

having at first administered the wine on account of the accompanying diarrhoea. When the disease is advanced, little, or only temporary, benefit results; but, when given at an earlier period, and associated with cod-liver oil or other means, the wine does exert a salutary influence that merits attention. In gastralgia, while the general debility seems to indicate the use of tonics, yet wine cannot be given, even in small quantities, without exciting a sensation of burning; but in several examples of this the vinous lavements have not only restored strength, but have so diminished the gastric irritability as to allow of a little wine being taken with the food. Another form of dyspepsia alike susceptible of benefit is vomiting, with the rejection of alimentary matters—the strength and *embonpoint* rapidly returning under the use of the enemata.

But the affection in which M. Aran has been most surprised at the amount of success obtained is chlorosis. Influenced by the so prevalent opinion that iron is the specific remedy, he at first gave it in conjunction; but he afterwards omitted it, only prescribing, besides the wine, general frictions with an ammoniacal liniment, and a powder consisting of bismuth, rhubarb, and valerian. In a few days, the patients seemed to have recovered their normal health just as if iron had been given, and that even in severe cases. In their relation to iron, cases of chlorosis may, indeed, be divided into three categories. In the first and most numerous, iron properly administered furnishes a durable cure. In others, a rapid amelioration ensues, which is followed by a relapse on its discontinuance. Temporary amendment attends its resumption, and so on for years, iron thus becoming a constant necessity. Thirdly, there are not a few cases in which iron fails altogether, or in which the amelioration never rises to the dignity of a cure. M. Aran has also advantageously employed the injections in various other conditions characterized by debility, and especially in paludal, syphilitic, or cancerous cachexy, in some cases of anasarca, and in different circumstances in which alcoholic stimuli are indicated.

The lavements are found to act advantageously, in proportion as the individuals have been previously unaccustomed to alcoholic drinks. Thus, they are, in general, more useful in women than in men, more still in young girls, and more so in the inhabitants of the country than those of Paris. Until the patient becomes accustomed to them, they induce a kind of drunkenness, but very different from that following the swallowing of wine; and if the enema be given in the evening, so that the patient may afterwards remain quietly recumbent, refreshing sleep ensues. It is to be remarked, that effects are produced by a dose of wine that, if swallowed, would induce little effect. It is sufficient, in slight cases, occurring in impressionable persons, to employ 150 grammes; but, in obstinate cases, 250 to 350 grammes may be required. In the chlorotic, it seems much to hasten the cure to divide the dose into two parts, administering one morning and evening. In general, from 150 to 250 grammes suffice, and it is better, if the wine is rich, to commence its use by diluting it with water. Immediately before giving the enema, the rectum should be emptied by a lukewarm injection, and the wine should be employed tepid, so as not to induce contraction of the rectum. At first, the patient should lie down, and endeavour to retain the fluid; but those who are accustomed to the treatment retain it with little trouble in any position.—*Med. Times and Gaz.*, March 17, from *Bull. de Thérap.*, 1855, t. xlviii.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

38. *Discussion on the Curability of Cancer, and its Diagnosis by means of the Microscope.*—[We continue, from p. 520 of our preceding number, the interesting discussion, at the Imperial Academy of Medicine, relative to this subject.]

Nov. 7, 1854. M. VELPEAU said: At the commencement, I advanced two propositions: 1st. That cancer was sometimes curable; and, 2d, that certain cases

of cancer could be recognized without the aid of the microscope. To these questions I mean now to confine myself, seeing that the discussion has embraced too many topics for me to overtake; and I intend also to state by what observations and researches I was led to form the opinions I then expressed.

I have said that the "cell" is not the specific element of cancer, as these gentlemen thought; and that there are epithelial cancers which contain no cells. M. Larrey must have spoken unthinkingly when he appealed to the unanimity of microscopists in this matter, for certain of them admit what others deny. Contrary to the opinion of Lebert and Broca, it is admitted by Mandl, Virchow, Bennett, Paget, and others, that there are epithelial cancers without cells. Microscopists are not all agreed, because cells do not exist in all cancers. There is some other thing in cancer of greater importance than the cell; and in the blood of patients having the cancerous cachexia there certainly exists something peculiar to the condition which we do not as yet know. The cell is one of the manifestations and material characteristics of cancer; but it is not the principle of the disease, or the malady itself. This cancer-cell has been found elsewhere than in cancers, and it existed in a non-cancerous portion of the vesical mucous membrane, lately exhibited to the Société de Biologie. It appears to me not to be a sufficiently constant characteristic to enable us to distinguish the different varieties of tumours, and, therefore, I have tried to diagnose them by other signs. I have examined a very large number of cancers, and admit the occasional, but by no means the constant existence of the so-called cancer-cells. It may be said that the cases where the cells were absent were not true cancers; be it so, I am willing to set them aside as doubtful. But I have seen tumours containing cells which were *not* cancerous. It has been asked whether I consider them to have been benign on account of their non-recurrence, and it has been objected that I cannot base my opinion on this character, if I admit that cancer is sometimes curable, and consequently does not constantly recur; but it is not from the non-recurrence of the tumours alone that I judge, but from the *tout ensemble* of their physical characters and clinical signs.

I affirm, then, that cancer-cells have been demonstrated in non-cancerous growths, and that they are often absent in true cancers.

M. Malgaigne has suggested that in these cases of cancer without cells I have mistaken fibro-plastic for scirrhus tumours. This I have taken special care to avoid doing, and when I speak of cases of scirrhus, they were undoubtedly true cases of it. It has been said that the cells were not found, because the whole tumour was not examined. I have never attempted to puzzle the microscopists, and I have always given them the entire tumours for examination. Cancerous tumours, like other pathological specimens, are not equally diseased throughout their whole extent; there is generally a central portion in them in which the disease is most marked; but if we have a section made through this part, it is just as good as if we had the entire tumour. If cancer-cells are not found in such a section, they are certain not to exist elsewhere. I have cited cases of tumours, which contained no cancer-cells at first, recurring afterwards, and exhibiting these cells in their structure. I have seen two or three such cases. In one of these, a man had a cancerous tumour, a portion of which was removed and sent to M. Lebert for examination. Finding no cells in it, he pronounced it to be benign. M. Richet, therefore, operated on the patient; things went well at first, and the cicatrix looked healthy; but ere long the tumour returned and killed the patient. The second tumour contained cancer-cells. Again, a woman had a mass of vegetations on the *left* breast, which resembled neither encephaloma, scirrhus, nor fungus. I removed one of these vegetations, and found that it contained no cancer-cells; but I was not therefore convinced that it was not a cancer. The woman returned after some time, having the *right* breast affected with all the most marked characters of cancer—partial indurations, and retraction of the nipple. She soon died of the affection. M. Robin found cancer-cells in the tumour of the right breast, and also numerous nodules, in the lungs and liver, containing cancer-cells. Can we say that in this case the tumour of the right side was a cancer, while that of the left was non-malignant? Certainly not. Moreover, the cells were found

on the side on which the tumour was least degenerated. M. Leblanc has attempted to explain these differences, by admitting the existence of "mixed tumours;" and he believes that cancer may become developed in a tumour not primarily malignant. Microscopists, however, deny the transformations of tumours. I myself am doubtful as to whether benign tumours may degenerate, and I would not be understood either to affirm or to deny it; for facts have led me to believe that such a change may be quite possible.

It appears to me, then, 1st, *that there are cells without cancer*; and, 2d, *that there are cancers without cells*; and if these facts be once admitted, they go very far to negative the assertions of the microscopists. A new definition of "cancer" is asked; but the remodelling of definitions is a barren and unprofitable work. It is far easier to attack an old definition than to make a new one, even when it relates to something we understand very well. For example, I should be puzzled to define M. Robert with exactitude, yet I can quite well recognize him. I would characterize cancer as a malady, which may appear as an excrescence, a tumour, an ulcer, or a fungosity; the specific nature of which is, when once established, to spread, to invade diseased tissues, and, finally, to cause death.

Let us look now at the tumours which the microscopists refuse to admit as cancers. They deny that epithelial tumours are cancerous, because their tissues are homomorphous, and such a structure, they say, cannot be malignant. They allege that these tumours do not recur, or if they do, that they affect only the original spot, but that they do not affect the lymphatics, or become generalized. Every-day experience, however, shows that these tumours may recur, not only on the original site, but also in the glands; and that their recurrence, far from being exceptional, is very frequent. Hence, M. Lebert and other microscopists have considerably modified their first opinion. Yet they affirm that epithelial tumours never, like cancer, become generalized. Now, M. Robert himself has adduced instances of their generalization; and others are recorded by Paget, Virchow, and Rokitansky, in which they become developed in the heart, lungs, and liver. Yet this, say they, happens very rarely. I do not know; but whether it occurs often or seldom does not at all alter the question. We remove a small tumour of this sort from the lip; at the end of two, three, six, or twelve months, we find that it returns; again we excise it, and again it recurs. Finally, we dare not touch it; and it kills the patient as surely as cancer. Wherein lies the difference between them?

Again, as to fibro-plastic tumours. To constitute them a distinct group from cancers, the microscopists have been obliged to class with them many tumours to which they have no analogy. How can fibro-plastic tumours, which prove fatal to the patients, be confounded with simple glandular hypertrophies? and in what respects do they resemble each other? Although the anatomical structure of both may be similar, there doubtless exists in them some unknown element which constitutes their essential difference. M. Lebert has said that these tumours, like the epithelial, do not recur, or if they do, that it is only locally; but I have shown that gentleman many instances of their reproduction in the viscera. M. Robert has cited cases to prove the non-recurrence of fibro-plastic tumours; but—so great is the blindness which an adherence to a preconceived idea occasions—all these cases occurred before the use of the microscope. For thirty years I have constantly applied the great law of Bichat, cited by M. Robert, to everything in pathological anatomy. But, in pathology, anatomy, though very necessary, is not all-important; for we must also take into consideration the oftentimes obscure nature and causes of disease. There exists in cancer something special, something not anatomical, which has hitherto escaped all the investigation of our senses, and of which the microscopists have taken no note.

Here, then, is my basis for the distinction of tumours. *Cancerous tumours all possess one character in common: they have all a tendency to destroy the tissues by becoming generalized.* All that can be said against this proposition lacks demonstration, and proves nothing.

I turn now to the question of its *curability*; and the first difficulty that presents itself is *diagnosis*. And, first of all, I would remark that it has been

said that I lay pretensions to infallibility of diagnosis, and that I boast I am never deceived. God knows that I lay claim to no such lofty pretensions. I have merely said that there are certain tumours, whose clinical characters are so well marked that it is impossible for *any one* to be deceived. I have taken notes, for the past year, of 120 cases of tumours; 66 of which were cancers. Among these 66 cases, I have removed 49 cancerous mammae; in every case, the extirpated tumours were examined by the microscope, and in every instance the microscope confirmed the diagnosis I had made before the operation. Let me add, however, that I have never alleged that a correct diagnosis can *always* be made. When I say that in certain cases I cannot be deceived, I allude to those in which the characters are so well marked that any surgeon could recognize them.

I come now to the important point. Why may not cancer be curable? When I advanced facts supporting the curability of cancer, it was said that they were inconclusive, because they were anterior to the use of the microscope. But when, in the 49 cases I have mentioned, which were microscopically examined, my diagnosis was in no instance wrong, how can it be said that I was deceived before the introduction of the instrument? As regards my statistics, it has been said that 20 cures in 50 cases is too good to be true; but I have not alleged that I cured 20 out of 50 patients, but out of 200, one-half of whom died. (Here M. Velpeau cited several cases of persons who were cured of cancer; that is to say, who never had any recurrence of the disease for a greater or less number of years after the extirpation of a cancer—cases which were almost all witnessed and examined by the microscopists.) These facts, said he, are authentic; and the curability of cancer cannot be denied. M. Robert has alluded to the case of a lady operated on in 1850; but he has not observed that, in detailing this case, I made reservations in respect to it.

As to relapses recurring long after the operation, as after ten, fifteen, or twenty years, may we not inquire whether or not it is possible that cancer, like other diseases, may become twice manifested in the same individual, without this being looked upon as a recurrence? I am always asked why we are unwilling that cancer should be curable? and what interest microscopists have in maintaining that it is so? I have never been able to understand it. Statistics, and the authority of highly respectable names—as Munro, Boyer, etc.—have been cited in support of the incurability of cancer. But the statistics of Munro and Boyer are of great value; those of Hill and Flajani do not militate against mine; and as to the recent ones of MM. Lebert and Broca, they do not appear to me a whit more conclusive, as many of the patients enumerated in them were entirely lost sight of.

What we seek here is truth; but we should not make the obtaining of it depend upon a mere theoretical opinion, since theory alone has given rise to the unbelief in the curability of cancer, which I so much deplore. As to the microscope, I maintain that it has done good service to science; I believe that it will render still more; but I am confident that it has also committed several errors. I accept all the new facts which it has revealed to us; and I only contest the false or doubtful explanations which have been given to them. And as regards the question at present under debate, I think that its resolution should be made with great caution and reserve, inasmuch as there probably exists some special element with which we are not yet acquainted. I have ever encouraged, and I will continue to encourage fresh researches; but I will only accept as true what I find to be satisfactorily demonstrated. I hail the march of progress with unmixed delight, so long as the car of science is propelled, but is not overturned.

Nov. 14, 1854.—The secretary read the following letter, addressed by M. Mandl to the president:—

“Mr. President: M. Velpeau, in his last address, has done me the honour to class me among the microscopists who do not admit the existence of characteristic cancer-cells. I beg to say a few words to the Academy on my reasons for so thinking:—

“Shortly after Müller, in 1839, discovered cells in cancerous tumours, I published my observations in the *Archives de Médecine*. But in 1843, I owned

that the sanguine hopes which I had at first formed of the advantages to arise from the employment of the microscope in the examination of such pathological specimens, had not been realized; and my researches since then have confirmed this opinion. I had soon abundant opportunities of examining tumours which had appeared to clinical observers to be cancerous, from their origin, progress, and fatal termination, but which contained none of the cells considered by Lebert to be characteristic of the malady. I can remember cases of epithelial and fibro-plastic tumours; but I would more particularly direct attention to cancers of the retina. In four out of five tumours of this last kind, extirpated by Sichel and Bérard, it was impossible to discover any specific cell; and yet subsequent general infection left no doubt as to their malignant character.

"On the other hand, in the cells found in cancerous tumours, I have not observed the characters which they are said to present in the majority of cases. According to M. Lebert, the nucleus of the cancer-cell has always a mean diameter of, from the $\frac{1}{1000}$ th to the $\frac{1}{500}$ th of a millimetre. (*On Cancer*, p. 30.) Yet, in encephaloid tumours, I have found the nuclei not above the $\frac{1}{1000}$ th or the $\frac{1}{500}$ th of a millimetre; and in such cases it is impossible to distinguish them from the other nuclei of normal tissues. Again, as to nucleoli, I have often found their characters very imperfectly marked. In M. Lebert's work, we find examples of these inferior dimensions in cancers of the bones, the stomach, etc. As good, and even better observers than M. Lebert, believe that it is necessary to take into consideration all the elements, and also to know the history of the disease, before pronouncing a judgment upon its nature. Does not this show that the cell is not sufficiently characteristic of cancer?

"The truth of this is proved, not only by observation, but also by the laws governing histogenesis. It seems to me that, in order to solve the problem which we are at present studying, it is indispensably necessary that we engage in the comparative study of normal histology, and especially in the study of the development of the tissues. Space precludes me from entering into any detailed explanation of development, so far as it regards this subject; but I shall briefly state the principal results of my researches.

"All the tissues of the organism form two grand series; the one composed of cells and scales (*ou lamelles*), the other of fibres; and this essential distinction characterizes the different elements from their very first appearance. Nuclei placed in a soft amorphous substance, become surrounded, in the first instance, by membranes; they increase in size, multiply, and constitute the cells, of which the glandular, epithelial, and other tissues are composed. In the second class, on the other hand, the nuclei are placed in an amorphous, homogeneous, solid substance, which splits, and goes on subdividing into more and more delicate fibres, so as eventually to constitute a fibrillated tissue. There is no cell-formation in all this series of organic elements, among which may be classed the fibrous, cellular, and serous tissues, etc.

"This principle of development and of texture, inherent in each series of the tissues, is rigorously maintained in all their various physiological and pathological phases. In the normal renovation which accompanies secretion, growth, etc., as well as in anomalous renovation or in regeneration, we always observe that cells are developed in those places where cells originally existed, and that fibres are developed in the other tissues. Although hypertrophy may occur, the same principle continues to preside over the production of the new elements; fibres do not produce cells, nor cells fibres, but cells are always developed in the midst of cells, and fibres in the midst of fibres.

"Facts have proved to me that these principles hold good in the case of cancer. The cancerous diathesis may affect the blastema, which ought to produce either cells in the glands, scales in the epithelial, or fibres in the fibrous tissues; and according to the nature of the tissue infected by the diathesis, the morbid blastema will produce either cells or fibres. The product will then be characterized according to the species of the affected blastema; and we will have either cancer (*cancer à cellules*), fibro-plastic tumours (*cancer à fibres*), or canceroid growths (*cancer à lamelles*). But if it be permitted to us in most, though not in all cases, to recognize the alteration produced by the diathesis in the cells—that is to say, if we are allowed to recognize the cancerous

cells, and to distinguish them from other analogous elements—why may we not also take into account *fibres* and *scales* as elements of cancerous tumours? So long as we are unaware in what respect fibres and scales are altered when they are produced under the influence of the cancerous diathesis, so long will the microscope continue to give imperfect information to the surgeon in the diagnosis of tumours.

“As to the frequency and gravity of recurrences, all histologists know that there is no element which is increased, developed, multiplied, and regenerated with so much facility and rapidity as the cell. Fibres are much more tardy in the stages of their development. Now, in which of the varieties of cancer are the recurrences the most frequent and the most serious? Which species extends most rapidly, invades neighbouring organs most speedily, and destroys most energetically the whole economy? Undoubtedly, it is the *cancer with cells* (*cancer à cellules*.) The conditions are completely changed when the cancerous diathesis affects a blastema which ought to produce fibres; and when we see it form incompletely developed fibres, we call it the *fibro-plastic element*. Fibres, in general, are developed very slowly. I have seen a regenerated tendon presenting incomplete fibres at the end of seven years; that is to say, there was a blastema in which fibrillar division had not been completed. Thus the microscope is capable of informing the practitioner of the severity of the affection, and it also enables him to pronounce a proper prognosis as to its course, and as to the greater or less probability of its recurrence, inasmuch as it acquaints him exactly with the nature of the elements which constitute the tumour.

“I believe, then, that cancer, capable of occasioning general infection, can exist without the cancerous cell—that heteromorphous element which has been considered to be a peculiar and specific product of the diathesis—being present. This belief of mine has been long ago expressed by M. Velpeau, as well as by Bennett, Paget, Virchow, Vogel, and others.

“The cancerous diathesis may affect cells, scales (*lamelles*), and fibres, and accordingly there are as many different varieties, yet microscopical anatomy agrees with clinical observation in classing them together as one family.”

M. AMUSSAT felt confident that he had cured many very serious cases of cancer, which otherwise must have terminated in a painful and horrible death. The influence of grief appeared to him to be, speaking generally, the most frequent cause of cancer in persons of otherwise good constitution; hence he was led to believe that the seat of cancer lay in the nervous system. He trusted that this discussion on the microscopical characters of cancer would elicit fresh researches, which might elucidate the nature of the disease, and lay the foundation for a rational, efficacious treatment of it. He thought the question of the greatest moment in relation to cancer was, whether or not there existed any hereditary taint. If this was present, the most prompt and energetic measures were necessary. When it was resolved to operate, either by means of the knife or caustic, we should freely destroy the surrounding tissues, as the timidity of the operator often occasioned recurrences. When caustics are preferred to the knife, only the strongest should be used.

M. CLOQUET. Cancer was like inflammation—there were many very different varieties of it; yet all its various forms possessed certain common characters; they never healed spontaneously; they were very prone to recur, and, unless extirpated, they infallibly occasioned death. He considered that the microscopists had not formed a separate school of their own; they belonged to the anatomico-pathological school, and, being possessed of more powerful means of investigation, they had arrived at most important results. To insure our obtaining still more certain and important results, it was necessary that all medical men should unite in strengthening the hands of the microscopists, and in placing within their reach everything which they considered likely to advance the interests of science.

M. DELAFOND proposed to examine the question of cancer in relation to comparative pathology, to surgery, and to micrography.

It had been alleged by M. Leblanc that herbivorous animals were less affected

by cancer than carnivorous; but this proposition was too wide; and he believed that the herbivora were equally subject to its ravages. It was by no means rare to find cancer affecting the teats of mares and the testicles of horses, and it often occurred in the viscera of these animals. Asses and mules were very subject to it. Cancer was common among oxen, especially affecting the jaw. Pigs were very liable to cancer of the teats, and dogs were often affected by this disease. It also occurred among gallinaceous fowls, and many varieties of birds. Therefore, it did not appear that animals deriving their sustenance from the vegetable were less disposed to cancer than those whose nourishment was drawn from the animal kingdom.

He had found, from microscopical investigations, that fibro-plastic tissue recurred like cancer. He did not consider the cell characteristic of cancer. The cancer-cell varied in its appearance according to the stage of the malady. At first, the cells found in a small tumour, not compressed by the surrounding tissues, were of a rounded form, having a nuclei and nucleoli in their centres. Compression subsequently rendered them elongated and ovoid; subsequently, they became still more elongated; and, in scirrhus cancers, compression deformed them still further. In old cancers, they become infiltrated with calcareous matters; their nuclei become flattened, and the cells become thin, like fibro-plastic tissue. Secondary cells also become developed within the parent cells, both being inclosed within a common membrane. In encephaloma, where there existed abundance of liquids and fatty matters, the cells were rounded or ovular.

He had read that a German pathologist had been able to produce cancer by injecting the juice of a cancerous tumour into the veins of a healthy dog. He had several times tried this, but had never been able to obtain any such successful results, although he had injected cancerous juice *containing abundance of cells* into the veins, and had put it into the trachea, and had also inserted it beneath the integuments of animals. The characteristic element of cancer was not the cell, but the *juice* in which it floated. Inoculation with the pus of glanders produced glanders; that of the varioloid disease of sheep gave rise to the same disease; and the saliva of the hydrophobic dog occasioned canine rabies; yet the pus and the saliva in these diseases presented no special characters when microscopically examined. Thus there existed different properties with identical appearances.

As regarded the *dimensions of the cells*, he considered that the cells of fibro-plastic tissue were the smallest; those of encephaloma the largest; whilst those of scirrhus were of a medium size, between that of the other two. The appearance of the nuclei and nucleoli varied very much, according to the variety of cancer examined; they were very small in fibro-plastic growths. These facts accorded well with clinical results; for it was known that encephaloid tumours grew more rapidly, spread more certainly, and produced general infection more speedily than any other variety of cancer.

He described the effects of various reagents on the cancer-cell, and showed the importance of acetic acid in rendering more distinct the nucleus and nucleolus.

He eloquently defended the use of the microscope, and maintained that it had great claims to the attention of all practical men, because if by means of it they perceived the cells which ordinarily accompanied cancer, their diagnosis became more certain, and they knew that they had to deal with either fibro-plastic, scirrhus, or encephaloid tissue.

As regarded the *treatment of cancer*, he recommended early extirpation. He had found that, after the removal of a cancer, there were a great many of the—he would not say characteristic, but *habitual* cells of cancer, disseminated through all the normal tissues near the spot affected, and also through those which were at some distance from it. Hence, in excising cancers, we ought to cut away a considerable portion of the surrounding tissues, even though they appeared healthy and unchanged, and that we ought even to cauterize them afterwards. By microscopically examining the discharges from wounds left by operations for the excision of cancer, he had ascertained that, so long as the pus contained cancer-cells mixed with the pus-cells, cicatrization did not

take place; and that, simultaneously with the disappearance of the cancer-cells, the process of reunion commenced. He was, therefore, of opinion that, after removing a cancer, we should allow the wound to suppurate for a certain time, in order to allow the cancer-cells to be eliminated by this channel.—*Monthly Journ. Med. Sci.*, Jan. 1855.

39. *On the Formation and Extension of Cancer-Cells in the Neighbourhood of Cancer, and their Importance in the Performance of an Operation.*—The following are the conclusions of SCHROENER VAN DER KOLK:—

1. Through an interchange of material, taking place between cancer-cells and intercellular fluid, the latter acquires the property of forming new nuclei and cells of a similar nature.

2. This intercellular fluid passes, along with the parenchymatous fluid pervading the sound parts, into the textures adjoining the tumour. The parenchymatous fluid thus acquires the same constituents and tendency to form similar cells, which now become developed among the healthy surrounding tissue, in the course of the areolar membrane.

3. On account of the minuteness and small number of the last-mentioned cells, their presence cannot be detected with the naked eye; so that the surrounding parts may appear to be perfectly sound, notwithstanding that they contain the germs of the advancing formation of cancer.

4. It is, therefore, of importance, in removing cancer by operation, not only to take away, at the same time, a large quantity of the adjacent sound parts, but also to examine the innermost sectional edges under the microscope, in order to ascertain whether any trace of cancer-cells in process of formation is to be discovered in them.

5. The existence of burning, shooting pains in carcinoma, may be taken as a proof that the cancer-cells have reached the neighbouring nerves, and the disease can then scarcely be looked upon as a local one, in which an operation might be permanently successful.

6. By the absorption of the infected parenchymatous fluid through the lymphatics and veins, the whole body seems to become more or less tainted, so that secondary cancer ensues in distant situations, when, as is self-evident, operation can no longer be thought of.

7. This altered parenchymatous fluid penetrates the organic tissues, which are washed by it, the sarcolemma of the muscular fibres, the tubes of the nerves, &c. These membranes, too, both the sarcolemma and the walls of the nervous tubes, appear to take up the altered nutritive fluid; the consequence of which is, that both within the sarcolemma and the nervous tubes similar nuclei and cells arise, accompanied with an absorption of the muscular fibre and of the contents of the nerve, and attended with the deposition of fat, by which these parts waste and are destroyed, while the surrounding membranes (sarcolemma and walls of the nervous tubes) remain.—*Brit. and For. Med.-Chirurgy. Rev.*, April, 1855, from *Lederlansch Lancet*, Sept. 1853.

40. *Cancer with Benign Clinical Features.*—Dr. ROBERT D. LYONS, in a paper recently read before the Dublin Pathological Society, states that he has on several occasions seen examples of undoubted cancer, which wanted many of the so-called malignant features; and he relates the following case as the most remarkable one of the kind, in many respects, which he has met with.

"The patient was aged 37. At the time I saw him, the disease had been in progress somewhat more than a year. He had enjoyed excellent health, and was not aware of the existence of any disease, until, having been tripped one day in the street by a dog, he shortly afterwards noticed a small tumour on the inside of the left groin, which subsequently increased in size, but gave him little pain. Indeed, with the exception of the inconvenience caused by its gradually enlarging dimensions, he had no cause of complaint whatever. Advice, however, was sought, and the question of the propriety of operation was entertained; but a second opinion having been taken, it was finally determined to allow matters to follow their own course.

"At a subsequent period, I saw the case, when the following observations