a special fondness for bacteria; taking them into their protoplasmic substance and digesting them, thus preventing their indefinite propagation among the tissues. The cells which exercise this devouring function he termed phagocytes.

Various objections have been urged against Metchnikoff's views; but so far as I am able to judge, he has met these effectively by his masterly series of researches; and his observations have been confirmed and extended by several independent investigators.[1] For the sake of those among my audience who may chance not to be familiar with Metchnikoff's work, I am tempted to relate briefly some of his experiments. The green frog, below the temperature of 20° C. (68° Fahr.) is incapable of taking anthrax: the bacilli of that disease cannot grow when introduced under the skin of that animal. To what was this immunity of the frog to anthrax due? Were its juices an unfit pabulum for the microbe, or was the phagocytic action of its leucocytes the explanation? In the hope of solving this question, Metchnikoff formed a tiny bag out of the pith of the reed, and having placed in it some spores of anthrax, closed the bag and inserted it beneath a frog's skin. The pith wall of the bag allowed the animal's lymph to penetrate by diffusion, but excluded the leucocytes; and the result was that the spores sprouted and grew into luxuriant threads of anthrax in the lymph, which was thus proved to be a suitable medium for the growth of the bacillus. Meanwhile under another part of the skin of the same frog had been placed a small piece of the spleen of an animal that had just died of anthrax and contained the microbe in its most virulent form; but there, the leucocytes having free access, no growth occurred.

Another experiment on the same principle was still more instructive. It consisted in introducing the spores of anthrax into the anterior chamber of the eye of a frog, which, as we have seen, is naturally insusceptible of the disease; and also into that of a sheep and of a rabbit rendered insusceptible artificially by "vaccination" with Pasteur's attenuated virus. The aqueous humour of the healthy eye contains few if any leucocytes to interfere with the perfect transparency essential to vision. Accordingly, the spores sprouted and grew for a while freely in the anterior chamber. Meanwhile, the growth of the bacillus occasioned irritation to the eye, resulting in the immigration of

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[1] See, for example, Dr. Tchistovitch, *Annales de l'Institut Pasteur*, 25 juillet, 1889, and Dr. Armand Ruffer, *British Medical Journal*, May 24, 1890.

a constantly increasing number of leucocytes, producing turbidity and, in time, hypopion. If a drop of the aqueous humour was withdrawn at an early period after the commencement of the experiment, and examined with the microscope, it was found to contain anthrax bacilli, some of them free in the liquid, but others enclosed in the bodies of leucocytes. But a drop taken after a longer period had elapsed showed no free bacilli, all being now within the leucocytes, and exhibiting signs of degeneration in various degrees as the result of their advancing digestion. Finally the anthrax disappeared entirely and the eye cleared up, the animal in all cases remaining healthy, although inoculation into the aqueous humour proved a peculiarly deadly mode of infecting a susceptible animal.[1]

Here we see that the inflammation excited by the microbe becomes, through the medium of the leucocytes, the cause of its destruction. How little can the lamented Cohnheim have dreamed that his observation of the emigration of leucocytes in inflammation would prove to have so far-reaching a bearing upon the pathology of infective diseases!

I have brought before you two samples of the kind of evidence upon which the phagocyte theory rests, and if we accept it, as I believe we must, it serves at once to explain much that has hitherto been mysterious in the relations of micro-organisms to wounds. Take, for example, that which the surgeon makes for the cure of hare-lip. Its posterior edge is perpetually bathed with the saliva, which contains many kinds of septic bacteria. But these do not enter and people the fibrine that glues together the cut surfaces, as they infallibly would do if those surfaces were composed of glass or any other chemically inert material destitute of life. It has long been very evident that the living tissues exerted a potent influence in checking bacteric development in such a wound; but what was the nature of that influence? This used to be an enigma, but now receives its natural explanation in the phagocytic action of the cells that crowd the lymph soon after its effusion.

At the London Congress I brought forward an experiment which proved that a blood-clot within the body may exert a powerful anti-bacteric agency. I will not repeat the details of

[1] See *Annales de l'Institut Pasteur*, 25 juillet, 1887, pp. 326, 327.

that experiment further than to say that a very small piece of linen cloth soaked with putrid blood was mounted by means of silver wire in the interior of a short glass tube open at both ends, which was slipped into the jugular vein of a donkey, and kept in position between two ligatures. After t o days the venous compartment was removed, and the coagulum within it investigated. In and near the glass tube it was in a state of advanced putrefaction, as was indicated by its foul odour and greatly altered appearance; and microscopic examination showed that it abounded with bacteria. But near the wall of the vein it looked to the naked eye like a recent clot; I could not detect in it any putrid odour, nor could I discover bacteria with the microscope.[1] Stained sections of these outer parts of the coagulum, made after hardening in alcohol, showed great multitudes of cells differing from one another in size and other characters, just as is often the case with Metchnikoff's phagocytes. I supposed that these cells must have been in some way or other the anti-bacteric agents, but how, I could not imagine. The phagocyte theory clears up the mystery.

By means of this same theory we can account for what would otherwise have seemed to me incomprehensible—the use, without evil consequences, of silk ligatures which have not been subjected to any antiseptic preparation. We learn from the experiments of Ziegler and others that leucocytes soon penetrate very thin spaces between plates of glass or other chemically inert foreign bodies inserted among the tissues. And we can understand that they may creep into the intervals between the fibres of a silk thread and destroy any microbes that may have lodged there before they have had time to develop serious septic mischief. But there must surely be a limit to the thickness of the threads. No one, I imagine, would feel justified in leaving in the peritoneal cavity an unsterilized cord as thick as a finger. Dr. Bantock, whose remarkable series of successful ovariotomies may seem to justify his practice, does not, I believe, prepare his ligatures antiseptically; and I understand that he uses, for tying the pedicle of a tumour, silk twist of so strong a nature that it can be trusted to bear the needful strain, with a diameter of

[1] See *Transactions of the London International Medical Congress; Collected Papers,* vol. ii, p. 275.

only about 1-30th of an inch. But it would surely be wiser to sterilize even so slender a cord. Who can say that septic mischief may not occasionally lurk in the ligature in a form which may baffle the phagocytes?

The success in abdominal surgery achieved by Bantock and Lawson Tait, without, it is said, the use of antiseptic means, proves a stumbling-block to some minds. But in truth the practice of these surgeons is by no means conducted without antiseptic precautions, nor would they, I am persuaded, desire that such an impression should prevail. Both are scrupulously careful in the purification of their sponges, and if there is one thing more important than another in the antiseptic management of wounds of the peritoneum it is the avoidance of impure sponges. Both observe the strictest cleanliness—which is surely an antiseptic precaution—for it owes its virtue to the fact that it presents the septic organisms in the smallest possible numbers and thus reduces their power for evil to the utmost that can be done by any measures that are not germicidal. Both these surgeons also wash out the peritoneum with water so as to get rid of coagula without injuring the peritoneal surface by rubbing it with sponges, and this is done in order to avoid the risk of sepsis in residual clots. The drainage of the peritoneum is another antiseptic measure, and Dr. Bantock, I am informed, has the sponges which absorb the serum wrung out of sulphurous acid, and changes them very frequently.

This is a department of surgery in which I have had but little personal experience. But I can see that while the measures to which I have referred are, so far as they go, highly valuable, it must be in itself a very desirable thing to avoid the direct application to the peritoneum of strong and irritating antiseptic solutions. But now that we are all agreed that microbes are the evil with which we have to contend, it is surely wiser to ensure by germicidal means their entire absence from our hands and instruments rather than trust to the most perfect cleanliness in the ordinary sense of the term. And if water is used for washing out the peritoneum, prudence seems to me to dictate that it ought to be freed entirely from living organisms, if this can be done without making it irritating. This object is, I believe, aimed at by Dr. Bantock by boiling the water before using it, but I would advise as more effectual an extremely weak solution of corrosive

sublimate, such as 1 in 10,000, which, as Koch has taught us, may be implicitly trusted as antiseptic, while it is not appreciably irritating and involves no risk of mercurial poisoning.

In general surgery, the direct application of strong antiseptic solutions is not attended with the same disadvantages as in operations in the peritoneal cavity. My practice for some time past has been to wash the wound, after securing the bleeding-points, with a pretty strong solution of corrosive sublimate (1 to 500) and irrigate with a weaker solution (1 to 4,000) during the stitching, and I have had no reason to complain of the results. To this, however, I must make one marked exception. When applied to the healthy synovial membrane of a joint, the 1 to 500 sublimate lotion produces inconvenient irritation, and therefore, when opening an articulation—as for suturing a transverse fracture of the patella—I abstain from the washing, and, as a substitute, have hitherto irrigated during the whole operation with the weak solution (1 to 4,000).

And yet I must confess that I have for a long time doubted whether either the washing or the irrigation was really necessary. These doubts have been raised partly by experiments—some of which I mentioned at the London Congress—which had proved to me that normal blood and serum, and even pus, were by no means favourable soils for the growth of microbes in the form in which they are present in the air—and partly by reflection upon the experience we had when we used the carbolic spray.

As regards the spray, I feel ashamed that I should have ever recommended it for the purpose of destroying the microbes of the air. If we watch the formation of the spray and observe how its narrow initial cone expands as it advances, with fresh portions of air continually drawn into its vortex, we see that many of the microbes in it, having only just come under its influence, cannot possibly have been deprived of their vitality. Yet there was a time when I assumed that such was the case, and, trusting the spray implicitly as an atmosphere free from living organisms, omitted various precautions which I had before supposed to be essential. Thus, in opening the pleura in empyema for the purpose of evacuating the pus and introducing a drainage-tube and afterwards in changing the dressings, I had previously applied over the opening a piece of cloth steeped in an antiseptic lotion to act as a valve and prevent the entrance

of air during inspiration. But under the spray I omitted the valve and allowed the air to pass freely in and out of the pleural cavity, although I used the spray at such a distance from the producing apparatus that it was dry and transparent, with the particles of carbolic solution necessarily widely separated from each other. And these particles cannot have been in more than instantaneous contact with much of the dust before it was drawn within the chest, and securely protected by the pus or serum there from any further action of the antiseptic. It is physically impossible that the microbes in such dust can have been in any way whatever affected by their momentary presence in the spray.

Yet we did not find our results in the treatment of empyema rendered worse by this false confidence in the spray. There are few more beautiful things in antiseptic surgery, as contrasted with the results of former practice, than to see the abundant purulent contents of the pleural cavity give place at once to a serous effusion, rapidly diminishing from day to day till, the opening being allowed to close, the pleura, restored to its healthy condition, resumes its normal function of absorbing gases; and, as the natural vacuum within it becomes re-established, the atmospheric pressure blows up the contracted lung, and brings it again into contact with the chest wall unimpaired in its dimensions. Such a case we had witnessed before the days of the spray, and such we continued to see during its use.

If, then, no harm resulted from the admission day after day of abundant atmospheric organisms to mingle unaltered with the serum in the pleural cavity, it seems to follow logically that the floating particles of the air may be disregarded in our surgical work; and, if so, we may dispense with antiseptic washing and irrigation, provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from other than atmospheric sources.

Since we abandoned the spray, three years ago, we have been careful to compensate for its absence, not only by antiseptic washing and irrigation, but by surrounding the seat of operation with widespread towels wrung out of an antiseptic solution. For the spray, though useless for the object for which it was originally designed, had its value as a diffuse and perpetual irrigator, maintaining purity of the surgeon's hands and their vicinity as an unconscious caretaker. But if besides the spray

we give up all washing and irrigation of the wound, our vigilance must be redoubled. Yet I believe that, with assistants duly impressed with the importance of their duties, the task would prove by no means difficult.

I have not yet ventured to make the experiment on any large scale, although I have long had it in contemplation. It is a serious thing to experiment upon the lives of our fellow men, but I believe the time has now arrived when it may be tried. And if it should succeed, then perhaps may be fulfilled my early dream. Judging from the analogy of subcutaneous injuries, I hoped that a wound made under antiseptic precautions might be forthwith closed completely, with the line of union perhaps sealed hermetically with some antiseptic varnish, and bitter was my disappointment at finding that the carbolic acid used as our antiseptic agent induced by its irritation such a copious effusion of bloody serum as to necessitate an opening for its exit; hence came the drainage of wounds. But if we can discard the application of an antiseptic to the cut surfaces, using sponges wrung out of a liquid that is aseptic but unirritating, such as the 1 to 10,000 solution of corrosive sublimate, we may fairly hope that the original ideal may be more or less nearly attained.

We have already made of late considerable approaches towards it. Our wounds being no longer subjected to the constant irrigation of the spray, and carbolic acid having given place to the less irritating, though more efficient, solutions of corrosive sublimate, serous discharge is much less than formerly, and less drainage is required. In many small wounds where we used to find drainage imperative we omit it altogether, and in those of larger extent we have greatly reduced it. Thus, after removing the mamma and clearing out the axilla, I now use one short tube of very moderate calibre, where I used to employ four of various dimensions. But it would be a grand thing if we could dispense with drainage altogether; without applying the very firm elastic compression adopted by some surgeons, which, besides involving the risk of sloughing of parts of low vital power, with the chance that it may after all fail in its object, proves often extremely irksome to the patient.

It remains for me to say a few words regarding the best form of external dressing. Some surgeons have thought that simplicity and efficiency may be combined in the maximum

degree by the use of cotton-wool sterilized by heat. But though it may be a simple thing to heat the wool appropriately by means of suitable apparatus in a public institution, for the ordinary practitioner it would be impracticable. And as regards efficiency, I need hardly remark that cotton-wool merely aseptic, can only exclude septic mischief when it is in the dry state. When it is soaked to its external surface with a copious discharge, it must be liable to become septic *en masse*. And however well we may succeed in the future in diminishing or abolishing discharge from wounds made by the surgeon, there must always remain cases in which it will occur in greater or less amount.

Contused wounds, for example, into which dirty material of one kind or another has been introduced before they are seen by the surgeon, must be purified by the use of powerful antiseptic means, and must, for a while, discharge freely. The same is to be said of cases in which we make the attempt, often with signal success, to restore an aseptic condition in a part affected with septic sinuses. Again, there are abscesses in which, in the present state of our knowledge, we cannot avoid the occurrence of considerable serous oozing, and in which a perfectly trustworthy antiseptic dressing is a matter of life and death. And whenever discharge is considerable, it is essential that the dressing be of a kind which will not permit the development of septic organisms in it, although it be saturated throughout; and this can, I believe, only be attained by the use of chemical antiseptic substances.

I have for some time past employed for this purpose a combination of the two cyanides of zinc and mercury, which appears to fulfil the requisite conditions of antiseptic efficacy and due storage of the agent in spite of free discharge, together with absence of irritating properties. Having already published on this subject, I will not detain the members of the Congress with details regarding it, further than to say that since the date of that publication Professor Dunstan, of the London Pharmaceutical Society, has devised means by which the substance can be prepared in a perfectly definite manner, and containing twice as great a percentage of the cyanide of mercury as that which we have hitherto used; and, as I have ascertained that the cyanide of mercury is the more important ingredient antiseptically, and also that its larger amount in Dunstan's material does not make the salt irritating, we may fairly regard the new pre-

paration as an improvement. And yet we have had no need to complain of this substance in the form in which we have used it hitherto. Those who have followed my practice at King's College Hospital during the year and a half in which this dressing has been employed will agree with me that we have secured a constancy of aseptic results which has more than ever justified the performance of operations once quite unwarrantable.

In thus referring to my own work, I do so, believe me, in no boastful spirit; but in the hope of stimulating some of those whom I address on this memorable occasion to more thorough earnestness in pursuit of the great objects of antiseptic surgery.

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