

descendants would have lost the characters of the fowl and have become another species. No precise estimate has been arrived at, and, indeed, one does not see how it is possible to obtain it, of the length of years which might be required to convert a variation capable of being transmitted into a new and definite specific character.

The circumstances which according to the Darwinian theory determined the perpetuation by hereditary transmission of a variety and its assumption of a specific character depended, it was argued, on whether it possessed such properties as enabled the plant or animal in which it appeared to adapt itself more readily to its environment—i.e., to the surrounding conditions. If it were to be of use the organism in so far became better adapted to hold its own in the struggle for existence with its fellows and with the forces of nature operating on it. Through the accumulation of useful characters the specific variety was perpetuated by natural selection so long as the conditions were favourable for its existence and it survived as being the best fitted to live. In the study of the transmission of variations which may arise in the course of development it should not be too exclusively thought that only those variations are likely to be preserved which can be of service during the life of the individual, or in the perpetuation of the species, and possibly available for the evolution of new species. It should also be kept in mind that morphological characters can be transmitted by hereditary descent, which, though doubtless of service in some bygone ancestor, are in the new conditions of life of the species of no physiological value. Our knowledge of the structural and functional modifications to be found in the human body, in connexion with abnormalities and with tendencies or predisposition to diseases of various kinds, teaches us that characters which are of no use, and, indeed, detrimental to the individual, may be hereditarily transmitted from parents to offspring through a succession of generations.

Since the conception of the possibility of the evolution of new species from pre-existing forms took possession of the minds of naturalists, attempts have been made to trace out the lines on which it has proceeded. The first to give a systematic account of what he conceived to be the order of succession in the evolution of animals was Ernst Haeckel of Jena, in a well-known treatise. Memoirs on special departments of the subject, too numerous to particularise, have subsequently appeared. The problem has been attacked along two different lines: the one by embryologists, of whom may be named Kowalewsky, Gegenbaur, Dohrn, Ray Lankester, Balfour, and Gaskell, who with many others have conducted careful and methodical inquiries into the stages of development of numerous forms belonging to the two great divisions of the animal kingdom. Invertebrates, as well as vertebrates, have been carefully compared with each other in the bearing of their development and structure on their affinities and descent, and the possible sequence in the evolution of the vertebrata from the invertebrata has been discussed. The other method pursued by palæontologists, of whom Huxley, Marsh, Cope, Osborne, and Traquair are prominent authorities, has been the study of the extinct forms preserved in the rocks and the comparison of their structure with each other and with that of existing organisms. In the attempts to trace the line of descent the imagination has not infrequently been called into play in constructing various conflicting hypotheses. Though from the nature of things the order of descent is, and without doubt will continue to be, ever a matter of speculation and not of demonstration, the study of the subject has been a valuable intellectual exercise and a powerful stimulant to research.

We know not as regards time when the fiat went forth, "Let there be Life, and there was Life." All we can say is that it must have been in the far-distant past, at a period so remote from the present that the mind fails to grasp the duration of the interval. Prior to its genesis our earth consisted of barren rock and desolate ocean. When matter became endowed with life, with the capacity of self-maintenance and of resisting external disintegrating forces, the face of nature began to undergo a momentous change. Living organisms multiplied, the land became covered with vegetation, and multitudinous varieties of plants, from the humble fungus and moss to the stately palm and oak, beautified its surface and fitted it to sustain higher kinds of living beings. Animal forms appeared, in the first instance simple in structure, to be followed by others more complex, until the mammalian type was produced. The ocean also became peopled with

plant and animal organisms, from the microscopic diatom to the hugh leviathan. Plants and animals acted and reacted on each other, on the atmosphere which surrounded them, and on the earth on which they dwelt, the surface of which became modified in character and aspect. At last Man came into existence. His nerve-energy, in addition to regulating the processes in his economy which he possesses in common with animals, was endowed with higher powers. When translated into psychical activity it has enabled him throughout the ages to progress from the condition of a rude savage to an advanced stage of civilisation; to produce works in literature, art, and the moral sciences which have exerted, and must continue to exert, a lasting influence on the development of his higher being; to make discoveries in physical science; to acquire a knowledge of the structure of the earth, of the ocean in its changing aspects, of the atmosphere and the stellar universe, of the chemical composition and physical properties of matter in its various forms, and to analyse, comprehend, and subdue the forces of nature. By the application of these discoveries to his own purposes man has, to a large extent, overcome time and space; he has studded the ocean with steamships, girdled the earth with the electric wire, tunneled the lofty Alps, spanned the Forth with a bridge of steel, invented machines and founded industries of all kinds for the promotion of his material welfare, elaborated systems of government fitted for the management of great communities, formulated economic principles, obtained an insight into the laws of health, the causes of infective diseases, and the means of controlling and preventing them.

When we reflect that many of the most important discoveries in abstract science and in its applications have been made during the present century, and indeed since the British Association held its first meeting in the ancient capital of your county 69 years ago, we may look forward with confidence to the future. Every advance in science provides a fresh platform from which a new start can be made. The human intellect is still in process of evolution. The power of application and of concentration of thought for the elucidation of scientific problems is by no means exhausted. In science is no hereditary aristocracy. The army of workers is recruited from all classes. The natural ambition of even the private in the ranks to maintain and increase the reputation of the branch of knowledge which he cultivates affords an ample guarantee that the march of science is ever onwards, and justifies us in proclaiming for the next century, as in the one fast ebbing to a close, that great is science and it will prevail.

A Clinical Lecture

ON

THE EXPECTANCY OF LIFE IN CASES OF CANCER OF THE BREAST.

Delivered at University College Hospital on July 14th, 1900,

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GENTLEMEN,—The gloom which has hung over the question of the expectancy of life in cancer of the breast has hitherto been fully justified. We know from the experience of the older surgeons that the majority of women afflicted with this disease are doomed if untreated by operation to death within two years. And even those who in former years submitted to the removal of the breast for this disease in most cases suffered from speedy local or general recurrence. But within the last few years some rays of hope have been cast upon this dark chapter of surgery by the achievements reported in various directions of the results of more recent and improved operations. At the present moment the outlook of a woman with cancer of the breast is

by no means so desperate as, say, 20 years ago, either as regards the prospect of prolongation of life by operation or complete immunity from recurrence. This is due to many causes operating together and reacting one upon the other. Before all things it depends upon the spread of knowledge among the general population and the profession in regard to the danger of neglecting any tumour of the breast. This is as much as to say that the knowledge of the pathology of breast carcinoma is more accurate and general than in earlier days. The particular way in which this more general diffusion of knowledge has influenced our results can perhaps be most briefly expressed by putting before you some of the axioms now pretty generally received in relation to this question.

In the first place, our pathological knowledge is now much more accurate than formerly in regard to the commencement and mode of dissemination of cancer of the breast. The following are the pathological axioms which most directly influence our surgical practice in dealing with the disease. 1. Carcinoma of the breast is probably for a short time a purely local disease. 2. It very soon begins to spread from the primary focus into the surrounding tissues by way of the lymphatics. 3. Three systems of lymphatics participate in this process—the epi-mammary under the skin, the mammary in the breast substance, and the sub-mammary in the fascia covering the pectoral muscles. 4. The lymphatic glands of the axilla receive most of the lymph and cancer germs from these systems and act as a dam to their further progress for a time. In some rarer cases the flow of lymph takes place—in part, at all events, towards the lymph-channels underlying the pleura. 5. It is probably possible to reinfect otherwise healthy tissues during an excision of the breast by the juices of the infected material in process of removal. This may take place either by the knife or other instruments, the surgeon's hands, or by actual contact of a cut surface of the infected area with the dissected surface. 6. When the cancerous infection has reached the underlying muscles it usually corresponds with a spread to the internal organs.

Bearing these points in mind the following surgical axioms have come to be pretty generally held and acted on in the operative treatment of the disease with great advantage to the patient. 1. An excision of the breast to be effectual must be early. 2. The overlying skin, subcutaneous fat, pectoral fascia, axillary fat and glands with the entire breast must be widely and deliberately removed. 3. During this dissection all division of carcinomatous tissue should be avoided and if inevitable freshly sterilised instruments, &c., must be used. 4. As little direct handling of the parts to be removed as possible should take place. 5. Arrest of all immediate bleeding should be accomplished preferably by forcipressure, and if not by ligature and subsequent oosing by elastic pressure and bandaging. Rest for the wound and the patient should be secured for at least two weeks. 6. All this extensive removal can be accomplished with but little risk to the patient in fairly early cases. 7. When the disease has infected the muscles palliation alone can be expected from surgical interference, and it is questionable whether very extensive operations involving risk from shock ought to be performed with only this end in view. Indeed, the apparently paradoxical rule (which for my own part I have followed of late) appears to be justified—viz., that the more localised the primary focus of carcinoma in the breast is the more wide reaching should be the excision on the above lines. That is to say, there is in such cases a fair prospect of complete eradication of the disease by a wide-reaching operation, and therefore it is justifiable to run some risk of shock. The converse rule appears also to have much to recommend it—viz., that the more wide reaching the disease the more clearly should the operator keep mere palliation in view and by limiting his operation avoid the risk of extreme shock.

These considerations at all events have materially influenced my own practice of late years and I should now like to place before you the results of 100 consecutive excisions of the breast which I have done. They have been carefully analysed by a brilliant former student, Mr. J. E. Simpson, in his Liston Prize Essay for 1898, and I have only for your benefit to-day brought the record down to date which he had hoped to do had he lived. He spared no pains to make the analysis accurate by looking up the patients and examining the documents in the Registrar-General's office at Somerset House, and his industry and ability are known to us all. The first point he examined

into was the nature of the disease for which excision was performed. The following were his results:—

Nature of tumour.	Number of cases.	Nature of tumour.	Number of cases.
Scirrhus	82	Cystic adenoma... ..	1
Colloid cancer	2	Duct papilloma... ..	1
Scirrho-encephaloid ...	2	Chronic fibrosis... ..	1
Duct cancer	4	Doubtful	1
Cystic degeneration ...	4		
Fibro-adenoma	2	Total	100

This gives us a proportion of 90 malignant cases to nine of non-malignant and one doubtful.

The next point studied was the gross mortality in 100 excisions. This comes out as follows, and you will notice that the deaths fall mostly to the earlier years when procedures were far less perfect than of late.

No. of cases.	Cause of death.	Date of death.
1	Pyæmia.	1878—3 months after operation.
1	Septicæmia.	1880—1 month " "
1	Cellulitis.	1888—8 days " "
1	Syncope.	1894—9 " " "
1	Pleurisy.	1895—4 " " "

The first three of this group of deaths from operation would probably not appear in any subsequent series of cases so much has been the improvement in wound treatment since 1888. The mortality, therefore, to-day is probably far below this 5 per cent. But such cases as that of syncope occurring in an old lady on the eighth day when the wound was perfectly healed per primam belong to the accidents of our experience against which we cannot be shielded by any amount of care. In the case of pleurisy there was no injury of the pleura so that the cause was probably the exposure in a feeble woman. Leaving out the cases of so-called duct cancer as perhaps not strictly carcinoma there remain 86 cases of undoubtedly malignant character, including scirrhus (82), colloid (2), and scirrho-encephaloid (2). These I have grouped in the following tables in a way which gives us some idea as to the "expectancy of life in cancer of the breast."

TABLE I.—Cases known to have Died long after Excision, presumably from Recurrence, though one case certainly died from Calculous Pyelitis without Recurrence, and possibly others.

Lived over 7 years ... 1	} 26.9 per cent. over 3 years.	Lived over 2 years 14
" " 6 " ... 3		" " 1 " 14
" " 5 " ... 1		" under 1 " 10
" " 4 " ... 2		
" " 3 " ... 7		Total 52

It is evident from this that 26.9 per cent. have lived over three years and yet succumbed to cancer. Mr. Simpson traced a further nine cases for over three years, but found no record of death in any.

Here we see that 52 cases have died within intervals ranging from two months to over seven years. In Table II. you have 20 cases known to be alive at the intervals given; and of these 15 are or were alive over three years.

TABLE II.—Cases of Excision of the Breast known to be Alive at the Intervals given in July, 1900, grouped with those Alive and without Recurrence respectively 10 years and one month, one year and seven months, and one year and two months when last heard of more than a year ago.

Living after 14 years ... 1	} 15	Living after 2 years 3
" " 10 " ... 1		" " 1 " 2
" " 8 " ... 2		
" " 7 " ... 1		Total 20
" " " ... 3		
" " " ... 1		
" " 4 " ... 3		
" " " ... 3		

TABLE III.—Cases known to have Died taken together with those still Alive and Examined as to Duration of Life.

Lived over 13 years ... 1	Lived over 1 year 16
" " 10 " ... 1	62
" " 8 " ... 2	" " 1 month... .. 10
" " 7 " ... 2	72
" " 6 " ... 6	Untraced... .. 9
" " 5 " ... 2	81
" " 4 " ... 5	Died from operation 5
" " 3 " ... 10	86
" " 2 " ... 1	

From this it will be seen that 16 per cent. lived over five years and 33·7 per cent. over three years. These figures appear to justify the hope that the expectancy of life is improving as time goes on. It is clear that over 33 per cent. live more than three years after the operation, which has suggested to some surgeons that such cases should be regarded as permanent cures. But some of the cases (26·9 per cent.) have died most likely from recurrence in all but one after an interval of immunity of over three years, so that such a presumption is unwarranted.

Another interesting point comes out from this analysis—namely, that only seven suffered from *local* recurrence. Those in whom the disease has returned have shown it in internal parts. This is another good feature of the modern operations, local recurrence being most depressing and painful, while internal generalisation is often not so.

These cases have been accurately recorded and in almost every case the diagnosis with the naked eye has been confirmed by microscopical examination by various competent observers, mostly the hospital registrars. In the oldest case in the list the glands were extensively invaded as well as the breast, and a bad prognosis was given at the end of the operation, and yet at the end of 14 years the patient is well and free from any trace of cancer. This and some of the other cases seem to justify the hope that this disease can be eradicated by the knife. And it is not too much to hope that with still further improvement in early diagnosis and treatment a far larger proportion of cases may be saved. If the four cases of duct cancer were included the figures would be still more favourable.

ON THE PATHOLOGY AND THERAPY OF ANGINA PECTORIS.

By PROFESSOR THEODOR SCHOTT, M.D.

AMONG chronic diseases of the heart angina pectoris has in the passing century attracted the interest of the medical profession in a remarkable manner. Ever since the first excellent description by Heberden appeared more than 100 years ago the various theories advanced as to the nature and the causes of stenocardiac accesses have been in sharp opposition. This may be explained in great part by the fact that, as Latham justly remarks, angina pectoris must be regarded not as a disease *sui generis* but as a series of symptoms. Even, however, in regard to angina pectoris vera—to which the following observations are exclusively directed—opinions still remain unsettled and divided. The hypothesis of Heberden and of Latham that the anginal fit is to be regarded as a real tetanic contraction of the cardiac muscle has found at different times a varying number of adherents. A tetanic cramp of the whole cardiac wall would, if it prevailed but a short time, certainly destroy human life; but a tetanic contraction of a part of the heart might take place without deadly result, as has been demonstrated latterly by experiments of Basch and Grossmann. In direct opposition, however, is the theory of Parry which has been effectively supported by the celebrated Stokes. According to this opinion angina pectoris must be attributed, not to an increase but to a further reduction of the muscular energy of a heart already enfeebled. According to Parry the stenocardia is a sort of syncope with a preceding strong oppression or pain in or near the heart, the consequence of an organic lesion the effects of which are brought into

evidence by a lowering of heart power. Thus Parry opines that the symptoms are caused by an accumulation of blood in the cardiac cavities. This explanation has been powerfully supported in Germany by the well-known Professor Traube, an authority on the subject of heart diseases. Traube also believes that there is loss of power of the heart muscle, but he thinks that the access results from a rapidly increasing tension of the walls of the ventricles under the influence of an accelerated over-filling of the cardiac cavities, in consequence of which the motor as well as the sensor nerves of the heart respectively are subjected to a degree of pressure or tension sufficient to account for the sudden pain. Samuelson, whose experiments are founded upon an artificial constriction of the coronary vessels, differs from Traube only in that he supposes the over-filling of the heart to take place, not in the left ventricle but in the right auricle. French physicians, such as Potain, Germain Sée, and others, attribute the attack to an ischaemic state of the cardiac muscle, attributing the pains to causes similar to those observed when there is a blocking of the arteries as in endarteritis obliterans and in similar processes. But the ischaemic state of the cardiac muscle may be taken to induce a weakness of the organ, so that this theory agrees in the main with that of Parry and Stokes. Although the latter view commands a majority of adherents—as may be gathered, for example, from the opinion expressed at the meetings of the London medical societies—it has always found opponents as at the Congress of Internal Medicine at Wiesbaden (1891) where Vierordt, one of those who opened the discussion, revived the question whether the stenocardiac fit is really dependent on debility of the heart. The principal argument for that doubt is that the pulse does not of necessity show any corresponding change—that there may be no diminution of volume—that in some cases, indeed, it becomes stronger. Another argument is that manifest signs of weakness of the heart are not invariably discoverable.

Before we discuss these theories let us consider what processes are found to be associated with angina pectoris vera. They are, in the first place, sclerosis of the coronary vessels, alterations of the aortic valves, and aortitis, especially that form which causes an ectasia of the ascending portion. (In a case of angina pectoris combined with insufficiency of the mitral valve which has been under my treatment these three years I observed latterly the development of an aortic stenosis; so that also in this case, though there are no signs of rigidity of the vessels, the sclerotic process at the heart cannot be regarded as doubtful. I may add that latterly I have seen several similar cases.) It is known, however, that the sclerotic change in just this part of the aorta is not without an influence on that spot from which the coronary vessels take their origin, and not seldom it may be noted post mortem that, though the other parts of the coronary arteries still show a sufficient lumen it is reduced at the point referred to in such a degree that a bristle can scarcely be introduced. The next consequence of such an arterial change must be that as a result of decreased circulation the muscles of the heart are no longer adequately nourished. Weakening of the heart and defect of energy follow of necessity. Moreover in the stenocardia of angina pectoris vasomotoria—the pseudo angina resulting from so many different causes may be left aside—the heart is called on to exert itself against a contracted arterial system, so that in such cases, also, there is a sufficient cause for a sudden relative weakness of the heart. The conditions calculated to induce a weakening of the heart are apparently present in both instances, and it is easy to understand that every demand for additional effort is likely to be followed by an additional, as well as sudden, failure of energy. The suddenness of the paroxysm may be also directly attributable to the retardation of the circulation within the coronary vessels. A moderate distension of the cardiac muscle may easily lead to a temporary occlusion of one or more coronary vessels at the seat of an already existing constriction. In other cases a thrombus or an embolus are the cause of the block, and we know that such vascular conditions give rise to violent as well as sudden pain in other parts of the muscular system. The experiments of Samuelson, of Cohnheim, of Schultess-Reihberg, and of many others have furnished sufficient evidence of this.

Let us now inquire whether clinical observation lends confirmation to what has been suggested. I restrict myself to my own experience, and I would in the first place