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THE NATURE

OF SO-CALLED

“PARASITES” OF THE SKIN.

BY

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## THE NATURE OF SO-CALLED "PARASITES" OF THE SKIN.

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IN the *British and Foreign Medico-Chirurgical Review* for January 1864, is an elaborate article by Mr. Erasmus Wilson, *On the Phytopathology of the Skin and Nosophytodermata, the so-called Parasitic Affections of the Skin*, in which an attempt is made to overthrow the almost universally accepted view of modern authorities, in regard to the nature of "parasitic" bodies. The vast influence which the name of Mr. Wilson exercises in dermatological matters, must alone guarantee an acceptance of the doctrines put forth in the essay referred to, by those not well versed in the special literature of skin-diseases. Now, of late years, several questions relating to parasitic diseases have been centres of the most opposite opinions. Are the "parasites" the result of spontaneous generation? Are they produced by a transformation of the molecules of a higher into a lower state of life? Are they vegetable in any degree in their nature? Are they the *vera causa* of disease, or only secondary products? In my work on *Parasitic Diseases*, an attempt has been made to satisfactorily determine most of these queries, but no discussion is contained therein touching the exact nature of so-called parasites, because almost every authority admits absolutely their *vegetable* nature. The essay of Mr. Wilson necessitates a detailed examination of the question; for, indeed, if the more prevalent view be erroneous, and that of Mr. Wilson correct, the whole of our pathology and treatment is bad in the extreme.

Speaking generally, the article is a representation of the views of MM. Robin, Bazin, and Hardy. The original matter relates more particularly to the origin

and nature of the parasitic germs; there is scarcely more than a passing notice of the question pressing now-a-days most strongly for solution: viz., the *identity* or *relation* of parasites, a part of the subject which contains the most telling arguments *against* the doctrine advanced by Mr. Wilson.

The present remarks will be directed especially to the examination of the opinion advanced by Mr. Wilson, touching the nature of the so-called parasites. I may first call attention to the very elaborate and complicated nomenclature introduced into the article, forming a total sufficient to bewilder and reduce to despair not only the student but the more learned practitioner. The following may be quoted:—Phytopathology; Nosophytopodermata; Phytiform; Porrigophyton; Aphthophyton; Mentagrophyton; Porrigo tonsurans; Porrigo decalvans; Alopecia areata; Phytodermic; Chromatogenesis; Epidermophyton; Mucedinales; Dermophyton; Mycodermis; Trichosis furfuracea tonsurans; Tinea pelada; Tinea mentagrophytes; Furfuræ dermatophyticæ; Lichen annulatus serpiginosus; Herpes circinatus pustulosus; Herpes esthiomenos; Pelada decalvans; Tinea achromatosa; Tinea pelada simplex; Area alopeciata; Trichosis arca ophiasis; Dyschromatogenous; Furfuræ parasiticæ; Dermopathology; Trichosis maxillaris; Trichosis corporis seu lichen annulatus; Phyticides.

It is a matter of no denial, that these "hard names" are very obstructive to the advance of knowledge. Take one general term: be it porrigo or tinea, and then particularise each variety by appropriate additions: Tinea favosa; Tinea tonsurans; Tinea decalvans; Tinea circinata; Tinea sycosis; Tinea versicolor; and the result will be materially the better, in that we may reject, *in toto*, all sources of confusion in nomenclature. Why cannot we use the term Parasitic Diseases instead of Nosophytopodermata? Indeed, if the latter be not produced by "vegetable growths" then is the term Nosophytopodermata in every way incorrect. It is much more desirable to employ names which are "easy to read and write," and which "convey quite as correct a notion of the characters of the disease," even "in preference to the more lofty-sounding" ones.

It is a fair criticism, to say that the article "Phy-



topathology" in the *British and Foreign Medico-Chirurgical Review*, does not deal with the subject in a liberal manner. The arguments contained in it, cannot be fairly said to be a "total" of *pros* and *cons*; indeed, the data which form the basis upon which the vegetable nature of parasites rests, are not refuted—not examined. True, the history of their discovery, their outward resemblances, and their microscopic appearances are discussed; but the arguments derivable from analogy, the vital and chemical behaviour of the "parasites," the effect of treatment and the like, are not noticed. Mr. Wilson details very clearly and forcibly the mode in which he conceives the parasitic bodies to be produced by a "granular degeneration" of normal tissues, and refers for corroboration to the instances of the development of "mucus and pus" to be noticed presently; but it seems to me that the whole point has been examined by an anatomical eye and anatomical ideas, without any reference to the *botanical* view of the case. "More than twenty years since," says Mr. Wilson, "after a careful microscopic investigation of the pathological elements of which they are composed, we came to the conclusion, that they resulted from an aberration of cell-formation; that this aberration consisted in the growth and proliferation of the primary granules of which epidermic cells and hair-cells are normally constituted; that this growth and proliferation had the effect of arresting the granules at their embryonic stage, and in the performance of their embryonic functions, and, as a consequence, that the cell-tissue (epidermis and hair) composed of these embryonic granules and imperfectly elaborated cells, was, upon desiccation by the air, dry, spongy, friable and brittle; that, in trichosis, this change alone existed in the hair and epidermis; whereas, in favus, the granules composing the yellow disk around the mouth of the follicles passed through a pustular stage, the consequence of a higher degree of inflammation, and, in their pustular condition, obtained their peculiar yellow colour. We called this morbid alteration of the primary granules of the epidermis and hair-tissue 'granular degeneration', and subsequent experience has not altered our opinion."

As regards the mode of increase and production: "the granules are nucleated, separate or in groups,

or adherent in moniliform strings of two to four, or five in length. The greater number of the granules are uninuclear; some, however, are binuclear or trinuclear. The binuclear granule is in process of proliferation; the original nucleus has split into two, each moiety has become a separate nucleus, the space between them has increased, a septum has arisen in the interval, and the cell has become oblong—an oblong cell with two nuclei, or rather two independent but adherent cells; cells that may subsequently divide and become separate unities. This is the mode of cell-proliferation." At an early stage, and in near contact with the vital tissues, the tendency is to the formation of granules; but, "removed from the source of nutrition proliferation weakens, the divided cells remain adherent, and either retain their globular and embryonic shape, and give rise to moniliform filaments, or they grow in length at the expense of their contents, and form cylindrical and transparent filaments divided by septa, each intermodal segment representing a single original globular granule or cell." They lapse, in the latter case, "into an irregular and filamentary proliferation, giving rise to an inferior form of organisation neither so complete nor so permanent, and which converts the higher animal organisation into a lower and phytiform organisation—an animal tissue into one which might be likened to a vegetable tissue." These quotations give us as the source of the so-called fungus-elements, the normal tissues (epidermic granules); the nature of the disease pathologically as a "granular degeneration;" the mode of increase by the division of the nuclei of the cells, and the secondary division of the cell itself; the formation of so-called mycelium by the division of the nuclei, but the non-separation of the binucleated cell into separate unities. These stages are best seen in favus. The granules are globular, uninucleated, bi- and trinucleated; the mycelial forms are moniliform and opaque, cylindrical and transparent, with a branched and ramified filament. "In the case of the trinuclear cell a bifurcation is effected, and the proliferation of each part of the tissue-cell lays the foundation of a branched and phytiform growth." The peculiarity of favus "is due to a pustular complication;" the layer of the favous mass next the basement membrane of the derma is



made up of pus-globules ; "these pus-globules contain from four to seven or eight well-formed nuclei ; and these nuclei, on their escape from the cell-membrane of the pus-globule, become the nucleated granules which are the chief constituent of the pathological product." The moniliform thread is produced by the coalescence of these escaped nuclei. We have, then, a further proposition to notice ; that in favus the "phytiform" elements are derived from the nuclei of pus-cells. Mr. Wilson interprets the term "granular degeneration," by "the idea of an arrest of development of the cell-tissue of the epidermis at its embryonic stage, and the production of a tissue constructed of crude and imperfect materials, which represent an earlier period of cell-generation than that which Nature intended, and which, in consequence, is truly in a state of degeneration from the perfect type."

Now, it is impossible to overrate the immense importance of the opinions contained in the quotations given ; for, seeing that they differ in *almost every particular* from the opinions of our great dermatologists and botanists, a considerable check must be given to the advancement of the study of Parasitic Diseases, until the conflicting opinions of high authorities are harmonised, or the one or the other is shewn to be untenable. The whole matter rests upon the solution of two questions. The one is, Are the so-called parasites vegetable in their nature ? The other is, Can an animal by a retrograde metamorphosis ever become converted into vegetable tissue ? Then comes the subsidiary question : What are the effects of the growth of parasites ?

First, then : What are the grounds upon which the opinion of the vegetable nature of parasites rests ? Speaking generally, we find that the existence of vegetable growths is very general in the hard external covering of living beings ; and not only this, but there is a complete similarity between the unicellular plants found upon man, and those found on animals. They have been observed, especially by Kölliker, in sponges, foraminifera, corals, bivalves, brachiopods, gastropods, annelids, cirrhipeds, fish, etc. A very familiar instance is that of the silkworm and caterpillar disease of parasitic nature.

We should be led, then, to *expect* the occurrence of

vegetable parasites upon the outer covering of man; and the attributes possessed by so-called parasites are sufficiently distinctive of vegetable bodies: they are, briefly, *structural*, *chemical*, and *vital*.

*a. Structural.* We are able to distinguish the presence of cellulose externally, and internally the primordial utricle coloured by iodine; also the *tubed* mycelial form and fructification; the latter being unrepresented in animal structures.

*b. Chemical.* Ether, chloroform, and spirit of wine render epithelial tissues transparent, and dissolve all fatty substances; whilst vegetable parasites remain unchanged. Ammonia dissolves animal products, *ex. gr.*, pus, puriform secretion, "crusts, etc., converting them into a gelatinous mass"; and caustic potash behaves in like manner.

*c. Vital.* These are by far the most important to notice here. Something may be learnt from a consideration of the localities in which parasites grow, as follows; on the skin generally; in depressions of the derma (follicles); on the epithelial tissues, in the case of the Madura foot or podelkoma; a wound is often followed by the development in it of fungus elements; in the stomach, intestines (hence in the stools); in the œsophagus, vagina, uterus, pleuræ, cavities in the lungs, urine, the wax of the ear, etc. Now, in the vast majority of cases, it is clear that the external air has free access to those situations in which fungi are wont to occur. The exceptional cases admit of very considerable doubt. For example, in the interior of the egg a fungus has been found, but has been supposed to obtain an entrance before the formation of the shell, when there can be no difficulty in accounting for its presence through the medium of the external air. The fluid of the ventricles of the brain, the kidney, and the bladder, have been stated as places where the elements of a fungus may spring up without communication with the exterior. The evidence, however, is open to grave negation. It is quite possible that the development, rapid as it has been, has taken place after the removal from the body or the exposure to the air of the fluids in which it is found. The most notable case of the kind is recorded by Dr. Begbie in the *Edinburgh Medical Journal* for April 1856, in which sarcinæ were found in fresh urine, immediately after micturition, upon two occa-



sions. Can air enter the bladder from without? Another explanation has been offered, by supposing the "germs" of the entophyte to be taken up by the blood, and deposited in the kidney or bladder, as the case may be; reasoning from analogy, it appears most likely that the "germs" are derived *ab externo* in some way as yet unexplained. At any rate, we may reasonably conclude that, in the great majority of cases, there is the greatest facility for the introduction of parasitic elements from without; and that the occurrence of parasites in certain internal parts of the body directly militates against the theory of Mr. Wilson, inasmuch as their presence is unaccompanied by any change of tissue, especially such as would lead us to imagine anything like "granular degeneration" of the epithelial linings. To argue in this case from a narrow limit of observation, *ex. gr.*, the parasitic tinea, must inevitably lead to error and misinterpretation.

The argument which determines the whole question at once is the fact that parasites (or Mr. Wilson's "granular degeneration") will grow when removed from the influence of living tissues, and away from the influence of the blood—a property which no one will accord to animal tissue. The most casual experiment will convince any one of the truth of this assertion. If a piece of favous matter, a hair from tonsurans or "herpes circinatus", be "put up" and kept in a warm place, in a short time the *growth* of the fungus will be noticed; not so that of the animal tissues present. The stromal (nuclear) will develop into the sporular form; the sporules will increase in size, join together, bud, or produce mycelium. I have seen in the trichophyton a most luxuriant crop of mycelial threads sprout away from a diseased hair, while the sporules in the interior became chained and moniliform, while not a particle of fungus could be found in any other part of the field, to suggest an origin from any other source. These artificial germinations are not always successful, and require care in their manipulation; in them we see another very important argument militating against the "granular" theory, and confirming in strong terms the vegetable nature of parasites—viz., the pathological tendencies. After a time, these artificial preparations exhibit a great contrast. The spores become chained, double-

contoured, large, forming a mass of slightly squared cells, chained and linked in all directions, mostly in the axis of the least pressure—viz., longitudinally, or rather parallel to the long axis of the hair; mycelial threads sprout away from the side of the latter, and now and again attempts at fructification are observed; but, inasmuch as the influence of the external air is withdrawn, it cannot be perfected. In contrast to this state of growth is the fatty degeneration of the animal structures—hair, epithelial cells, and the like. I remember to have shown some of my friends a most charming specimen of the kind in perfect preservation (a hair loaded with the trichophyton tonsurans), in which these changes were as marked as possible; the fibres of the hair had degenerated, and their place was supplied by beautiful tufts of crystallised fatty matter; while the fungus itself was freely growing and encroaching upon the space occupied by the hair-fibres. Originally, the shaft of the hair was perfect in its structure, only freely infiltrated with sporules. A prettier object for the polariscope could not be imagined.

Now, if the parasite were a lower form of animal life, a degeneration of the normal structures, surely it should first of all undergo that kind of degeneration so characteristic and cardinal in, as they may be called, dead or dying tissues of the animal body; whereas, in the case in point, we find a positive divergence, a development of the vegetable and dissolution of the animal tissue. It avails nothing to argue here, that there is a conversion of the one into the other; because, leaving out of view the different kind of metamorphosis going on, there are no transitional stages; and the perfectly formed structure of the hair disintegrates by a chemical process, its vital life being wholly null and void; and, according to Mr. Wilson's theory, the "granular degeneration" is a vital process, of course. The only vital process present is the growth of the parasitic vegetation. Hence there is a further point to notice. The change in the animal structures is a *chemical*, that in the parasitic a *vital* one in addition—a distinctive difference, which needs no further consideration. The chemical destruction of the animal, no doubt, helps the development of the vegetable; but the one is not produced by a transformation or modification of the molecules of the

other. The same kind of independent existence of parasites is seen in those cases in which the shaft of the hair is retained in the follicle, while its root is totally disconnected with the formative process. The hair is in this case, to all intents and purposes, a dead structure, yet the parasitic vegetation luxuriates; and other cases might be quoted. The existence of budding is another feature of distinctive significance as to the vegetable nature of the parasites. This will be referred to again.

The occurrence of an union of cells as an active process may be mentioned. This is a fact which every one will admit. Hebra, Mr. Wilson, and others seem to recognise, as the universal mode of the production of sporules, the constriction of filaments, and separation of the constricted portions, as well as the bidivision of free cells. This is surely an error of observation. The sporules are primary in point of existence, therefore cannot be formed by division of the mycelial cells. Besides, the larger are seen to be produced by the enlargement of the smaller cells. In the case of the torula, this is seen to an extreme degree in its endogenous growth. Saving in the fructification, which is rare in human epiphytes, the most usual process is an increase by endogenous formation, and also the formation of the mycelium by a union of cells (with superadded budding); and in the mycelium the same endogenous growth obtains. Now, nothing of this complete kind exists in the epithelial tissues: endogenous growth, yes, but not a true junction and union of cells. I refer now to the epithelial tissues only, which are those concerned in the discussion.

Parasitic disease can undoubtedly be produced by the introduction of the germs from without, provided there exist a suitable soil. The experiments of various observers will occur to the memory of most readers. In accordance with all that is known of fungi generally, it must be accepted as certain that vegetable growths arise in the case of "porrigo", of favus, and such like, "as they do if a piece of melon or a bit of cheese, not over dry, be placed in a cupboard"; that is to say, in accordance with the same general laws; or, in other words, they never flourish upon "healthy" surfaces, though it is difficult to define clearly the exact state of pabulum by external manifestations;

but that some favourable condition of soil, some deviation of the nutritive process, exists, must be admitted. Well, then, introduce the sporules of a fungus to a person so nutritioned, and parasitic disease will result. Does not the contagious character of tinea make a dead stand against the acceptance by us of the theory of "granular degeneration"? Mr. Wilson states that, in a certain sense, "mucus may be viewed as a *parasite* receiving nutriment from the body, but not shape, nor claim to vitality"; and adds, in a foot-note: "In this sense, we are willing to accord to the nosophytodermata the title which has been assumed for them of parasitic diseases, but not as organisms originating from without, and intruding upon the tissues of man, as the phytopathologists claim." Yet he does not notice the bearings, upon this question, of artificial inoculation and contagion. It has been pretty surely shown that, in the instance of Madura foot, which presents a larger amount of fungus than any other disease, the germs are derived through wounds in the feet; and it may not be uninteresting to observe here, that Mr. Wilson's theory could not explain the development of the chionyphe *Carteri* in the carious bones of the foot. Looking for a moment to the applicability of the word parasite to the case of mucus (pus), these latter have most special and useful functions to perform, without which nature would suffer very materially. The term parasite, as given to them, leads one to form by far too low an estimate of their nature and worth, and detracts from the beauty and perfectness of conservative nutrition.

The effects of treatment are worthy of mention.

In the tinea, if the parasite be destroyed, a cure is effected without the use of any internal remedies. It is so in herpes circinatus, in many cases of tinea tonsurans, and in pityriasis versicolor, for instance. Depilation acts in the same way by the removal of the parasite in part, and more especially by enabling us to reach the fungous elements more easily with parasiticides. Mr. Wilson admits that depilation "disposes to the cure of the local disease"; and adds: "But we do and must disbelieve that the removal of the fungus is the proper explanation of the cure." It is partly by removal, and partly by affording facility of access to the fungus; that is the true *modus operandi* of the operation. "We see,"

says Mr. Wilson, "in depilation a stimulant, and a valuable stimulant," etc.; and "we have been led to regard avulsion as one of the best, indeed, our only reliable remedy, for producing *deep stimulation of the cutaneous tissues*, for setting up a new action in the papillæ of the hair; as the only remedy, in fact, by which we can reach successfully the fundus of the hair-follicle." "It sets up a new action, a healthful inflammatory process, in place of a morbid inflammatory process—a process whose natural course is to end in resolution, instead of one whose nature is perpetuity. For morbid cell-germs, it establishes healthy and plastic inflammation, and sometimes healthy suppuration; when, as Bazin declares, the fungus is starved or drowned." Now, in the very worst forms of the disease, the hairs are perfectly loose, and can be even readily combed out; hence avulsion can do nothing here by stimulation, only by removing the sporules. Indeed, if there be a point of practical importance in the treatment of parasitic disease, it is the removal of all "dark stubs", which are quite loose, but which are literally crammed with "germs" of disease. How the mere removal of a hair from its follicle can set up a healthy process, and alter *in toto* the particular form of nutrition upon which depends the morbid cell-genesis, is a problem which would be indeed difficult to prove; it is curious pathology. The depilation is followed by a reparative action, by which the hair is reproduced in integrity, upon the same principle that all local injuries are remedied. "Even Bazin has recourse to avulsion as a stimulant in old cases of mentagra, wherein the hair acts the part of a foreign body, of a thorn, in the skin, and so keeps up irritation". This is an admission that the hair acts as an *irritant*. But a healthy hair does not do so; consequently, the removal of the hair is effected not with the view of *stimulation*. In addition, it creates a free outlet for the retained contents of the follicle. Stimulation is best promoted by anything which increases the supply of blood (in harmony with proper nutrition) to the follicle and its papilla; at least, so teaches pathology. In ophthalmia tarsi, the extraction of the hairs acts in a great degree in like manner, by giving a free outlet for the contents of the inflamed follicles. Depilation, *plus* the use of parasitocides, is the most efficacious plan of treatment; for



the least amount of fungus left behind will itself overcome the good effects of avulsion. These considerations are very relevant to the question under consideration.

At page 202 of Mr. Wilson's essay, we read: "We have remarked, that the chief difference between favus and the other forms of phytodermic disease is due to a pustular complication. In fact, we have observed that the layer of the favus-cup, which lies in contact with the basement-membrane, is composed of pus-globules." This is not consonant with general observation; it is rather the exception, according to most observation.

At page 215, we read: "Suppuration of the follicles performs the part of a natural cure of the disease. Bazin emphatically observes, that pus destroys the parasitic fungus; and therefore, in seeking to find it when suppuration has commenced, we must avoid those follicles and those hairs which are bathed with pus." So far, then, there is an evident contradiction. Pus is the great destroyer, and yet the great producer, of the parasitic growth ("granular degeneration"); to avoid which, Mr. Wilson adds: "That kind of inflammation which is productive of normal pus is not the form of inflammation which causes granular degeneration of the cell-tissues of the skin, but, on the contrary, is opposed to it." I dare call this argument pure assumption. My examinations have been carefully made, and have not resulted in determining the existence of the presence of pus and a luxuriating state of fungus; nay, the reverse. Nor does it occur to me to adduce, as a matter of fairness, any fact in support of the existence of a specificity in the attendant inflammation. An abnormal state of nutrition exists, which is seen by the presence in the follicle, in favus especially, of a secretion "possessing a fluid or semifluid state when first formed, and hardening into a kind of paste, which subsequently becomes dry and pulverulent," etc. This is not pus; it is observed in the convalescence from favus, when the true favus-matter has disappeared, and is typified by the "secondary eruptions" which follow the fully developed disease. It is an evidence of the existence of a special pabulum or soil; and, be it observed, does not give origin to the "granules" or parasites, which arise from out their

own proper germs. Pus, then, is often absent; and there is no ground for believing in the existence of a specific kind of inflammation, but in a state of nutrition which, *per se*, does not give origin to parasites. But there is another consideration. It has been clearly shown that there is *great similitude* between the spores of favus (supposed by Mr. Wilson to arise from pus-nuclei) and those of the trichophyton and even oidium and torula, if there be not an identity.

“We can only say that we attach very little weight to the opinion of any man who has the temerity to pretend to establish a diagnosis between favus and trichosis tonsurans, on the more than doubtful differential characters of the achorion and trichophyton, as viewed by the microscope,” says Mr. Wilson, in words which happily confirm the opinion advocated by myself in my work on *Parasitic Disease*, and which certainly imply that the “granular” bodies in the two cases have a common origin. How, then, come favus from pus, and the trichophyton from the epidermic granules?

The colour of favus has been attributed to the appropriation by that disease of the colouring principle of pus. Well, it may be. But, if the colour of parasitic vegetations (granular degenerations) be derived from the tissues, how are we to explain the red, golden, and yellow red aspect of the *zooglæa capillorum* discovered by Dr. Aloys Martin (*Med.-Chir. Review*, April 1863) upon the heads of two children ill with typhus? Such is the name given to a parasite which was found attacking the hairs after the usual method of parasitic growth. The colour of favus is, then, no very great argument in favour of the origin of the plant from pus.

It will be well to consider, in the next place, some subsidiary conditions, including also the manner in which proliferation takes place, according to my own observations. And, first, it appears to me that the trinuclear and binuclear character of the spore is not an established fact. It is not uncommon to find a nucleus and granular contents in the cells or spores; and the arrangement may be bi- or tri-nuclear, as in torula; but this cannot be established as the rule in the parasitic fungi, except in trichophyton, achorion, and tri-sporuloides. In microsporion Audouini, nuclei

are mostly invisible, and granules are even indistinct. This much is true, that whenever the nuclei are *double*, the cell has a *tendency* to assume the oblong form. The junction and growth of cells produces mycelium, and in the latter cellules are formed by endogenous growth. The sporules do not increase in number, as a general rule, by the process of budding, but by the discharge of cell-contents and the enlargement and growth of the discharged granules (or nucleoli). Dr. Bennett's doctrine appears to me to be the true one, that not division of filaments and cells, but *molecular growth*, is the true developmental process. On the contrary, Dr. Beale, in the controversy upon this point (BRITISH MEDICAL JOURNAL for 1863), asserts that cells grow until they reach a certain size, and then divide. But, as Dr. Bennett says, if the molecules divide, "how is it that the filaments can form at all?" Mr. Rainey's observations are confirmatory of Dr. Bennett's view, which is also shown to be applicable to the early formation of the ovum. The best example of the production of sporules by simple molecular growth is seen in the enlargement of the stromal (or nuclear) into the perfect form of fungus. It has never happened to me to see the division of a trinuclear cell, so as to produce a branched mycelial filament, but only a process of sprouting on a limited scale.

"Favus matter and the mucedinales of phyto-dermata are organic matter arrested in development at the lowest degree of life, the function of reproduction; the sporules are growing organic substance, aborted epidermic granules, the filamentary portion fully formed organic substance, beyond which there is no further growth, the highest and perfected form of development"—are the concluding words of Mr. Wilson (pp. 204-5) upon this part of the subject. It is strange that, in the animal body, we have no analogical instances to prove this view. Reference is made cursorily to the case of mucus and pus; but these two latter differ in every essential particular from the parasites, structurally, chemically, vitally. The increase of pus may take place by "cell-proliferation"; but parasites increase by endogenous growth and union of molecules (mycelium). Pus is a common product; but it never is seen to be "transformed"

into a parasite, except supposedly in favus, where it is often absent.

In the last place, it is absolutely needful to scrutinise the question of the conversion of animal into vegetable tissues. Physiologists instruct us largely upon the laws of growth and reproduction; but deducible from the former is one which is second in importance to none, and it is the *law of limitation*. It is of wide and clear significance; by it tissues preserve their type, though they may deviate to a certain degree in this respect. The law of limitation, it appears to me, forbids us to entertain the conversion of which mention was made a moment since. Let us see, first of all, what Mr. Wilson's opinions are at p. 202 (*loc. cit.*). He says, after their escape, "the nuclei of the pus-globule are adherent to each other; so as to constitute the moniliform strings, etc.; and already, on the first birth of the granules, they are impressed with the type which subsequent development converts into a phytiform tissue": at p. 201, lines 22 and 23, "and which converts the higher animal organisation into a lower and phytiform organisation—an animal tissue into one which might be likened to a vegetable tissue": at line 32, "until we are led to the conclusion that we have before us no longer an animal organism, but a vegetable organism of the fungoid or mucedinous type": at line 45, "a combination which, out of the body and in the free air, would be regarded as a fungoid plant."

Dr. Willshire, some time ago, examined this question; and concluded that "low vegetable organisms are the result, and not the cause, of the maladies they are seen in connection with; and that their development is readily explainable according to well known laws ruling the so-called decomposition of organised matter."

Mr. Wilson, it would appear, thinks that the so-called sporules are altered animal tissues; but, as far as can be learnt, though he calls them phytiform tissue, he does not regard them as of vegetable nature. Then it only remains to examine Dr. Willshire's views, detailed in the *Medical Times* for 1848-49, vol. xix; which remain, I believe, with very little modification, the same in this year 1864. The presence of the oidium in thrush is admitted. Berg, Grüby, and others, taught that this was the sole cause of the

disease. "Thrush" (so these writers say) "is the springing up and propagating of a parasitic fungus, the fine threads and fibres of which insert themselves in every direction amid the superior layer of the epithelial scales"; and (according to Gruby especially) even passing into the cells themselves. According to Berg, this generation of the fungoid parasite overwhelms, as it were, the mucous membrane; and is due to a reaction between the albumen and the acid in the mouth, the acid generated by the results of bad feeding, or gastric or other derangement, influencing the buccal secretion. He maintains, also, that local disorder of the buccal lining membrane itself is not to be recognised as the necessary, or even the frequent, antecedent of the true parasitic thrush disorder.

This view was soon refuted by clinical observation, which proved incontestably that "the presence of thrush deposit is preceded by symptoms, not only of a general and constitutional disorder, but of local disease of the mucous lining of the mouth . . . the plastic deposit the result of a peculiar kind of membranous inflammation . . . the presence of vegetable parasite, as perfectly irrelevant to the true or only essential pathology of the disorder; and consider its development as readily explainable according to well known laws ruling the so-called decomposition of organised matter." (Willshire).

With a little patience and care, the truth must have been established; the decided but erroneous views of Berg and others were, however, followed by a strong reaction; and pathologists framed a code of laws ruling the decomposition of organised bodies, which, in turn, required modification; at present little cared for, because the subject has fallen into utter neglect. What are the laws, then, *quoad* thrush, by which parasites are governed?

Dr. Willshire says: "Whenever or wherever organic matter, or the cells of vegetable or animal substance previously endowed with a special form of life, are passing into what we term a state of decomposition; a certain amount of the ultimate cells yield up their endowment of vitality to the overwhelming laws of pure chemistry and physics; their chemical elements separating and then re-combining according to these laws, and forming purely chemical compounds subser-



vient to laws of a like nature. But other cells do not surrender up their vitality to any such influences of chemistry and physics; but preserve their great endowment of the spirit of life, and take on afresh a new kind of organic existence, differing from that expressed in the totality of such organism, of which they previously formed a part, or into the formation of whose existence they had before entered. . . . In the production of the fungus, or mould, or new organism, we observe the preservation of the vitality of certain cells, and their continued exemplification of this vitality, in the assumption of a new type of organised existence."

Here we have, then, the opinion of many moderns, that an animal may degenerate into a vegetable tissue; a doctrine which the theory of Mr. Wilson entirely upholds; affirming, as it does, the production of "granular degeneration" (shown to be vegetable in nature, *i. e.*, parasite) from an animal tissue. This extreme doctrine is a gross libel upon the laws of nature; it involves, at the outset, a total abnegation of the existence of a law of limitation; it presents a difficulty, in so far as it asserts that some cells in the decomposition undergo a chemico-physical change, and lose their vitality; but that others do not lose their vitality, and are not made subservient to chemico-physical laws. Yet the same influences and co-existences play upon the two classes in equal force and extent. How can we explain the difference? We know that the air is full of germs of the thrush and other fungi; that these have an existence independent of the body; that they grow upon vegetable structures, etc.; and that it is unnecessary to appeal to any such theory as the conversion of animal into vegetable for their production.

The experiments of M. Pasteur pretty clearly show by negative evidence, notwithstanding the recent experiments of Dr. Wyman, which are open to grave objection (as the reviewer in the *British and Foreign Medico-Chirurgical Review* for January 1864, p. 171, observes), that the origin of germs of low life is the air. We have had a view expressed in one, and a great reaction in the other direction; from which have resulted two absolutely conflicting and contrasting doctrines, and, as in all other instances of the kind, we shall find the truth to be between the two ex-



tremes, and to this present pathology is rapidly tending.

Having regard to the law of limitation, as it obtains generally; to the vegetable nature of the parasites; the differential attributes of animal and vegetable; the effects of inoculation (the latter now succeeding, now failing); the experiments of various observers, showing that nutrition is lowered in its standard in cases of parasitic disease, both in man, animals, and vegetables; the experiments of Stilling on frogs; the free existence of the germs in the air; the existence of fungi generally in the hard structures of animals; the independent life of the parasite; the adaptation of certain soils to certain vegetations; the effects of treatment (a combination of local and general remedies succeeding best in tinea); the distinct evidence of the existence of a state of local malnutrition before the appearance of any fungus—*ex.* thrush, potato-disease, and the like—it appears certain that, in all cases of parasitic complication, two conditions are necessary: the presence of a suitable soil, which may or may not be appreciable to the naked or microscopic eye; and the implantation thereon of the germs of a parasite derived *ab externo*. These conditions may vary much in degree; the parasite may take hold upon the surface, and the soil may be too little developed to nourish it; hence its death and disappearance. This explanation holds good equally in tinea, in thrush, in the instances of fungi found in the cavities of tuberculous lungs, the stools of cholera, or the shreddy discharges of muco-enteritis, as in the ordinary “mouldy jam” or oïdial vine disease. The doctrine that the tissue of animals can retrograde into vegetable structures like that of spontaneous generation, shocks my mind and offends my reason.

In the case of the caterpillar, the itch in animals, the vine or potato disease, the uredinous disease of cereals, the thrush of infants, the favus of children, it has been incontestably shown that this double action is at work. It is a matter of very decided importance, as concerning the therapeutics of disease. We may treat the disease—favus—locally, and cure it. We may even treat analogous cases generally and cure them, if we can exert a profound influence upon the existing state of nutrition; but the combined

plan of remedial action will save us an innensity of trouble and bring us greater *kudos*.

But then, limiting our observations to those forms of parasitic disease, which the physician is called upon to treat, if there be a peculiar malnutrition present, and a growing parasite, which is the more important to notice? Shall we regard *tinea favosa*, for example, more deserving the name of malnutrition or of parasitic? We may put it in another way. What is the action of a parasite? Does it exert any special influence?

We leave out of consideration the tendency it has to accelerate "fermentative" action"; and, on careful inquiry, we notice that it has some special action of its own over and above that exerted by the state of soil, and that is, to destroy the formation of hair, and to disintegrate already formed hair. The disease of the hair (and epithelium) is that pathognomonic of a growing fungus; this has been shown in my work on *Parasitic Diseases* (p. 28). General disease, whereby the whole vitality and nourishment of the body is lowered, may be followed by loss of hair generally—atrophy, in fact; but this differs in every essential particular from the active localised *destruction* in parasitic disease.

No disease, except it be accompanied by the growth of a fungus, can produce splitting up of the hairs, atrophy of the bulb, and alopecia. In parasitic disease, other structures may be involved, but not necessarily. Eruption is oftentimes present, but may be evoked by the action of any *irritant*; whereas nothing but the growth of a fungus can produce the damaged hairs, etc., of *tinea*. Take away the latter, and nothing *diagnostic* of the ravages of the parasite remains. *Per contra*, remove all the *eruptive*, and the parasitic remains in its integrity; the *necessaries* remain, but the *accidentals* are absent. The presence of the fungus increases the local irritation. There is nothing special in that; so do other irritants; but it is not unlikely that the force exerted in its growth and development, which we know belongs in an eminent extent to fungi, may be an additional degree.

At first sight, the behaviour of entophytes (parasites of the mucous surface) appears to contradict that of epiphytes (cutaneous parasites). Remark certainly

greatly erred when he wrote, "there is a remarkable difference between the pathogenetic relations of the achorion and the fungi growing in the mouth, and probably also in aphthæ," etc. There is the existence of a suitable soil in each case, the presence of a parasite in a state of growth, and certain effects produced by the latter. In the case of aphthæ, it amounts to local irritation only; in the disease in which achorion occurs, hairs and hair-follicles exist and become attacked so as to produce peculiar results, in addition to mere local irritation; it is a difference, not of nature, but of extent and degree in the two cases—a difference due to the character of the structure attacked, which alters, to some extent, the plan of treatment in the two cases. In aphthæ, it is the alteration of the soil which demands attention with the destruction of the fungus; or, in other words, the removal of an efficient cause of irritation. We do not destroy it with the view of curing the disease; not at all. In favus (achorion disease), we alter the soil. We destroy the parasite with the view of removing a cause of local irritation; but more especially because vast damage and destruction will happen to the hair and hair-follicles unless this be effected. This cannot happen in thrush. There is no contradiction in pathological tendencies; surely not. If it arise in our minds, it must be from a want of correct appreciation; there is perfect harmony in principle, in law, and in result, as far as it goes.

These remarks will at once recall to memory, that parasites have been divided into those which are *true* and those which are *false* (represented, generally speaking, by epiphytes and entophytes), a distinction very important in a practical point of view; but suggestive of a difference in the pathological tendency of the two cases, which cannot be maintained in strict argument. The false parasites would inevitably give rise to disease and destruction of hairs, etc., if the latter existed on the mucous surfaces; but, this not being the case, the growth of entophytes (or false parasites) is comparatively harmless, still the contrast of true and false is by no means inappropriate in a *therapeutical* point of view.

Reference has been freely made to the essay of Mr. Wilson; for it contains doctrines which are diametrically opposed to the more usually accepted view—

the view which guides and directs all our treatment; and upon which is based the most efficacious of all plans of treatment, that by local means. There should be no doubt in the matter; it is one of two things: either we are absolutely wrong or right—absolutely wrong, if the so-called parasites be not vegetable in their nature, or if they be produced by a transformation of animal structures. That neither of these two hypotheses is tenable, it has been the aim of these remarks to show. The view advanced by Mr. Wilson is similar to, indeed a resuscitation of, Unger's idea, broached in 1823, that the uredo (*a parasite*) was nothing more than “broken-up cells, a disrupted and altered condition of certain portions of diseased plants”; an idea which was refuted by the fact of artificial germination.

P.S. I might have gone into the question of the artificial germination of parasites, but this would have opened up too wide a field for the present discussion. The power of parasites (achorion, for instance) to induce fermentation, and the production of fructification typical of ordinary forms of *mould*, may be mentioned as additional arguments in favour of the vegetable nature of parasites. I contented myself with insisting upon their independent vitality. Before concluding, one cannot but express regret that, in the recent number of the *British and Foreign Medico-Chirurgical Review* (April), in a notice of the recent works on skin diseases, this most novel and interesting subject of the relationship of parasites has been almost entirely *depreciated*: the reviewer says, “that, after all, the question is one which concerns the botanist more than the medical man; be the fungi one or many it is the same thing to the practitioner,” and the reason given is that the same drug will destroy each fungus with equal facility; but the reviewer states, also, that to reach the parasite is often “a matter of considerable difficulty,” tacitly admitting of course the benefit of internal remedies. Now, if the fungi be *different*, there must be *different* soils, and the treatment must be *different—in kind*. If the fungi be identical in nature, then the treatment is a matter of *degree* only; if fungi be different, then ought they to be so many indices to states of economy, states which in time will be fully interpreted. I have





no doubt, for example, that when the exact nature of sarcina shall be determined, then we shall possess a tolerable clue to the character of the diseased conditions (the state of stomach derangement) which favour its development,—on the road thereto at any rate,—and the relations and kind of fungus will presently become of inestimable value as guides to treatment. As a writer in the *Popular Science Review* states, in speaking of the variation of fungi, it is of “greatest importance, and throws an entirely new light upon the diagnosis of skin diseases;” and botanists generally share freely in this opinion. No reference is made in the article in question to entophytes, nor to the extremely interesting disease the fungus foot of India, in which the chionyphe Carteri is found, of which full notice is taken in one of the works reviewed, and which is attracting a large share of attentive observation; zooglœa capillorum is omitted, too; and, amongst animal parasites, the ehigoe, the œstrus, the filaria, etc.; when, at the same time, Dr. Anderson and I are brought to book for not ranking molluscum contagiosum as parasitic, because especially “Hardy has discovered the spores of a cryptogamous plant in the sebaceous contents of the shining semi-lueid tumours which characterise this affection.” But fungi have been also found in lepra, psoriasis, eczema, etc., and these are not looked upon as parasitic, simply upon the ground that fungi have been observed in them. It must be shown that the parasite in molluscum is not an accidental phenomenon by clinical and experimental evidence, and as yet this has not been done; and an *à priori* argument of much moment, is the fact that parasites are not wont to produce sebaceous disease, nay, sebaceous matter is by no means congenial to their growth.

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