Illustrations OF HOSPITAL PRACTICE: METROPOLITAN AND PROVINCIAL.

WESTMINSTER HOSPITAL.

CALCULUS IMPACTED IN THE URETHRA OF A CHILD:
ENORMOUS DISTENSION OF BLADDER:
REMOVAL: RECOVERY.

Under the care of CHRISTOPHER HEATH, Esq.

[Reported by Mr. WATTS, House-Surgeon.]

FEBRUARY 5th, 1864. Richard P., aged 3½ years, was sent to Mr. Heath by a practitioner a few miles from town, suffering from retention of urine. For three weeks his mother had noticed that he had complained of pain about the bladder and penis, particularly when passing urine. On the 2nd (three days ago), she found that he could not pass his urine at all; and since that time he had been unable to do so, but a small quantity had constantly dribbled from him. He had been constantly crying and pulling his penis; and on the day before admission, she took him to a practitioner, who only ordered a powder; and afterwards, to the medical man who sent her to Mr. Heath.

On admission, the lower part of the abdomen was distended by the full bladder, which formed a distinct prominence as high as the umbilicus. There was some edema of the scrotum, but no appearance of extravasation of urine. Mr. Heath, who was in the hospital when the child arrived, had him put under chloroform, and then examined the urethra externally to see if there was an impacted calculus, but failed to find one. He then proceeded to pass a catheter, which struck against a calculus immediately within the meatus, which was concealed by a congenital phimosis. Mr. Heath therefore at once performed circumcision; and on exposing the glans penis, the calculus was seen slightly projecting from the meatus, from which it was easily extracted. It proved to be an oval calculus, broken at one end, and weighing four grains. The outer coat was uric acid; but at the broken end could be seen an inner layer of phosphates; and probably there was an inner nucleus of oxalate of lime, as is frequently the case in these small calculi of renal origin. A catheter was passed, and a very large quantity of urine evacuated.

February 7th. The boy was quite comfortable; but appeared unable to hold his urine, large quantities of which had been passed under him.

February 9th. The thread was removed from the foreskin, which was healing. The incontinence of urine continued.

February 11th. The boy was up to-day, and able to hold his urine. Nitrate of silver lotion was applied to the foreskin.

February 23rd. He was discharged. The boy has quite recovered the control of his bladder, and the foreskin is all but healed.

In some clinical remarks upon this case, Mr. Heath explained to the pupils that retention of urine in children was almost always associated with calculus, and that the early recognition and treatment of the case was most essential. In this boy, the calculus had passed through the whole length of the urethra, and was only obstructed by the narrow meatus, from which it was readily extracted; but calculi not unfrequently became impacted in other parts of the urethra, and if permitted to remain there, gave rise

to fatal extravasation of urine. Fortunately, this boy was seen in time to prevent such an accident; but the bladder was in a dangerous state of over-distension.

The treatment of this case was simple enough; but had the stone become fixed further back, it would have been necessary to open the urethra at that point. Mr. Heath said that he agreed with Mr. Cooper Forster, in believing that there was no more danger in opening the urethra in front of the scrotum than behind it, if the calculus were impacted in the anterior part.

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.

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; Nosophytodermata; Phytiform; Porrigophyton; Aphthophyton; Mentagrophyton; Porrigo tonsurans; Porrigo decalvans; Alopecia areata; Phytodermic; Chromatogene-

sis; Epidermophyton; Mucedinales; Dermophyton; Mycodermis; Trichosis furfuracea tonsurans; Tinea pelada; Tinea mentagrophytes; Furfura dermatophytica; Lichen annulatus serpiginosus; Herpes circinatus pustulosus; Herpes esthiomenos; Pelada decalvans; Tinea achromatosa; Tinea pelada simplex; Area alopeciata; Trichosis area ophiasis; Dyschromatogenous; Furfura parasitica; 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 Nosophytodermata? Indeed, if the latter be not produced by "vegetable growths" then is the term Nosophytodermata 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 "Phytopathology" 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 inter-medial 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 fungous 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 pododermatitis; a wound is often followed by the development in it of fungus elements; in the stomach, intestines (hence in the stools); in the oesophagus, vagina, uterus, pleura, 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 sarcinae were found in fresh urine, immediately after micturition, upon two occasions. 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 unaccom-

panied 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.

[To be continued.]

RECENT INTUSSUSCEPTION.

By E. COUSINS, Esq., F.R.C.S.

ON December 13, 1862, a male child, aged 21 months (the same patient whose case in a former illness is reported in the BRITISH MEDICAL JOURNAL for June 21, 1862), came again under my care. The child was said to have had diarrhoea for the last day or two, supposed to be connected with teething. At 1½ A.M., the nurse gave it two teaspoonfuls of castor oil, which acted before 3; giving a not unhealthy-looking liquid evacuation, on which oil floated. The child, however, strained much in passing it, and seemed faint. He then had frequent paroxysms of pain and slight tenesmus, during which he writhed much, and then fainted, remaining pale and more or less unconscious till fresh torminia appeared to rouse him. He occasionally vomited a few teaspoonfuls of serous fluid with greenish flakes. At 3½ A.M., he passed about half an ounce of bright blood *per anum*, with tenesmus, and then fainted (as usual after each fit of pain). At 4 o'clock, I found him in a state of syncope, with the pulse barely perceptible, even during the fits of restlessness. These occurred every ten minutes; in the intervals between them, the abdomen was soft and flaccid, and free from tenderness or from any discoverable tumour; and the right iliac region and hypochondrium were sensibly full; the epigastric, umbilical, and left iliac regions, were depressed; the space from the umbilicus to the pubes was moderately full. Whilst palpating, the transverse colon was felt to harden under the hand, and formed a well-defined hard tumour, which could be traced, though less hard, through the region of the ascending colon to the cæcum. Evident distress began after this; the rectus muscle hardened so as to obscure the tumour; and the child writhed severely. Laudanum was given, with a little sal volatile. The fit of pain passed off; and during the subsequent syncope the tumour again became perceptible, and then gradually became less and less defined, and nearly ceased at last to be distinguishable, until the approach of the next paroxysm.

The diagnosis was, intussusception of the ileum through the ilio-cæcal valve, reaching half-way along the tract of the colon. Some brandy being given, inflation *per anum* was performed with the stomach-pump at seven o'clock. The abdomen was rendered very tense with air, which in about three minutes was heard suddenly to escape with a gurgle into the small intestine; and on examination, the tumour was found to have disappeared, while the small bowels were discoverable as an elastic mass surrounded by the easily traced colon. It was interesting to note the remarkable change in the feel of this organ, during an ordinary peristaltic contraction of it which followed in a few minutes after the operation. The pulse recovered its tone and fullness instantly afterwards, and the cheeks regained some colour. The child seemed to rest for about a quarter of an hour, and then showed indications of returning playfulness and interest in those around him. He was ordered rice or barley-water with cream as an exclusive diet.

1 P.M. He had had a quiet sleep; after which he craved for food; then had a bloody stool.

8 P.M. He had had two evacuations; the last was