

appearance was certainly more like urates than phosphates, and I found, on examining it for myself, that the acid only dissolved the precipitate with heat, but had no action on the cold urine. It was consequently placed under the microscope, and was found to consist almost entirely of globular urates, with high refractive powers, mixed with some crystals of oxalate of lime. Their presence in nearly neutral urine was readily explained by the scanty amount of secretion consequent on the previous severe diarrhoea, but their insolubility by heat was a phenomenon for which I was not prepared. On the following day the urine was more decidedly acid, but there was no precipitate. On boiling, however, a copious precipitate was thrown down, which was immediately dissolved on the addition of a few drops of acid. I have already alluded to the difficulty of explaining the precipitation of phosphates in the presence of acid; when the deposit only occurs after the application of heat the change is still more difficult of explanation, because the urine after being boiled becomes rather more acid than before, as I ascertained distinctly in this case, and the granular precipitate does not dissolve readily with acid after it is allowed to cool.

Formerly we used to be taught that the presence of urates was only due to acidity, of phosphates to alkalinescence of the urine; but that is a very limited view. It was probably only put forward in this broad form as antagonistic to the common expression of those days, that one or other deposit showed an excess of uric or phosphoric acid, the "lithic-acid diathesis" and the "phosphatic diathesis" of Dr. Prout. It is certain that such a conclusion is very likely to be erroneous, although, perhaps, it contains some element of truth. All that can be said with certainty at present in regard to the uric-acid salts is, that when they are precipitated there must be a certain excess of acidity, and if not an excess of the salt, at least a disproportion between it and the water as ingredients of the urine.

With reference to the presence of phosphates, I conceive that we must assume the same disproportion to exist between the water and the earthy salts whenever the precipitate is abundant, especially when the urine is not decidedly alkaline. It is not an uncommon practice in the present day to maintain a certain degree of alkalinescence of the urine both in rheumatism and gout. In such cases it is quite remarkable how the amount of earthy deposit varies while the urine is freely alkaline, but I have long learned by experience that a copious deposit is an indication of depression, and is sure to be followed, if not accompanied, by other symptoms of a corresponding kind. At one time I used immediately to diminish the dose of the alkali, but I have recently observed that, without making any other change in treatment, the addition of a few grains of quinine daily will have the effect of at once diminishing the amount of deposit, and will enable the patient to persevere with such a dose of alkali as he could not otherwise have taken with impunity. Believing as I do that in both forms of disease it is desirable to neutralise as far as possible excessive acidity, the effect of the quinine is to me a point of great importance in the treatment of these disorders.

Bruton-street, July 12th, 1871.

ON THE ORIGIN OF CANCER.

By CAMPBELL DE MORGAN, F.R.S.,

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(Continued from page 81.)

I BELIEVE no one doubts that the secondary growths of cancer are often the results of implantation, as are those of non-cancerous diseases. The difficulty which lies in the way of the admission of an invariable local origin is the immunity for a protracted period found at times after operation. A patient may go on for eight or ten years, or longer, apparently free from disease, and then it may return. The return in these cases is often in the neighbourhood of the original tumour. This opens up a very wide field of inquiry. As has been before stated, those who maintain the blood-origin of cancer suppose that there must be

another and concurrent cause—a certain aptitude in the tissue to develop the disease. The tissue alone may be in a fit state, or the blood alone in a fit state; but no cancer will be formed. It is argued then that, when the operation was performed, either the whole existing disease was taken away, and the blood, although still containing the poison, had no fit tissue on which to act; or that the operation was done at a time when the blood had been deprived of its poisonous qualities by the previous growth; and that, though the tissue might be fit, the blood was not. Hence a length of time might elapse before the two conditions came into conjunction. How long may the blood be cancerous without cancer being developed? It may be for life according to this view; but it would be to my mind more difficult to receive this than to accept all, and more than all, that lies in the way of the contrary hypothesis.

There is no lack of facts of development which we cannot explain, and are content to say that they occur in obedience to a law, but in connexion with which we never imagine an altered state of blood. Two children of different sexes are born, and the mammae remain alike up to a certain age, and then in one only they begin to develop; they remain effective for a certain time, and then, while other organs are in full activity, they begin to decline. In some families the hair will fall at a certain age from a definite part of the scalp, elsewhere it will grow as well as ever; or a patch of hair may become white, while in other parts it will retain its natural colour. Cartilage will remain as cartilage for eighteen years, and will then ossify. In the first of these cases we see a tissue remaining quiescent for years and then starting into activity; in the second, the degeneration of a structure limited to a small spot, the remaining structure undergoing no change; in the third, a tissue undergoing continued and active growth of one kind for a long period, and then taking on a new phase of development. Now I believe that in instances such as these—and they are plentiful both in the animal and vegetable world—we have a clue to the fact that cancer may be present *potentially*, and remain for a time quiescent, to be developed in due course, independent of the condition of the blood. In breast cancer it is constantly noticed that for years before the disease has declared itself the gland has been functionally imperfect, if not physically different. The patient has been unable to nurse with it, or she has always noticed that it was not so full as the sound one, or that the nipple has never been so prominent. In the testicle it may happen that the diseased gland has always been a small one, or that it has not fully descended. In the case of epithelioma, a wart, or mole, or scale, has been seated on the skin for thirty or forty years perhaps before the disease has developed itself. I have been more and more led to the notion that the conditions determining a cancer exist in the tissue from the first, and that the disease has declared itself in due course, just as has the local degeneration of the hair. But I should not limit this to cancer. The same would apply, I believe, to many other tumours and degenerations; and I can see no greater reason for accusing the blood as a cause of these growths and degenerations than I should for the local baldness or the local greyness. The very conditions under which both scirrhus and epithelioma arise strongly confirm this. In the latter especially it often happens that there are several similar moles or scales which remain unchanged for years, and then the disease develops rapidly in one, the others still remaining quiescent. Now considering the recognised proneness of such scales to become epitheliomatous, we should expect that, if a cancer poison were in the blood, several of these would take on cancerous action. But this we never see. In scirrhus, as has been before mentioned, various forms of aberrant growth frequently precede or accompany the development of the cancer, but no cancer attacks them. What is seen in cancer in relation to these outgrowths is seen as often in connexion with other tumours not cancerous, and not even malignant in their nature. This will be again referred to when the hereditary tendency to the disease is discussed. It is admitted, then, that in some individuals cancer is impressed on the tissue long before the tumour shows itself, even from the embryonic state. But besides this, it seems not improbable that in many there is a condition of tissue, in one part or another, which readily takes on cancerous degeneration under irritation, while without that irritation it might never occur at all. It is only what we see in the

case of other diseases. A thousand people may have a blow over the tibia, and in one will form an exostosis, in another a myeloid tumour, in a third an enchondroma, while in the others, though the inflammatory action may be of greater or less intensity, no special disease will arise. There must be some predisposition to the disease, yet there is no ground for belief that it would have arisen without some exciting cause. I know no disease which better illustrates this than the cicatrix keloid before referred to. In some situations it will follow the healing of any wound. Yet the individual may be quite healthy, and the skin apparently quite natural, and in the same subject wounds may be made in other parts of the body and will heal without the development of keloid. Putting time out of consideration, this is just what we see in the return of rodent cancer or of epithelioma round the edges of a part cicatrised after operation. But though a greater tendency to cancer may exist, and though that tendency may exist in more than one part, that is widely different from the existence of a blood poison, which, when the disease is removed from one part, will be seeking some other habitation in which to work mischief.

What is here stated with regard to the long existence of a cancerous or other tendency in a tissue will apply to the degree of growth of cancer elements when they actually exist. In the young girl the breast tissue is present, and there is abundant growth going on in the body, but the special development of the mamma only takes place at a certain age, which may be hastened or retarded by physical or by moral circumstances. The same may be seen in a fatty tumour even. A small lump will be felt under the skin, and remain unchanged for years, and then rapidly grow into a large lipoma; but the general fat of the body will not increase—it may diminish. The same is often seen in other tumours. Now let us take a common case. A patient tells you that she has that day discovered that she has a lump in the breast. You find one as large as a walnut. It is watched, and at the end of many months has made but a slight increase. The growth of a cancer will probably be, *cæteris paribus*, proportioned to the amount of active growth elements, and should be the quicker the larger the cancer has become—i. e., the more of these elements it contains. If, then, but a very slight enlargement has taken place in the six months or more during which the tumour has been watched, how long may it not have been since the first true cancer tissue was developed? In some cases it may have been years. Of course in the large majority of cases the growth is from the first far more rapid. Take now a case on which an operation has been performed. Seven or eight years may elapse, and then there may be return. These, again, are exceptional cases; but the disease may have been present, though inactive, all that time. Two of the last cases which have occurred to me are these. I removed a cancer from the breast of a lady in July, 1866. She remained quite well, save that a little fulness was found in the axilla in 1869, which underwent no perceptible change till the end of 1870, when there was a distinct tumour. This began to grow rather quickly, and I removed it in March of the present year. It consisted of a congeries of enlarged cancerous glands. In November, 1864, I removed a cancer from the breast of a lady who had had symptoms of the disease for more than three years. For six years there was no appearance of return. Then there was found a small hard tumour, not larger than a small pea, about two inches to the outside of the cicatrix. There appeared also a fulness above the clavicle on the same side. The nodule near the cicatrix is now a little larger, and the skin is adherent to it. The gland above the clavicle has increased to a much greater extent. She has no constitutional symptoms, however. Are we to suppose that in these cases the cancerous state of blood was in abeyance for years? or that the cancer poison was in the blood, but found no fitting tissue all this time, and that then the axillary and cervical glands, and the single point near the cicatrix on the diseased side, assumed the proper condition? This would be strange, considering how rarely scirrhus is developed primarily in the lymphatic glands and skin. Is it not much more likely that some cancer elements were in the glands or beneath the skin at the time of operation, and remained dormant—just as a lipoma or an atheroma will remain dormant? If this may be the case for two or three years, there is no assignable reason why it may not be so for double that time.

As if to illustrate this point, my colleague, Mr. Nunn, called my attention, while this was being written, to the case of a patient in the hospital, who was admitted six weeks ago with a very large ulcer of the leg. Soon after his admission the house-surgeon grafted several small pieces of skin upon it, and several more at different times after. For more than three weeks these grafts remained as mere raised spots on the ulcer, then suddenly started into activity, and in the course of a week or less the whole ulcer was nearly cicatrised over. The patient was a red-faced, sturdy countryman. It might be said that the cases do not admit of comparison. I believe that they do, as showing that, under certain conditions which we do not recognise, the actions of growth will be in abeyance; and that then, under certain conditions of the nature of which we are equally in ignorance, growth of unusual energy will set in. The vegetable seed, we know well enough, can be kept for centuries without action, and can be made to germinate under recognised amounts of heat and moisture. In the animal kingdom, however, the higher we proceed the more difficulty do we find in determining the vital stimuli necessary to bring each part into action. I fully admit that there is great difficulty in accounting for the long delay which may occur between removal and recurrence. I fully admit that, in some cases, there may be the same disposition in other tissues to develop cancer *de novo* as there was in the tissue originally attacked; but the difficulty is not removed by accepting the doctrine of blood-poisoning, when all the facts are taken into consideration, especially the almost invariable return in some organ near the original seat, or in some internal organ, neither being the seat, save in very rare instances, of the primary disease.

The retrogression of cancer, either standing alone or connected with the development of other disease, is a subject of great interest. It has been proved from the records of the cancer cases in the Middlesex Hospital, that the occurrence and growth of cancer are not influenced by the majority of diseases.* But cancer has been known to be arrested in its growth and to retrograde spontaneously; and the development of pulmonary tubercle seems sometimes to produce the same effect.

The cases of spontaneous recession of cancer are too few to allow of any general inference being drawn from them. Instances have been seen of the disappearance of cancer while tubercle has been developing. But Mr. Sibley's paper shows that this is by no means a necessary event. Out of 173 cases in which a post-mortem examination was made, there were 11 in which active tuberculosis was present at the time of death, and not one in which the cancer appeared to be influenced by the presence of tubercle. The occurrence of cancer, moreover, is very common in families of which other members are consumptive. While it may be admitted that cancer and tubercle do not often coexist, there is no proof that they are antagonistic the one to the other. The very fact of the general healthiness of cancer patients may give one reason why tubercle is so rarely found in them. Another may be the fact that cancer is generally the disease of advanced life,—tubercle of early life. Before any conclusion can be arrived at as to the relation of opposition or otherwise between cancer and tubercle, and of the bearings of that relation on the question of blood disease, it must be ascertained whether the same does not exist between tubercle and other morbid non-cancerous growths.

(To be concluded.)

ON GASTROTOMY: WITH CASE.†

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THE term "gastrotomy," applied by Sédillot to those operations which are performed for the purpose of providing a new and permanent aperture to the stomach, excludes, of course, all those cases in which it is opened for the removal of foreign bodies.

There is some advantage in this beyond the mere improvement in terminology: it serves to group together, for more accurate comparison, those instances in which the

* Sibley: Med.-Chir. Trans., vol. xlii.

† Read before the Medical Society of London.