

representative of the larger hospitals, provincial as well as metropolitan. It is designed to include all subjects susceptible of useful illustration by pictorial aid and although, as far as may be found practicable, the subjects will be classified, it will not be considered that any one has been completed in the sense of being concluded. Each plate or group of plates will, as far as possible, be made to constitute independent illustrations of the malady concerned and will be accompanied by a full narrative of the case or cases concerned. On this plan supplementary plates may from time to time be introduced and more recent observation thus made available. To keep the work well up to date and to insure its usefulness for purposes of diagnosis will be the constant aim of those concerned in its editing.

A moment's thought will convince anyone that it is respecting the more rare class of maladies that pictorial illustrations are of especial value. The practitioner who has not learned to recognise lupus vulgaris in its common forms, which he must have seen repeatedly, is not likely to be helped by a plate. The same remark applies to a host of other conditions with which even the youngest practitioner cannot have failed to become familiar. After all a picture cannot compete with the living patient in adequacy of illustration and it is little better than wasteful expenditure to depict conditions which are matters of everyday experience. It is respecting those which do not come frequently under notice that help is especially needed. It is obvious that it would conduce vastly to the progress of clinical knowledge if all the examples of rare maladies were correctly recognised. Diseases which are now but too often allowed to pass as being little more than curiosities would be found to be sufficiently common to admit of intelligent classification and to be made to supply their quota to the stock of well-investigated observations. Nor is the addition of a new malady to our nosological list, although it may be a very rare one, of less than the very highest value for the advancement of medicine. The results which have accrued from the discovery of Myxœdema may be allowed to stand as a good instance of what is meant. The next issue of our Atlas will contain a portrait taken from a patient recently under the care of Dr. Gee in St. Bartholomew's Hospital which exhibits the very peculiar conditions which Hebra described as Sarcoma melanodes. Although it is 35 years since Hebra observed his first case this malady has received only very scant recognition from subsequent observers. Yet it is a well-characterised malady, and standing as it does as a connecting link between innocent and malignant processes and between those of childhood and those of senility it may be destined to throw very valuable light on some of the most pressing questions of the day. Who can doubt that during the last quarter of a century many examples of it have occurred and have escaped recognition and thus been lost to medical science? Whilst, however, it is claimed for our Atlas, as one of its most important functions, that it will help to the prompt recognition of exceptional forms of disease, it must at the same time be clearly understood that, after all, its principal vocation is in dealing with what is common. A glance at the index to the fasciculi already published will prove this. We have dealt not only with leprosy, Sprengel's shoulder, albinism, the exceptional forms of syphilis, morphœa, xanthoma, and arsenical cancer, but with the ordinary results of arsenical poisoning, with Colles's fracture (in detail) and other forms of fracture and dislocation as illustrated by the x rays; with the eruptions caused by insect bites, cretinism, scabies, pemphigus of pregnancy, urticaria, and many other diseases of common occurrence.

The next fasciculus, which will, as I have said, contain Dr. Gee's portrait of a very rare malady, will contain others illustrating flea-bites and ringworm, and those in immediate preparation will be concerned with drug eruptions, vaccination, varicella, and small-pox. In this connexion it may be mentioned that it is under consideration to publish from time to time a page or two of retrospective letterpress, consisting of emendations in which references to facts or cases already published will find their place. By this means case narratives may be made more nearly complete, questions answered, and statements explained or supplemented. It will, I trust, be obvious to all that my object in asking your insertion of this letter is the hope of inducing a large number of your readers to join our ranks and thus help in the production of a work which will, it is hoped, take in the future a foremost share in the development of clinical knowledge. The council (at whose request I write) is determined to spare no pains in the

endeavour to make the Atlas useful to all branches of the profession and especially to those engaged in the absorbing routine of general practice and residing, it may be, in more or less remote districts. Its value to teachers and for hospital libraries will be obvious and it is possible that there may be some amongst those who, no longer actively engaged in practice, may feel that it has claims on their sympathetic support. The Atlas is supplied to members of the Society at a cost which has probably had no precedent in medical literature. That cost might, however, be still further diminished if we could add a few thousand more to our members' list. Such an addition would enable us not only to give more frequent fasciculi but also to publish each year a printed volume. When this shall become practicable the council has in contemplation to publish volumes of important short monographs and to select chiefly such as deal with topics which have been referred to in the Atlas.

As regards the details of subscription, I may state that the council has decided to allow the regulation which permitted the payment in advance of five years' subscriptions to secure six years' publications to remain in force a little longer. We have in stock a certain quantity of the fasciculi already published and those who make the 5-guinea payment can obtain the Atlas from its commencement and for the ensuing three years. It is not probable, however, that this privilege can continue long in force nor unless the demand should be very large can the work ever be reprinted. The stones for the coloured plates have already been destroyed and it is not improbable that the earlier fasciculi of the work may rise considerably above their present cost as soon as the stock in hand is exhausted. For future years larger editions can be printed.

I am, Sirs, yours faithfully,

JONATHAN HUTCHINSON,

Nov. 5th, 1904.

Honorary Secretary, New Sydenham Society.

CANCER GENESIS.

To the Editors of THE LANCET.

SIRS,—There is such a strong family likeness between Dr. J. Beard's conception of tumour genesis as set forth in THE LANCET of Oct. 29th, p. 1200, and the theory of tumour genesis elaborated by me many years ago, as set forth in my work on "The Principles of Cancer and Tumour Formation" (1888), that I feel bound to call attention to the coincidence, especially as it seems probable that Dr. Beard is unacquainted with my work. At the outset, I take this opportunity of expressing my conviction as to the great value of Dr. Beard's work as tending to give a right direction to tumour investigation, although most of the reasons he gives for his conclusions seem to me to be altogether inadequate. In what follows I hope to be able to show that my own steps in this direction rest on a more logical basis than Dr. Beard's, mainly owing to the wider range and generality of the conceptions adopted, although, of course, allowance must be made for the comparatively remote date of their origin, for in the region of biological speculation much has happened since I wrote.

With this object in view I propose to take the cardinal propositions of Dr. Beard's thesis *seriatim*, contrasting each one with corresponding passages in my work, while offering a few critical remarks *en passant* as to some of the speculations bearing on this matter that more particularly appertain to Dr. Beard himself.

In the first place the doctrine of the alternation of generations in the course of human ontogeny is postulated; the asexual generation is asserted to be identical with the chorion and it is from this source that the tumour germs are stated to arise. Now with regard to each of the items of this proposition Dr. Beard produces them as extraordinary novelties of his own invention; but if he will kindly read the subjoined passage from my above-mentioned work (p. 117) he will see the fallaciousness of this notion. The citation is as follows:—

If we regard the cells combining to form the higher animals as autonomous beings, possessed of morphological and physiological independence, we shall then see, although there is no such thing as true alternation of generations in the ontogeny of such animals, that nevertheless, as Haeckel has pointed out, a very complex alternation of the constituent cells does take place which has a resemblance to it. The developmental cycle commences with the union of the male and female reproductive cells, whence the cytula or fertilised germ results, which by agamic multiplication originates the mass of similar cells called the morula. These differentiate into the various cells of the blastodermic layers. By further agamic multiplication the cells of these layers originate the very many generations of variously modified

cells, whence the different tissues and organs arise. All of these polymorphic cell generations multiply agamically. Eventually, however, two of them differentiate sexually, forming the ova and sperm cells. By the union of these in the act of sexual reproduction the developmental cycle is completed. The reversion of the cells has led them back to their original starting point.

The only difference between this process and true alternation of generation lies in the fact that in the former the products of agamogenesis remain in close contact with one another to form a multicellular organism, whereas in the latter the agamic products (persons) which represent the different generations are separated and free. But the conditions prevailing in siphonophora show that this distinction is not of fundamental importance, for in these creatures the same persons—widely differentiated by division of labour—remain united into one stock that in other hydro-medusæ lead separate and independent lives.

When we examine the phenomena of gemmation and pathological neoplasia in this light it is obvious that the essential thing in both cases is the interpolation in the developmental series of additional agamic cell generations owing to excess of nutrition in these situations.

It is evident from the foregoing that the doctrine of the alternation of generations in human ontogenesis is no new idea, for it forms an important part of Haeckel's biological generalisations; neither is the idea of connecting the origin of tumours with this phenomenon a novelty, for this forms an important part of my theory of tumour genesis, as fully explained in my work on "The Principles of Cancer and Tumour Formation," from which the above passages are cited. The only novelty, therefore, attaching to this first proposition of Dr. Beard is that which asserts the identity of the chorion with the asexual generation, and this is a hypothesis of which the sole supporter at the present moment is its distinguished author. It is evidently quite impossible for pathologists to ground a sound theory of tumour genesis on a hypothesis in such an unprecedentedly rudimentary state of development. Besides, the known facts of tumour pathology directly contradict this assumption in many ways. As I have shown in my work, tumours may arise in *any* multicellular animal or vegetable being, but a chorion is only found in mammalian animals, so that if this hypothesis were correct tumours could only occur in mammals, which we know is not the case.

Dr. Beard complains with regard to the reception of his doctrine *re* alternation of generations by the scientific world that "his arguments and conclusions have been neither refuted nor confirmed but have been ignored." Well, I have to re-echo the same complaint anent the reception of my work on "The Principles of Cancer and Tumour Formation," and among the sinners in this respect I find Dr. Beard himself. Another person who has a good right to be heard in this connexion is Ernest Haeckel whose splendid work on "Generelle Morphologie" has been so generally ignored by biologists, although it is certainly the best treatise on the principles of biology that has appeared since H. Spencer's publication, on which it is a distinct advance. Here again it is surprising to find Dr. Beard among the sinners.

The second of Dr. Beard's fundamental propositions of tumour genesis centres round the doctrine of homologous twins, which, as he says, "pass gradually into double monsters, and these in their turn into the higher tumours or teratomata." In the course of my investigation I arrived at a similar conclusion, although by a different process of reasoning, as the following quotations show:—

In the ordinary course of organic evolution the growth and development of the cells proceeds in a regular and orderly manner, in accordance with the specific hereditary tendency of the whole. But the process once started does not cease on account of irregularity or because it is taking a wrong direction. Hence cells may arise at a place where they have no business, or at a time when they ought not to be produced, or to an extent which is at variance with the normal formation of the organism. In the early embryo monstrosities and malformations are thus originated; and, at later periods of development, the various pathological new formations. These extremes graduate so insensibly into one another that it is impossible to separate them.

It is generally admitted that the mass of undifferentiated protoplasmic cells, which in the ontogeny of the higher animals results from the proliferation of the fertilised germ, occasionally manifests reproductive properties similar to those of Hydra—multiplying by a kind of gemmation—so that from a single developing germ two or more gemmiparously produced individuals may proceed. The subsequent development of such gemmiparously produced individuals may be either continuous or discontinuous, as is ultimately determined by the conditions of nutrition. Thus when the division of the undifferentiated embryo into two equal parts is complete, and each of these develops into a new being, homologous twins are the result. Similarly when the division is more or less incomplete we get the various degrees of double monsters. However different in their several degrees of malformation, double monsters may be arranged in one continuous series. The most complete are those in which two bodies, nearly equally developed, are attached to one another only at a certain point. Such monsters differ but little from homologous twins. It sometimes happens that one of the bodies of such double monsters exceeds its fellow in size and degree of development.

We thus pass gradually to another class of these malformations in

which only one of the two fetuses attains its full development, while the other is more or less stunted. The latter are called parasites, because they depend for nutrition upon the body to which they are attached. In either type the parasite may be variously developed. Thus, it may be well formed but diminutive or it may be imperfectly formed, consisting only of part of the body, a single extremity, or even of nothing more than a confused mass of embryonic or other tissues. In short, whenever the development of the parasitic fetus falls below a certain grade, it is represented only by tumour-like formations, the so-called *teratomata*, which consist of a great variety of different tissues, chaotically arranged, and in various stages of development. Such malformations represent abortive parasitic fetuses; the attempt to form a double monster has failed through one of the twins developing at the expense of the other (p. 122 *et seq.*).

A certain degree of independence or individuality is the essential characteristic of every neoplasm, which in ultimate analysis must be regarded as the product of a more or less abortive attempt of certain cells to reproduce a new individual by agamogenesis. There is then some truth in the remarkable saying of Paracelsus: "That in such a disease a man is himself and another; he has two bodies at one time, inclosed the one in the other, and yet he is one man" (p. 130).

The third fundamental proposition underlying Dr. Beard's tumour hypothesis is that there is specific distinction between *somatic* and *germ* cells, which is, of course, the basal proposition of Weismannism.

The very remarkable manner in which the majority of modern biologists were at once captured by this fascinating intellectual divagation, is one of the features in the history of the scientific life of the period; it is likely to rank with the celebrated heresy of Liebig as to the chemical nature of fermentation which retarded the advent of the germ theory for such a lengthy period; and with the vagaries of Cuvier which caused his hypothesis as to the origin of species to be preferred to the more truthful conceptions of Lamarck, and so retarded the progress of modern biology for more than half a century.

My own belief is that the biologists never would have been captured wholesale by Weismann's hair-splitting trivialities if they had not been far too much immersed in, and befogged by, the details and technicalities of their special studies. A certain detachment, and perhaps also a certain intellectual superiority, which are rarely concomitant with a too exclusive devotion to details and technicalities, seem to be absolutely necessary for the adequate development of correct biological generalisation; and it is a characteristic of the present order of things that these special qualities are only too often conspicuous by their absence. A thorough acquaintance with the works of Spencer (physiological units), Darwin (pan-genesis), and Haeckel (perigenesis of the plastidule), would have preserved the biological profession from the pitfalls of Weismannism. I will not enter on the discussion of Weismannism *versus* Spencerism; it must suffice for me to state that I regard the fundamental distinction between somatic and germ cells on which Weismann's theory is based as artificial. I believe that the reproductive properties manifested by somatic and germ cells are the same in kind and that they differ only in degree. In the controversy between Weismann and De Vries I believe the latter, as to his main contentions, was generally in the right. The very fact that even Weismann's followers now recognise that the germ cells are not concentrated solely in the sex organs but are more or less widely diffused throughout the tissues of the soma clearly indicates the near approach of the dissolution of Weismannism and the return to more reasonable conceptions as to the nature of variation, heredity, and allied biological problems.—I am, Sirs, yours faithfully,

Clifton, Bristol, Oct. 31st, 1904.

W. ROGER WILLIAMS.

THE DURATION OF UTERO-GESTATION.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of August 27th, 1904, p. 597, in a review of Peterson and Haines's "Textbook of Legal Medicine and Toxicology," there is a reference to the length of utero-gestation. The occurrence of a case just completed where this question is raised leads me to cite it.

A woman, aged 28 years, a primipara, came in February, 1904, to engage me for her lying-in in August, giving as the date the first week approximately. She was married in October, and the last menstruation occurred on Nov. 2nd, ceasing on the 4th. Accepting this date, August 9th was decided on as due date. She complained of morning sickness as being severe and in a day or two I had to attend her for hyperemesis gravidarum, which only yielded to free evacuation of the bowels by hydrargyri subchloridum in repeated small doses and ingluvin. But for