

showing a less mortality in one particular epidemic of fever than in some other epidemic. The results really show nothing more than that in certain forms of fever blood-letting is less injurious than it is in others—for example, in relapsing or inflammatory than in typhus fever.

I ask you, then, are you ready to maintain that the practice of our forefathers, in this matter of bleeding in fevers, was a proper practice? I may surely answer, No. And if such be the answer, then what follows is this: their practice was wrong; and all conclusions founded on the assumption of the propriety of such practice must be ill founded, and therefore the conclusion that diseases have changed their type.

I might apply the same kind of reasoning to the cases of tubercular and other adynamic diseases, and ask if the mere fact that blood-letting was a common remedy with our forefathers in those diseases is any proof of the propriety of its use? and whether, therefore, we can fairly draw any reasonable conclusions as to a change of type in diseases from the mere fact that we do not resort to blood-letting now in their treatment, as our fathers did? Does not modern pathology inevitably show that blood-letting neither is nor ever could have been a right remedy for the cure of such tubercular diseases?

[To be continued.]

Illustrations

OF

HOSPITAL PRACTICE:

METROPOLITAN AND PROVINCIAL.

ROYAL PORTSMOUTH, PORTSEA, AND
GOSPORT HOSPITAL.
LOCK WARD, No. II.

CASE OF PHAGEDENIC ULCERATION: USE OF
MERCURY.

Under the care of K. E. KNIGHT, Esq.

M. M., aged 20, was admitted November 3rd, 1863, with a sloughing ulcer over the right trochanter; a deep ulcer three inches in diameter, over the right nates; an immense sloughing ulcer of the calf of the right leg, the fleshy portions of the gastrocnemius and soleus being eaten away, and the deep popliteal fascia laid bare. The patient was obliged to lie on her face. She was ordered to have bark and ammonia wine, and liberal diet; opium at night; and a linseed poultice to the sores. Under this treatment, the sloughing was arrested. She was ordered to take five grains of blue pill and half a grain of powdered opium night and morning; and to have a lotion of dilute nitric acid applied to the painful part.

As soon as the mercury affected the system, the effect was magical. The ulcers assumed a healthy aspect, and healed rapidly. The patient left the hospital at the end of two months, cured. Practical men can draw their own conclusions from this case.

EPITHELIAL CANCER OF PENIS AND SCROTUM;
REMOVAL: RECOVERY.

Under the care of F. PAGE, M.D.

T. D., a pensioner, aged 63 years, was admitted February 2nd, 1863, with epithelial cancer of the penis, involving a considerable portion of the scrotum, of

two years' duration. The whole surface of the parts was ulcerated; and the urethra was destroyed from the orifice to the pubes. The inguinal glands were healthy.

Dr. Page operated as follows. Commencing below, he removed all the anterior portion of the scrotum, laying bare the testes, and amputated the penis close to the pubes. The hæmorrhage was inconsiderable. The patient recovered without a single bad symptom; and at the end of twelve months he appears in robust health, and the parts operated on in a perfectly sound condition.

The result of this operation would go to show that epithelial cancer of the genitals offers a better chance of cure by removal than when the disease is situated in any other part of the body.

Original Communications.

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

By W. TILBURY FOX, M.D. Lond.; University Medical Scholar; Physician to the Farringdon General Dispensary.

[Concluded from page 312.]

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 *zooglea 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 sporules; 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 microsporon 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 phytodermata 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 analogous 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-prolifera-

tion"; 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, Gruby, 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 subservient 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 chemo-physical change, and lose their vitality; but that others do not lose their vitality, and are not made subservient to chemo-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 extremes, 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 oidal 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 corals, 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 immensity 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). Remak 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 are 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.

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