On pyæmia : read before the Medical Society of University College, November 25th, 1852 / by Joseph S. Gamgee, Esq.

To the Editors of the Edinburgh Monthly Journal of Medicine

ON

for the Author

PYÆMIA.

READ BEFORE THE MEDICAL SOCIETY

OF UNIVERSITY COLLEGE,

NOVEMBER 25TH, 1852.

BY

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[FROM THE "ASSOCIATION MEDICAL JOURNAL."]

PRINTED BY

T. RICHARDS, 37, GREAT QUEEN STREET,

LINCOLN'S INN FIELDS.

1853.

Convinced, as I avowed myself, when this paper was read, of its numerous imperfections, the fact of my now publishing it, may be held to signify a change in my opinion as to its value,—no such change however has occurred. It is only in compliance with the request of several friends that I send these papers to the press; compliance which, however, I should not be able to yield, were it not for the hope I entertain of one day aiding in giving to this important subject the developments it requires.

University College Hospital,
21*st February*, 1853.

ON PYÆMIA.

A CONSCIOUSNESS of the vastness and intricacy of the subject on which I have undertaken to submit to you a few reflections, and a feeling of my own inability to do it justice, constrains me to appeal to your indulgence; an appeal, the special need of which will at once be perceived, when I confess that I do not intend this communication to be a mere compilation of the ideas of authors on the subject of Pyæmia. The great discrepancies between them render it obvious that they are not all right; and my own clinical observations in thirteen cases force me strongly to suspect, that in many very important points the great majority of them are far from the truth. Thus believing, I should deem it most just to myself, and most consistent with my duty, to relate the histories of my cases, to analyse them, and then to point out in what respects my own observations agree with, or differ from the teaching of the day. But to carry this plan into execution, copious as my notes of cases are, a series of addresses, and not a single one, would be required; and hence I am forced to abandon it for another more expedient, though to me less congenial, and I fear to others less efficient scheme. Under existing circumstances, I must confine myself to presenting a sketch of the present state of knowledge among surgeons, especially among English surgeons, on the

subject of my thesis, and to giving a succinct account of the disease. In so doing, it shall be my special study to adduce from my note-book of cases as much evidence in support of my statements and of my criticism on accepted opinions, as time will permit.

Anxious as I am to be brief, I cannot abstain from making a few preliminary remarks.

Such is the present state and rate of progress of pathological science in general, but especially of surgical pathology, that even in the humblest of inquiring minds, a zealous desire to do somewhat to contribute to its advancement cannot but be awakened. If this be true in general, how true is it in this particular instance. Let any one look on a poor creature first seized with the pyæmic shiver: the violent shake of the whole body, the drawn and haggard features, the dim and sunken eye, the earthy look, the mental depression, the succeeding intense heat and profuse sweating, will at once indicate to him that something serious has happened. A succession of these attacks, the supervention of diarrhœa, and mental aberration, not unfrequently in a few hours hurl a previously stout and healthy man into eternity. Let the witness of this scene bear in mind that, but a very few years since, this disease was unknown; that there are very strong grounds for believing that it is among the most frequent of surgical diseases, and yields to none of them, if it do not exceed them all, in its fatality; that at the present day, to my certain knowledge, surgeons of eminence have shewn themselves ignorant of typical cases of this disease brought under their observation; that in the most recent systems of surgery, strong presumptive evidence is afforded in favour of the belief that their authors have exceedingly indefinite notions on the subject; that writers of monographs are not only remarkably at variance on some of the most important, nay, of the fundamental questions

involved in the subject, but have for the most part studied it in a manner which, if pursued for ever, will fail to lay bare the naked truth ; let these facts, I say, be borne in mind, and then I may hope to be judged fairly, when I state my intention to make this paper the commencement of a critical inquiry into the present state of knowledge of this disease, with the ulterior view of contributing something to its elucidation. In the criticisms which I have to make on the labours of others, it shall be my earnest endeavour to express myself in unobjectionable terms ; but should I, in the opinion of any one, fail in doing so, I trust that my intentions will not be misconstrued, however much I may fail in their fulfilment.

By pyæmia, I mean an affection almost invariably fatal, characterised by the rapid formation of purulent collections in different parts of the body, usually occurring as a sequel of suppurating wounds, but sometimes of spontaneous origin. While making use of the term pyæmia, which signifies an admixture of blood and pus, I do not intend giving in my adhesion to any particular doctrine as to the nature of the disease : its synonyms "purulent diathesis", "purulent infection", "purulent resorption", "purulent metastasis", are, like it, liable to the objection of being the expressions of theoretical opinions, rather than names signifying the correct nature of the disease.

As already stated in the definition, the great characteristic of pyæmia is the formation of purulent collections in different parts of the body. As peculiar features of these collections, I would call attention, firstly, to the great rapidity with which they frequently form, three or four days alone sometimes sufficing for the development of numerous abscesses in the lungs, and the collection of immense quantities of puriform fluid in the muscles and in the serous and synovial cavities. Secondly, to the asthenic character

of the accompanying inflammation. In exemplification of this character, I may state, that I have met with considerable purulent collections in the pleura, and in some joints, without having been led to suspect their existence by any complaint of the patient. Thirdly, to the very general distribution of purulent deposits over the body. Though no tissue is, I believe, exempt from them, doubtless it is that they are more frequent in some parts than in others. The lungs are their seat of predilection; next in order I should place the serous and synovial cavities; then the muscles, the liver, and other organs. I have in three cases met with metastatic abscesses in the prostate, a part in which no mention of their occurrence is made by the best writers. The fact that I have found them there so frequently, comparatively to the small extent of my experience, taken in conjunction with the fact that the prostate often escapes unexamined at autopsies, induces me to think it probable that abscesses of the prostate in pyæmia may be more frequent than authors have hitherto supposed.

As it is in the lungs that these purulent deposits are most frequently formed, it is necessary for me to dwell a little more at length on the characters which they present in these organs. They usually occur posteriorly and towards the lower part of the lung; but I have met with a metastatic abscess at the anterior edge and at the extreme apex, though the remainder of the upper lobe was perfectly healthy. Another very general character is, that the surface, rather than the deep part of the organ, is their seat of preference. On the surfaces corresponding to the interlobular fissures, and on the diaphragmatic surface, they are about as frequent as on the exterior.

They vary in size from a pin's head to a walnut, and present themselves under two different aspects: (*a*) As circumscribed collections of apparently laudable pus. (*b*) As solid

yellowish masses due to infiltration of the lung substance with pus, as proved in many cases by this fluid exuding from them on pressure. Frequently the lung substance for about half an inch around is of deep red colour, non-crepitant and sinks in water: in many cases again, it is only at a small part of the confines of the abscess that the red hepatization exists; and the lung tissue is not unfrequently found crepitant and otherwise healthy in the immediate neighbourhood of one of these puriform deposits.

As to abscesses in the muscles, it has been stated by Nélaton* that they are comparatively rare; and that, when they exist, the muscular fibres are sharply cut around the pus-containing cavity, which appears to be the result of the softening down into pus of the fibres amid which it exists. With reference to the first part of this paragraph, I am disposed to doubt its accuracy, and to regard the muscles as a rather favourite seat of these deposits; but possibly the frequency with which I have found them there is the result of accident in my brief experience: certain it is, however, that the pus is not always collected in circumscribed cavities, for it is sometimes diffused pretty uniformly between the interfascicular cellular tissue, without the least tendency to circumscription, much less to the formation of a cavity.

It may have been observed, that I have frequently made use of the term puriform fluid, instead of pus; and as my doing so has not been the result of caprice, it is necessary that I should state my reason. It was in the summer of 1851, that I first heard of the purulent depositions in pyæmia being destitute of true pus-corpuscles, and abounding in granular corpuscles, in which no such thing as the characteristic nucleus of the pus-cell was to be seen. This was made known to me by my friend Mr. Joseph Lister, as

* Elémens de Pathologie Chirurgicale, t. 1er, p. 162.

the result of his observations in one of the cases on which he wrote for the Fellowes' Clinical Prize, in the summer of last year. I have since discovered that similar observations had been made by others ; and I have observed the same fact myself. To convince you of its importance, I need only tell you, that on the character and size of the pus-corpuscules, a theory as to the mode of formation of the abscesses in pyæmia has been formed, and it will at once be perceived that the discovery of cases of this disease with corpuscles, other than those of pus, is a serious invalidation of the basis of such theoretical teaching.

In stating that the great anatomical character of pyæmia is the formation of purulent collections in different parts of the body, it may appear that I have given but a partial statement of the anatomical essence of this disease, in having omitted mention of the state of the blood. I have done so, because it appears to me that the consideration of this point may for the moment be profitably deferred.

Symptoms. Though necessarily incomplete, this pathological sketch will serve as a preparation for the more ready appreciation of the value of the symptoms.

A patient with a suppurating surface, doing well in every respect, is seized with a fit of shivering, which passes off in about a quarter of an hour, and is succeeded by more or less intense heat. Gradually the bodily temperature falls to its normal standard, and the patient is perhaps left in as good a condition as he was in prior to the rigor. But a few hours, rarely two or three days, elapse before a similar rigor and succeeding heat are manifested; and then, if not at the first, is the patient's appearance markedly changed—the eyes are sunken, and the features drawn.

Quite suddenly, notable emaciation appears to have occurred ; and ere long, the skin and conjunctivæ acquire a more or less icteric tinge. In rarer cases, the face becomes

of leaden hue, the respiratory and cardiac movements are much accelerated, the breath has a peculiar odour, the wound presents an unhealthy appearance, the tongue dries, sordes accumulate on the teeth, symptoms of pleurisy and of articular affections may supervene, and abscesses form in different parts of the body. As the bodily weakness increases, the mental faculties become impaired, and life ends.

This narrative of symptoms may be quite applicable to a particular case, though I have compounded it from a number of histories, and intended it as a general outline, rather than as a detailed account. It is now time to examine more particularly into the diagnostic value of some of the most prominent semeiological characters to which I have called attention.

1. *The Shivers.* Occurring, as this sign does, in a variety of affections, it might *a priori* be regarded as of little aid in diagnosis. The reverse is the case; yet its importance must not be exaggerated. It is usually severe, and associated with remarkable depression of spirits and alteration of countenance. It rarely exceeds a quarter of an hour in duration, and usually occurs at periods varying from twelve to thirty-six hours. In two cases, the patients have informed me that the violent shaking of the body has been unassociated with a feeling of cold. In some cases, the shiver is a symptom of very little value. I have seen a case of compound fracture of the leg, in which, on cadaveric inspection, metastatic abscesses were found in the lung. During its whole progress, the patient only shivered once, and then but slightly. Again, the shivers may merely be the forerunners of an attack of erysipelas; and it is only by the coexistence or absence of other signs that the diagnosis can be established.

11. *Odour of Breath.* The merit is usually attributed to

Bérard of having first called attention to a peculiar odour as emanating from pyæmic subjects. The best idea I can give of this smell of the breath, is by comparing it to the smell of sweetish liver; so peculiar is it, that its value in diagnosis is very considerable. In one well marked case of the disease, which recently came under my notice, the breath had not, at any stage, the peculiar odour in question. On the other hand, it existed markedly in a man affected with prostatic abscess; and in another who died from extravasation of urine, after the operation of lithotrity: in both of whom I failed to discover, after death, the signs of purulent infection. These facts, it must be admitted, notably decrease the value of the breath's peculiar odour in diagnosis; still it is certainly of much worth in many cases. Its precise import will only be susceptible of determination when pyæmia shall have been clinically studied as it ought to be.

III. *Appearance of the Wound.* As a rule, the appearance of the constitutional symptoms of the purulent affection is attended with a marked deterioration in the appearance of the wound. If partial union have occurred, the bond breaks down, and the previously pink, compact, and pus-secreting granulations become pale and flabby, and pour out a thin, scanty, and offensive discharge. At other times the wound looks pale, and does not pour out any secretion; at others, again, it acquires a black colour as it dries. The neighbouring integument may be of perfectly healthy colour, or the seat of a faint blush; and when suppurative phlebitis is the cause of the affection, its signs are manifest in the neighbouring veins. Sometimes, however, the appearance of the wound very sensibly improves after pyæmia has set in; but the improvement is not durable. I have seen two cases of this kind.

IV. *Pulse and Respiration Ratio.* The number of the

heart's beats, and of the respiratory movements, is, we have observed, much increased in this affection. How far the ratio is altered, known facts do not enable us to state; though their scantiness, conjoined with the fact that stethoscopic examination is, as a rule, difficult in these cases (the cause of this difficulty being the extreme restlessness of the patients), suggests the importance of further inquiry, as a means of determining the occurrence of chest mischief.

v. *Colour of the Skin.* In the greater number of cases, the skin at an early stage acquires an icteric tinge, which is also noted in the conjunctivæ. That this peculiarity of colour is not owing to organic disease of the liver, is proved by the fact that, in all the cases which I have seen, this organ happens to have been healthy. Of the leaden hue which sometimes occurs in this disease, one well marked instance has fallen under my notice. It occurs at an advanced stage, and is doubtless due to imperfect arterialization of the blood, in consequence of extensive chest mischief. This existed in my case.

Causes. I have stated in the definition, that pyæmia is a disease usually occurring as the result of wounds, but sometimes of spontaneous origin. In stating that the disease may sometimes originate spontaneously, I am advocating a doctrine which has few supporters, but which, nevertheless, I believe to be correct. I have seen two cases of the kind; and as perhaps this is one of the most important questions which we shall have to discuss, I shall read the history of one of them, and state the reasons for my regarding it as an instance of purulent infection occurring independently of a wound.

Case. Thomas Harley, of lymphatic temperament, aged 20, single, a French polisher, native of London, was admitted into University College Hospital, 30th October, 1852. He had never heard of consumption, cancer, or gout in any of

his family, but his father was subject to rheumatism for several years before death, the cause of which was unknown. He had been a very healthy person, and had never laid up for illness. He was of very temperate habits; slept in an airy and dry back room; his working-place was warm and dry; he had never had rheumatism, scarlet or typhoid fever.

History of present Illness. About four months ago, the glands in the left submaxillary space enlarged, and continued so ever since. He continued in good health up to Sunday, 24th October, when the left side of the face began to swell, and he felt difficulty in swallowing. The swelling was painful, and increased in size up to Tuesday, 26th October, since when it had diminished, and been painless. The difficulty of swallowing disappeared on the 29th. On the evening of the first day of attack, he had a severe shivering fit for a quarter of an hour, after which he became very hot. He shivered every evening afterwards. He had been very thirsty. The bowels were open five times on October 29th, after medicine. On the day before admission, for the first time, he felt pain in the muscles of the calves and thighs, which continued in these parts ever since. He had also pain in the muscles of the body generally, and in the left knee and ankle-joints. For several days, even when lying still, he had had severe pain in the back of the head.

Present State. Three o'clock, P.M., 30th October. The pulse was 112, of pretty good size, but weak and soft; the respirations 36; the skin hot and dry. He had very severe pain at the back of the head when he moved it, not otherwise, and a good deal of pain in the eyelids; none in the eyeballs. The face was pale, except over the swelling on the left side, where the skin was red. There were slight sordes, both on the upper and lower teeth; the tongue was moist all over, vividly red at the edges and tip, and covered

with pasty brown fur over the remainder of its surface; the papillæ at the tip were very prominent; the lips were dry, the lower one brownish. The breath was not peculiar. He had no cough, nor pain in breathing. There was a good deal of tenderness at the epigastrium (he vomited on the previous morning), not in other part of abdomen. There was no gurgling in the right iliac fossa; nor spots on anterior surface of abdomen or chest.

Limbs. There was pain in the left biceps muscle, but none in any other part of the left arm; also in the muscles of the right arm and forearm, increased by pressure; there was no pain in the joints of these limbs, nor in the muscles of the back, but the spine was tender from the middle of the dorsal region downwards. Great tenderness was manifested on grasping the muscles of the thighs, especially the right one; there was still greater pain in the calf, and some, but less, in the muscles in the front of the leg and on the insteps; but none in the joints of the lower limbs. Some tenderness was however felt on pressing over the inner malleolus. No redness or swelling was noticed in any of these painful parts. There was not, nor had there been, any wound in any part of the body.

Swelling of Face and Neck. On the left side of the face and upper part of the neck, was a considerable swelling, over which the skin was red and hot. The limits of this swelling were not defined; it extended upwards to the cheek, backwards to the anterior border of the sterno-mastoid muscle, forwards almost to the chin, and downwards to the level of the upper border of the thyroid cartilage. Nothing was seen of this swelling inside the mouth. The lower jaw felt healthy, and the mouth was opened without pain, though with a little mechanical impediment, owing to the size of the swelling, which felt hard everywhere, except at the anterior and lower part, where very distinct fluctuation

was felt. On making an incision in this situation, exit was given to about a drachm of pus mixed with blood. A little lint was introduced into the lips of the wound, and hot linseed poultice applied all over the swelling, to be renewed every six hours.

11, P.M. He had had two copious stools, after a dose of castor oil. On removing the poultice and lint from the aperture, and on pressing in the neighbourhood, a good deal of laudable pus escaped. The swelling, heat, redness, and pain were much diminished.

October 31st, 10 P.M. Pulse 120, full, and rather bounding. Respirations 28. The skin was hot and dry; the tongue brown in the centre, but less so than yesterday : the tip and edges were vividly red. He had no motion this day. He felt very weak, and complained of great pains in the loins, and also in the joints and muscles of the limbs. The abscesses discharged perfectly laudable pus, in small quantity. The left biceps was much swollen, and the skin covering it was hot and red. The muscles on the back of the right forearm were in precisely the same condition. At the under part, and almost at the middle of the right thigh, was a diffused swelling, about the size of a fist. It was hot and very tender, and the skin over it was reddish. In front of the left ankle, the skin was red; and the redness extended about half way up the front of the leg; without, however, any swelling. There was no pain in the course of the large veins ; he had great thirst; no peculiar odour of breath; no cough. Hot fomentations were ordered to be applied to the swollen parts.

November 1st, 10, A.M. The swelling in the parts of the limbs mentioned in last report, was on the increase. Pulse 128, tolerably full, soft. Respirations 36. The respiratory murmur was perfectly healthy all over the front of the chest. The patient was in such pain, that the back could not be examined.

Midnight. The skin was hot and moist; but the patient said that he felt cold. He was evidently somewhat light-headed. The nurse informed me that, for the last two days, he had talked incoherently in his sleep. He had no peculiar odour of breath: the tongue was moist, dirty white furred, but much less so than at admission: the pupils were much dilated, and fixed.

November 2nd, 11 A.M. The pulse was 150, of medium fulness, soft; the skin hot, and bedewed with perspiration; the pupils extremely dilated, and slightly, but very slightly moveable, when a lighted candle was approached to the eyes; the face much drawn; the expression wild and haggard. He was extremely restless, and constantly chattered incoherently and somewhat indistinctly, about a great variety of matters. The bowels had not been opened since yesterday. There was no discharge from the abscess on the left side of the face. The redness in front of the left ankle was not so great as it had been; but the redness in front of left leg was greater than yesterday, and was now associated with heat and swelling. The signs of phlegmon under the right thigh had diminished; those in the right forearm and left biceps remained the same. There were now also redness, heat, and swelling, on the back of the left forearm. About the centre of the wrist of the right forearm, I this morning had felt apparently distinct fluctuation; but no matter escaped on a puncture, half an inch deep, being made with a narrow bistoury. There was no peculiarity in the odour of the breath.

A drop of blood taken from the back of the right hand and tip of the right index finger (both parts apparently healthy), showed a very large excess of white corpuscles on the addition of acetic acid. I asked Dr. Jenner to examine this blood, before I told him of the case. He kindly did so, and said that there was a large excess of white corpuscles; and, in answer

to my question as to whether there were any pus-corpuscles, replied in the negative. I had already made up my mind to the same effect. The white corpuscles in no way differed in character from those of healthy persons: the great majority of their nuclei were of a horse-shoe shape; others were centrally grouped, but their divisions were separated from each other, and not aggregated into a central mass, as the nuclei of pus-corpuscles.

4, P.M. There was no sensible change since the last report, except that a good deal of perfectly white foam escaped from the mouth, and that the powers of speech had become much impaired.

7½, P.M. The nurse informed me, a few minutes before this time, that the man had just died. Her account agreed with that of several gentlemen who witnessed the case, to the effect that the face had acquired a decidedly purplish or bluish tint shortly before death.

Examination of the body, at 9, A.M., Nov. 4th. Cadaveric rigidity was generally well marked, as was also the livid discolouration of the posterior surface of the body. This livid discolouration did not advance uniformly on the sides of the trunk; but those parts, as well as the neck, clavicular regions, and arms, were mottled livid. The left arm was less mottled than the right; and whereas the skin was perfect on the latter, the cuticle was readily detached in large shreds from the inner aspect of the former, which, moreover, was somewhat, but not much, larger than its fellow. The right forearm was somewhat larger than the left: both were evidently rather larger than natural. They pitted slightly on pressure. Their palmar aspect was livid (both had been in a prone position). The dorsal aspect of the right arm was of a yellowish white colour; that of the left was slightly mottled. The external surface of the left leg was decidedly somewhat larger and paler than the right.

On looking at, and manipulating the posterior aspect of the thighs, no perceptible difference was found. Nothing peculiar was noticed on looking at and manipulating the joints.

Thorax. On removing the sternum, the parts appeared perfectly healthy at the first view. There was no fluid in either pleura; no adhesions on the left side; but some firm old ones on the right. Some blood, very carefully collected from a puncture made in the superior vena cava, which had been previously dissected out, showed, when acted on by acetic acid, the white corpuscles in excess, but by no means so much so as they appeared in the blood examined during life. Among the white corpuscles, I saw some with nuclei which I could not distinguish from the nuclei of pus, and which were certainly different from those which I saw in this man's blood during life. Dr. Jenner also acknowledged the similarity, but did not believe that the suspicious looking nuclei were those of pus-globules. My friend Mr. Lister happened to call on me while I had this blood in the field of the microscope. Without giving him any reason to suspect the nature of the case I was inquiring into, I asked him to give me his opinion as to what was in the field. He at once exclaimed, "Oh, you have pus here!" When I pressed him for his reasons, he said some of the nuclei were characteristically pus-like; but he did not feel much confidence in the stated distinctions between pus and white blood-corpuscles. He added, however, that he had never before seen blood with corpuscles like those under the microscope.

Left Lung. There was no sign whatever of metastatic abscess in any stage. It was everywhere crepitant. On section, some blood-aerated liquid exuded. The infiltration of this fluid affected all the lower part of the lung, from the base to the apex. There were no tubercles, nor emphysema. Portions of this lung floated, even after extreme

pressure. The congested portion readily broke down on pressure.

Right Lung. The preceding description applies strictly to it. On the diaphragmatic surfaces of both lungs, especially of the right one, were numerous little dusky red spots; others, of slate colour, being deeper seated. These spots simulated those met with in scarlet fever, and other blood-diseases. The lung-tissue immediately beneath these spots did not materially differ from the lung in any other part.

The heart, liver, kidneys, and prostate gland, were healthy.

The *spleen* measured six by three and a half inches. It readily broke down on pressure, but otherwise appeared healthy.

Brain. On removing the dura mater, the brain appeared generally pinkish; very notably more so than in health. On examination, the pink colour was found to be due to a large number of minute capillaries, which were quite as numerous on the anterior as on the posterior part of the brain. The superficial veins were not preternaturally congested. On section, the brain-substance appeared remarkably healthy. The *puncta vasculosa* were, however, more marked than natural; drops of blood, of considerable size, exuded from many of them. There was no excess of serum in the lateral ventricles; nor congestion at the base of the brain, which, in this particular, differed remarkably from the upper surface. The cerebellum presented nothing remarkable.

Abscess of Neck. On removing the integuments and platysma from over the seat of the first abscess, on the left side of the upper part of the neck, its cavity was found irregular, extending backwards under the parotid, forward under the submaxillary gland, and downwards for a little distance, and beneath the sterno-mastoid. The neighbour-

ing muscles were healthy on section. The cavity contained about ℥ss. of apparently good pus, which, on microscopic examination, showed (with the aid of acetic acid) numerous pus-corpuscles with the characteristic nuclei.

Limbs. On removing the integument from the back of the right forearm, the subcutaneous cellular tissue was found infiltrated with liquid not unlike melted butter. On dissecting the part, the intermuscular cellular tissue, especially in the superficial and deep layers, was loaded with the same liquid, which, in parts, was opaque, and similar to pus, to the naked eye. Under the microscope, a small number only of pus-corpuscles, but granular corpuscles in very great numbers, were seen. A vein, coursing amidst the cellular tissue thus infiltrated, had transparent coats, and in every respect appeared healthy. On removing the skin from the left arm, an appearance exactly similar to the other was presented; but, on dissection, a notable difference between these two parts was discovered; for, whereas the muscles of the right forearm appeared healthy, the left biceps was infiltrated throughout its whole substance with purulent looking fluid, which was not collected in masses, but disposed in longitudinal strata, evidently in the interfascicular cellular tissue. This condition affected the whole arm, but did not reach into the axilla. On cutting into the left ankle-joint, it was found perfectly healthy.

As the dissection of the body had already proceeded to a considerable extent, the other joints could not be consistently opened; but, as already observed, there was no external sign of disease in any of them.

My reasons for regarding this as a case of pyæmia, after my first examination of the patient, were, that though some of the symptoms might be mistaken for an attack of typhoid fever or rheumatism, yet all their signs were not present, while others (those connected with the abscess)

pointed to a different diagnosis. Admitting that all the signs of pyæmia were not present, and especially that it could be traced to no cause, yet the symptoms simulated it more closely than any other affection. The formation of the abscess contemporaneously with the commencement and progress of the constitutional affection, the subsequent manifestation of articular and muscular pains, and the aggravation of the general symptoms, while the signs attendant upon the formation of the abscess decreased, decided me in the diagnosis, in the correctness of which, cadaveric inspection confirmed me. True it is, that no abscesses were found in the parenchymatous organs; but they are not constant even in cases of traumatic pyæmia. I have, however, seen an instance of the idiopathic affection, in origin, symptoms, and progress, precisely like Harley's; in which, however, the lungs were the seat of metastatic abscesses. If it be admitted that Thomas Harley was the subject of pyæmia, there can be no doubt that the affection was independent of injury.

Certain it is, however, that in by far the majority of cases, pyæmia occurs as the result of injury. It is met with after slight, as well as after severe, wounds; after injuries to the soft parts, as well as to the bones. It is said to be especially liable to occur as the result of wounds of veins. In reflecting on this point of my subject, an important question has presented itself to me for solution. Continental writers dwell incomparably at greater length in their descriptions of this disease, than do our own. In visiting English and foreign schools, I have been struck with the fact that, whereas little mention was made of pyæmia in the former, it was regarded as a frequent complication of surgical practice in the latter. These facts suggest the question, Is the disease more frequent on the continent than in England? The momentous import of this question can scarcely be exaggerated.

The mode of dressing wounds, and the low dietary scale, prevailing in continental hospitals, tending as they do to deteriorate the quality of the secretion of pus, to favour its accumulation, and to lower the powers of the system, might be regarded as fruitful causes of the purulent infection; and so they have been by Professor Sédillot.* Of nine cases of this disease which I carefully studied in the Florence Hospital, one occurred as a sequel of venesection; two after compound fracture of the leg; one after punctured fracture of the skull; two after amputation of the thigh; one after necrosis of the jaw; one after extirpation of a tumour in the breast; and a ninth as a sequel of opening a small abscess in the groin.

As a whole, these are not cases in which continental differs most widely from English practice; and hence my own observation would not be favourable to the doctrine, that such difference leads to a greater frequency of the purulent infection in the hospitals of continental Europe, than in our own. To the fact that much less space is devoted to the consideration of this disease in the works of our own, than in those of foreign surgeons, I am disposed to attribute little value, as an evidence of the comparative rarity of this disease amongst us. On the contrary, the very great scantiness and ambiguous tone of the writings of English surgeons on pyæmia, taken in connexion with the errors of diagnosis which I have witnessed, and with the confession made to me by a candid and eminent teacher in this metropolis, that, to his certain knowledge, no definite value was attached to the symptoms of the disease a few years since,—forces me to suspect that the greater frequency of this affection on the continent is less real than apparent.

The settlement of this question by extensive observation is much to be desired; for, if the reply be in the affirma-

* De l'Infection Purulente ou Pyémie. Paris, 1849, pp. 431-434.

tive, the question will arise, What is the cause of the greater frequency? The solution of this problem may reasonably be expected to give a clue to the real cause of the disease, and consequently to the means of preventing it. On the contrary, if it shall appear, that in our own, as well as in continental practice, pyæmia rages with equal virulence, the theoretical notions now entertained as to the influence of peculiar dressings, etc., will be swept away, and the propriety will be apparent of diverting the channel of research for the discovery of the etiology of pyæmia.

It may already have been gleaned from remarks incidentally made, that, when once pyæmia has been diagnosed, the prognosis must be unfavourable. Nélaton† states that the disease is invariably fatal; but I am inclined to believe that such is not strictly the case. The grounds of my belief are histories of cases related by Sédillot‡ and Vidal§, and of others contained in my own note-books. Nevertheless, the great fatality is an undeniable fact.

Briefly as I have been obliged to treat my subject, I would gladly make amends for the defect by dwelling at greater length on the all-important question of *treatment.* If, by so doing, I had it in my power to contribute facts worthy of knowledge, I might be disposed to transgress the duty I have been obliged to impose on myself, of being brief; but, since entering into a discussion of the numerous modes of treatment proposed would demand time out of proportion to the instruction it would afford, I deem it advisable to proceed to analyse the opinions held by authors as to the nature of the disease.

These opinions may be arranged under two heads: firstly, those which hold pyæmia to be an aggravated form of

† Op. cit., p. 167.

‡ Op. cit., Observations Cliniques, 20, 21, 22, 24, 26, 27, 28.

§ Traité de Pathologie Ext., etc., 3me. Edit. Tome deuxième, p. 27.

inflammation of the veins; secondly, those which hold it to be a distinct disease.

The discovery of the symptoms, anatomical characters, and consequences of phlebitis, was one of the many fruits of the labours of John Hunter. He saw cases of death with the formation of metastatic abscesses after suppurative phlebitis, of which he regarded them as an aggravated form. In 1828, a paper, under the title of a "Pathological Inquiry into the Secondary Effects of Inflammation of the Veins", was read before the Medico-Chirurgical Society, by Mr. James Arnott, who has ever since been regarded by English surgeons as the prover of the doctrine, that the occurrence of purulent deposits and death, after surgical operations and injuries, is due to inflammation of the veins. This doctrine is taught, with but slight modifications, in the writings of Samuel Cooper, Liston, Syme, Hyde Salter, Bransby Cooper, and Vidal.

It is but just to say that, of this number, Vidal is the one who has given by far the most complete account of the disease. I have said that these writers have reproduced the teaching of Mr. Arnott, with but slight modifications: to one of the most notable of these I wish to call attention.

In his "Elements of Surgery", Mr. Liston remarks, that "when, from any cause, the extremity of a large vein in a wound is not closed, when it is not plugged up by plastic matter, pus seems to enter it readily, and, by mixing with the circulating fluid, causes dreadful mischief. Great constitutional disturbance accompanies the purulent deposits which follow in the solid viscera, and in the joints. Inflammation of a vein is also occasionally followed by the sudden appearance of a purulent deposit in some part of the body, external or internal, at a distance from the inflamed part."

Thus, though Mr. Liston shewed his leaning to the doc-

trine of Hunter and of Arnott, by alluding to the disease in his chapter on Inflammation of the Veins, he partly dissented from it, by acknowledging the possibility of a disease, similar to the alarming constitutional complication of suppurative phlebitis, arising from the entrance of pus into the circulation through an opening in a vein.

While there can be no doubt that suppurative phlebitis is about the most frequent cause of pyæmia, it is equally true that it is not the only cause; and that to regard pyæmia as suppurative phlebitis, is to mistake the effect for the cause. Even without taking into account those undoubted cases of spontaneous pyæmia, which are unassociated with disease of veins, certain it is that, in not a few cases of the disease following injuries, no trace of venous inflammation has been discovered. I have failed in discovering it in cases in which I have most accurately dissected the venous system; and even Bérard admits the existence of such cases; though he, a strong believer in the phlebitic doctrine, keenly perceiving what formidable obstacles to the safety of his theory such cases would be, endeavours to explain them away, by saying that the inflammation may have affected some venous radicles which have escaped detection.

But such a mode of argumentation, vicious as it is as a *petitio quæstionis*, cannot be assented to. If there are cases, as there undoubtedly are, even according to the admission of Bérard himself, in which the most careful dissection, purposely conducted, fails to discover the slightest trace of inflammation of the veins, the only legitimate deduction is, that pyæmia may occur independently of, and, *ergo*, is not identical with, suppurative phlebitis.

We now come to the consideration of the doctrines according to which pyæmia is held to be a distinct disease. As the very antithesis of the doctrine propounded by

Hunter and Arnott, Bérard and Vidal, I must first call attention to the opinion promulgated by Tessier in 1838, to the effect that not only is pyæmia a distinct affection, depending upon a blood-disease, but that it is always independent of venous suppurative inflammation. He grounded his opinion on a dogma which he thus expounded: "At all stages of venous inflammation, the pus is enclosed in the cavity of the vein by clots or false membranes, and at no period of the anatomico-pathological existence of phlebitis is its entrance into the blood possible."

Tessier, in thus expressing himself, erred, though in a different direction, yet for a similar reason, to that which led his opponents into error. On both sides, partial observation, and too sweeping generalization, were the screens which hid the truth. Certain it is, as has been long known, that cases of suppurative phlebitis do occur, in which neither clot, nor any other obstruction, exists between the pus accumulated in the inflamed veins and the heart; and in which, consequently, there is nothing to impede the admixture of pus with the circulating vital fluid.

Opinions differing from all those I have hitherto recorded have been published by Nélaton, Fergusson, Miller, Chelius, Lee, and Sédillot; and it is on these that I must now make a few remarks.

Nélaton recognises phlebitis as a powerful cause of this disease, though he avows himself ignorant of its mode of operation, and expresses a belief that pyæmia probably occurs spontaneously in some cases. In thus holding an opinion intermediate between that of Hunter and his followers on the one hand, and of Tessier's school on the other, Nélaton has, I believe, approached nearer the truth than any other writer.

Fergusson, Miller, and Chelius, have briefly alluded to the disease in question, as one due to purulent absorption;

but so brief and indefinite are their statements, that their writings must rather be held as proofs of the comparatively little importance which they attach to the subject, than as evidence of their adhesion to any particular doctrine.

Mr. Lee's opinion may, to some extent, be gleaned from the following passage at page 45 of his very elaborate monograph *On the Origin of Inflammation of the Veins, and on the Causes, Consequences, and Treatment of Purulent Deposits*:—"The introduction of pus into the system through an injured or inflamed vein, can rarely be the first step towards purulent infection of the system. Some change must previously have passed in the blood, by which its coagulating power is impaired, or some unusual mechanical means must have been employed, before the pus can find its way in the course of the circulation."

The main ground of Mr. Lee's opinion is experiment; and we must test its validity, as well as that of his mode of arguing thereon, in order to ascertain the purport of his teaching.

In the preface to his thesis, the following passage occurs:—"The simple experiment of mixing some pus with healthy recently drawn blood, will at once show that such a combination cannot circulate in the living body. It will be found that the blood coagulates round the globules of pus, and forms a solid mass, which will adhere to the first surface with which it comes in contact; and it will be evident that it is not till the coagulum formed is broken up or dissolved, that its elements can circulate with the blood."

To argue, as Mr. Lee does, from the fact that, out of the body, blood coagulates round pus, therefore such a combination cannot circulate in the living body, is about as warrantable as it would be predicate, from the observation that pure blood coagulates in a basin, it therefore cannot remain

fluid in the ventricles of the heart. The fact is, that the circumstances are so materially different—blood in an earthen vessel on the one hand, blood in the living body on the other—that no inference deduced in the one case is applicable to the other.*

Mr. Lee's experiments on living animals, according to his own statement, tend to show that pus, by its tendency to coagulate the blood when introduced into the vessels, is arrested in its progress in some parts of the circulating system, and thus to afford valid evidence in support of his opinion. But the soundness of Mr. Lee's observations may fairly be questioned. Moreover, Sédillot never found any difficulty in injecting pus into the circulation, and he thereby repeatedly produced a disease identical in symptoms and anatomical characters with the pyæmia met with in surgical wards; and I have been informed by a friend—a very trustworthy experimenter—that in trials which he conducted, Mr. Lee's opinion as to the pus being incapable of circulating with the blood in the living body on account of producing its coagulation, proved to be erroneous.

In cases, says Mr. Lee, where, from long-continued disease, and the repeated introduction of vitiated fluids into the circulation, the blood has lost its power, there appears no reason to doubt the correctness of Dr. Sédillot's observations, that it is probable that pus-globules may then circulate with those of the blood. Now, Sédillot's observations, to which Mr. Lee here makes allusion, are observations of fact; and the only mode in which they can be refuted, is by other experimental observations which shall, if not point out the truth, at least lay bare the fallacy, if any, of the

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* While these pages have been in the press, I have perused Professor Hughes Bennett's article on Leucocythemia, in the first volume of the *Monthly Journal of Medical Science* for 1852, p. 331. It gives me pleasure to find that my criticism on Mr. Lee is precisely in accordance with that of Professor Bennett.

French surgeon's observations. On a matter of such vital importance to the existence of his doctrine, it would have been well if Mr. Lee, instead of satisfying himself with saying that, under such and such circumstances, *there appears no reason to doubt* that pus-globules may circulate with those of the blood, had had recourse to experience, examined specimens of blood, and then analysed the conditions of the patients from which they were obtained. To say, apart from such experience, in support of a doctrine, that the circulation of pus in the blood is prevented by the power of the ~~former to induce coagulation~~; to say, I repeat, apart from such experience, in support of such a doctrine, that there appears no reason to doubt the possibility of pus circulating with the blood when it has lost this power, is begging the whole question.

If this criticism on the fundamental propositions of Mr. Lee's essay be just (and I believe it is), it must be admitted that the very foundations of his doctrine are assumed, and consequently, that it cannot be accepted as the theoretical explanation of the phenomena of the disease. The importance of refuting it will be at once evident, when it is borne in mind how baneful an influence incorrect theories always exert on the progressive study of the natural history of a disease, and, *a fortiori*, in the discovery of the best mode of treating it; a discovery which is almost hopelessly distant in pyæmia, and which will certainly only be arrived at by a just appreciation of the causes, signs, and anatomical characters of the disease, and by therapeutic experiments, conducted independently of any fallacious theoretical bias.

The only author to whose opinion it is left for me to allude, is Sédillot: whose work on the subject, published in 1849, must ever be regarded as one of the most valuable contributions to surgical pathology. He believes that the introduction of pus into the blood is the true cause of the

phenomenon described under the name purulent infection, and the grounds of his belief are threefold.

First. A suppuration developed on some part of the economy always precedes the appearance of pyæmia.

Second. A manifest relation exists between suppuration of the veins and pyæmia. In this paragraph he merely states what cannot be denied, that pyæmia does sometimes follow suppurative phlebitis.

Third. The introduction of pus into the blood, and its presence there, admit of positive demonstration.

To the first proof I cannot give assent. The case of Harley, which I have related, appears to give satisfactory evidence that pyæmia may occur without wound, but a still more exemplary case to prove the occurrence of idiopathic pyæmia has fallen under my notice; more exemplary, I say, because in addition to the disease occurring without that apparent cause, cadaveric inspection proved the existence of metastatic abscesses in the lungs. Such cases as these likewise invalidate Sédillot's statement, that the introduction of the pus into the blood admits of positive demonstration. But this statement is even opposed to experience. Cases of traumatic pyæmia do occur, in which all the veins capable of dissection are found to be closed at the surface of the suppurating wound, which has given rise to the disease. Even Sédillot admits the existence of such cases; and as they threaten the subversion of his doctrine, he endeavours to explain them away, by remarking that very probably venous erosions have occurred, too small to be seen, but large enough to admit the introduction of pus: but these erosions, he adds, could not occur without phlebitis, and then the pyæmia would have for double source the pus of the pyogenic surfaces, and that of the eroded venules. But what shall we say of this mode of argumentation on the part of one, who has made use of the fact that phlebitis is not

always discovered after death in pyæmic subjects, to prove that suppurative phlebitis is not the sole cause of pyæmia ; and who has condemned, as inadmissible, Bérard's explanation of the cases in which no phlebitis is found, that the inflammation may have been seated in the venous radicles, which, from their smallness, escaped detection ?

As I have argued that, if by careful dissection no inflammation can be discovered, the only warrantable conclusion is, that none exists ; so I must argue that if all the veins capable of dissection are found closed, it is not warrantable to assume the existence of openings apart from empirical observation. The presumptive evidence is in favour of the belief that cases of pyæmia occur without any solution of continuity, or other diseased condition of the veins.

The fact is, that if Bérard's mode of arguing be objectionable, Sédillot's is doubly so ; for, whereas the former only assumes the possibility of phlebitis existing without being capable of detection, the latter assumes the existence of phlebitis, and openings in veins, without the means of proving either.

There is yet one opinion of Sédillot's to which I have to call attention. He states that the presence of pus in blood admits of positive demonstration by the aid of the microscope. The distinction between the corpuscles of pus, and the white ones of the blood, has always been held to be matter of serious difficulty : and a majority of respectable authorities have held it to be impossible to discover the existence of pus in the blood of animals into whose vessels it has been injected just prior to death. Sédillot, on the contrary, speaks of the discovery of pus in the blood of pyæmic subjects as an easy microscopic observation, without in reality giving any character as absolutely diagnostic between the corpuscles of pus, and the white ones of the blood. The fact that he treats so

lightly, on a matter acknowledgedly so difficult, is strong presumptive evidence, warranting a right of questioning the validity of his statement.

In two cases which recently came under my notice, the blood carefully examined microscopically by myself, exhibited a large excess of white blood-corpuscles. But it is no use going further into the question, considering the paucity of evidence at hand.

The question of the condition of the blood in pyæmic subjects, must, in the present state of science at least, be regarded as a question *sub judice*, greatly requiring careful research for its solution, and promising a rich harvest to whomsoever shall carry it out.

In concluding this critical analysis of opinions,* I cannot forbear directing attention to the great desideratum for further investigation to which it points. The fact must be evident that the knowledge of this disease is comparatively recent, and that no sooner has attention been directed to it, than writers have been eager in theorizing on its nature without first studying the whole history of its phenomena. Let us inquire accurately into the causes, symptoms, and anatomical characters of pyæmia, and then, but then alone, will be the time to inquire into the *wherefore*. Obvious it is that the prosecution of any other plan than this must expose its followers to the danger, inseparable from being satisfied with a perception of partial truths, and forgetting that a discovery of the whole truth may prove any opinions deduced from prejudice and limited experience to be absolutely fallacious.

A task so incompletely achieved as this of mine is, stands in need of more excuse than I can plead for it. I feel that

* Since writing the above, I have become acquainted with the important observations of M. Velpeau (*Léçons orales de Clinique Chirurgicale*. Bruxelles, 1841). If this essay had any pretensions to completeness, I should feel it incumbent on me to append a digest of the professor's teaching; under existing circumstances, this may for the moment be deferred.

it is of no use to point in detail to the deficiencies of this paper, because so doing would be equivalent to writing a thesis on pyæmia—to doing, in fact, that which lack of time has prevented me accomplishing, and for not doing which I am constrained to plead indulgence. If, however, I have succeeded in drawing attention to the high import of the subject, and aroused to energetic action in its study, I have accomplished my greatest aim. I have spoken of a terrible and fatal disease, frequent yet almost unknown; and if I have not succeeded in imparting much information, I may at any rate say, that on this part I have not fallen below my aim. I am sufficiently repaid by the reflection that the few truths I have propounded, and the errors I have denounced, may incite some among you to endeavour to add to the former and lessen the number of the latter.

University College Hospital, November 1852.