A TRULY VIRTUOUS WILL IS ALMOST OMNIPOTENT.

EDITED BY
MILTON M. ANTONY, M. D.,
PROFESSOR OF OBSTETRICS, &c., IN THE MEDICAL COLLEGE OF GEORGIA.

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ARTICLE I.

Remarks on Fever, more especially that form of it usually denominated Yellow Fever. By J. B. Whitridge, A. M. M. D., President of the Medical Society, and ex-officio, of the Medical College of South Carolina; in reply to certain interrogatories by Chervin, D. M. P., a celebrated French Philosopher and Physician.

The subject was thoroughly investigated by Dr. Chervin* for whom the following epistolary essay was prepared, and by him deposited in the Archives of the Royal Academy of Paris, for the use of the French Government.

Although the essay has been some time written, the experience which it embodies and which is confirmed by subsequent observation, may perhaps be useful, and the remarks which it contains, not uninteresting to some of the readers of the Southern Medical and Surgical Journal.

* See Pétition addressée a la Chambre Des Députés Par N. Chervin membre titulaire de l' Académie Royale de Medicine.
INTERROGATORIES.

"Have you ever seen any case of yellow fever which originated from contagion?

"Do you believe this disease under any circumstances, to be contagious?

"What do you consider to be the most common causes of yellow fever?

"Have you seen any person attacked with the yellow fever a second time?

"Do you believe the human frame liable to be attacked with this disease more than once?"

To Doctor Chevin—

Sir:—Fever is a unit—I have long been of opinion that there are various forms and modifications of fever, or in the language of the illustrious Rush, that there are various symptoms and states of fever, but, that there is in fact, but one fever. Hence the nosological arrangement of fevers, according to their various forms and modifications, is calculated to bewilder the mind and to mislead the unwary.

There are three forms of fever, viz: intermittent, remittent and continued: these are subject to various modifications, according to circumstances, and constitute what is commonly called tertian, quartan, quotidian, &c. The remittent and continued forms of fever, have also their characteristic symptoms, but which, it is foreign to my present purpose to detail.

There are regular gradations of fever, from the simple intermittent, up to the highest grade of bilious or yellow fever. The latter is not confined to, but is more frequently found in, tropical climates. In the climate of Carolina and Georgia, the human constitution at some period of life, from infancy to decrepitude, is liable to every grade of fever. And these take place, according to the greater or less susceptibility of the subject, or according to the greater or less intensity of the predisposing and exciting causes. Persons accustomed to warm climates, or to the atmosphere of those places which are surcharged with the mephitic gasses, which are principally instrumental in the pro-
duction of the higher grades of fever, are less susceptible than strangers. Persons from high northern latitudes, are more susceptible than those within the tropics. In this climate, Europeans are more susceptible than Americans, and persons from the northern section of the United States are more susceptible than Carolinians. Strangers are much more susceptible than natives, or those who have been long resident. Native children, are more susceptible than native adults.

That modification of fever usually demonstrated yellow fever, is what now claims our more particular attention, and this is generally of the continuous form.

The yellow fever is not a disease sui generis. This then being conceded, there are no diagnostic symptoms by which (in popular language) the yellow fever can be distinguished from other fevers. It can only be known by a strict observance of its symptoms.

The term yellow fever is objectionable, in as much as it does not express that particular modification of fever, which is usually described under that name, nor is a yellowness of the rate mucosum a symptom which invariably attends this modification of fever. It depends in a great measure on the mode of treatment pursued. Probably in seven-eights of the cases which occur, if left to the operation of nature, this symptom would manifest itself; but if an active and vigorous mode of treatment, upon the mercurial plan, were pursued, in seven-eights of the cases, it would be prevented. The same may be said of the black vomit, which is generally considered a diagnostic symptom of yellow fever.

The nosological arrangement of this modification of fever, under the name of typhus icterodes, is also objectionable, in as much as the word typhus, according to the Greek original, implies a disease of low action. If it admitted of any classification, it might with more propriety be arranged under the genus synochus of Dr. Cullen, in as much, as it is a disease of a mixed character, and in the southern section of the United States, generally partakes of the nature of those modifications of fever, which he terms synocha and typhus. As the term yellow fever is familiar to all, and as the symptoms which characterize this modification of fever, are generally well understood, for the sake
of perspicuity, I shall adopt the term, in the subsequent remarks, according to its usual acceptation. The limits prescribed to these remarks, will not admit of my giving a description of the disease, nor is it necessary, as it has been so often and so well described by medical writers.

This being premised, I proceed to answer your interrogations, in as cursory a manner as possible, and

**First.**—Have you ever seen any case of yellow fever which originated from contagion?

I answer, from the experience of several years practice in the city of Charleston, where the yellow fever has frequently prevailed, no evidence has ever been presented to my mind, to produce a conviction, that the disease is of a contagious nature.—I have often seen persons predisposed to fever, under all circumstances of exposure to the yellow fever,—breathing the deoxigenated atmosphere of close apartments, both day and night,—often in contact with the sick, the dying, and the dead, in the performance of the sad offices of duty or of friendship,—and this too without the least indisposition. To those subjects who are predisposed to the disease, breathing the impure air of a sick room, watching, fatigue and anxiety of mind, especially when conjoined with the debilitating effects of fear,—these may, and no doubt often have proved, the exciting causes of yellow fever. This combination of circumstances, will, in some measure, account for the erroneous opinions of those, who suppose the yellow fever contagious. To those who have any experience, the real character of the yellow fever is too clearly developed, ever to be mistaken;—and to those who have not, (if they are open to conviction, and will reason fairly and candidly on the subject,) the facts and arguments before them are incontrovertible.

**Second.**—Do you believe the yellow fever under any circumstances to be contagious?

From the foregoing statement, I certainly cannot believe it to be so under any circumstances; because, if it were contagious, it would be a different modification of fever, or in other words, it would not be yellow fever, but some other fever.

Such, however, is the vague and uncertain use of language, especially in regard to the use of the terms *contagion* and *infection,*—and these, though words of very different signification,
are so often confounded; in order that I may not be misunderstood, it will be necessary for me here to introduce a few remarks for the purpose of explaining what I understand to be the true import and meaning of these terms, respectively. And this I will do by the adoption of the words of a friend,—an accomplished and elegant writer,—in preference to the employment of language of my own upon this long mooted and vexed question, which I trust, your laborious investigations, your indefatigable exertions, will ere long settle forever.

Is it not remarkable, that the word *contagion*, a word of so frequent use in professional writings, and in books of science, should never have acquired a definite or precise meaning; that, instead of being stamped with technical precision, as its importance demands, it should so long have been suffered to pass, as a fluctuating and uncertain currency of common language?

From the indiscriminate use, the words *contagion* and *infection*, *contagions* and *infections*, as applied to diseases, would seem to be synonymous or convertible terms; yet no respectable philologist, I think, will admit that they really are so. It is true, we cannot resort to etymology, in all instances, for the best authorized acceptance of words; because long use, or, perhaps it may be said, abuse, often renders that arbitrary. The particular use of words and terms, acquires, in this way, a sort of secondary, and indeed, an absolute legitimacy. But where this has not been fully attained, where words continue to be used in different senses, by different writers, and even by the same writers, at different times, it would seem desirable to limit their technical use, to accord, as nearly as possible, with their radical meaning. But to give technical precision to a term, it will often be found necessary to restrain its meaning, within limits, somewhat arbitrary. Thus, the word *contagion*, in an unlimited sense, might mean whatever, by coming in contact with the body, is capable of producing disease. And as there are probably few diseases which are not produced, either directly or indirectly, by some noxious principle, foreign to the body itself, and applied either to its external or internal surfaces, it would follow that, in this loose sense, a large proportion of our diseases might be said to be contagious. A somewhat similar remark would apply to the word *infection*. Without technical
limitation, it would perhaps, be nearly synonymous with contagion; except that the former would convey the idea of a poison mingled with, or attached to, or imbuing some other body. And as contagion must be supposed to be generally communicated, through the medium of some body which, in common language, is said to be infected with it, (for a body, in this sense, as well as the atmosphere, may be infected with contagion,) it is not difficult to see the origin of the loose and indiscriminate employment of the terms, infections and contagious diseases.

But, it appears to me, and I think it corresponds, in general, with the usage of the most correct modern writers, that, to give precision to medical language, the word contagion should be applied to such poisons only, as originate in a diseased animal secretion; and are capable of reproducing themselves by exciting, in a sound body, the same specific action, in which they themselves originated; such poisons as were, originally, by Mr. Hunter, and subsequently by Dr. Adams, denominated morbid poisons. A contagious disease, then, would be such only, as is produced by exposure to those morbid emanations, which are generated by a specific vascular action, in one labouring under a like disease.

"On the other hand, the term infection should be applied, I think, to such poisons as are produced by the changes taking place about inanimate matter, either animal or vegetable, during their decomposition. And the circumstances, which seem most to favour this process, are heat, moisture, and confinement; though the latter may have no further influence than to give effective intensity to the poison. Infection, then, differs from contagion, in this, that although the former may occasionally arise from animal secretions, such as the matter of sensible, or even insensible perspiration, or from the accumulated filth of numerous individuals, crowded into small and uncleanly apartments, yet there is not, as in the production of contagion, necessarily a morbid vascular action. Infection may be constituted by deleterious miasma or poisonous emanations from the surface of the earth, in certain situations; or it may arise from foul matter, at various depths beneath the surface, decomposing under the influence of long continued and intense heat, and thus, more or less extensively, impregnate or infest the atmosphere. So, it
may sometimes be generated locally, as even in an empty hold of a ship, or about the persons and beds of the sick on shore, or in filthy apartments occupied by the sick; yet healthy individuals, from abroad, who sicken by being exposed to such infection, do not necessarily sicken of the same disease as that about which it originated. And of the individuals exposed to it, probably but an inconsiderable proportion will suffer from its influence; the relative number depending on the condition of body, and other circumstances, in which the persons exposed may happen to be, at the time. The same individual, therefore, will contract an infectious disease, by an exposure at one time, which at another, precisely similar in all circumstances external to himself, he had escaped with impunity. Not so with contagion, its source is uniform and certain; of whatever kind it is, its origin is invariably a specific morbid secretion; and of those coming within its active sphere, rarely one escapes its specific effect; this too, scarcely influenced by circumstances of health, habit of body, temperature, or climate. It may be remarked, further, that in comparing infectious diseases, all of which are acute in their nature, with that class of contagions, which are also acute, the former appear at irregular and uncertain periods, after exposure to their cause; whereas the latter are, in this respect, uniform and certain."

Under this view of the subject, as well as from my own observation and experience, I do not believe the yellow fever under any circumstances to be contagious.

Third.—'What do you consider to be the most common causes of yellow fever?'

In answering this question, I shall divide the causes into two kinds, viz: remote and exciting, and briefly enumerate the principal. Under the first head, I shall include the predisposing, and to the last, I shall add the proximate.

A combination of causes generally conspire to produce yellow fever, especially that aggravated form which sometimes becomes epidemical;—these may be divided into two kinds, viz: general and local. I do not mean to be understood to say, that local causes alone are not sometimes sufficient for the production of yellow fever; but I do say, that a combination of both general and local causes produces a more aggravated form of disease
than either separately, and this combination will generally, perhaps always, be found to exist in large cities and towns, whenever the yellow fever prevails as an epidemick.

The influence of the latter is limited, but the former spreads far and wide.

Local causes may produce sporadic cases, and sometimes endemic diseases; general causes may assist in the production of these, but a combination of both, or of the latter only, are necessary for the production of epidemical diseases.

These observations are strengthened, by the fact, that whenever the yellow fever makes its appearance in any of the large towns and cities in the United States, (and the remark is particularly applicable to the city of Charleston,) it usually commences in the low, crowded, and filthy parts of the city, or wherever the local causes act with the greatest force.

REMOTE AND PREDISPOSING CAUSES.

1st.—*Vegetable and Animal Miasmata.*

Either of these, in a concentrated form, is sometimes sufficient for the production of yellow fever; but when combined, they produce a higher grade of fever, or a disease attended with more violent symptoms.

2d.—*Excessive Moisture.*

This is manifest from the well known fact, derived from the history of the diseases of Charleston, for the last half century, that in very wet and rainy seasons, the yellow fever has been more prevalent than in any others.

3d.—*Excessive Dryness.*

It has been observed also, that in very dry seasons, the yellow fever has prevailed much more, than in those seasons attended with a due degree of moisture.

4th.—*Cold.*

This may sometimes operate, both as a predisposing and exciting cause, according to the manner of its application.

5th.—*Heat.*

This may also operate, both as a predisposing and exciting cause, according to the manner in which it is applied.

6th.—*Bad regulated Police.*
7th.—The prevalence of particular winds.

As for instance, the prevalence of easterly winds, is one among many causes which conspired to produce that aggravated form of disease, with which the inhabitants of the city of Savannah were scourged, during the season of 1819. This general cause, operated, by bringing into action a greater portion of the noxious effluvia from the swamps of Carolina and Georgia, than could have been effected, by the prevalence of any other wind. On the contrary, for the same reason, a general prevalence of westerly winds, besides being hotter, are more mischievous in their consequences, and more productive of evil to the citizens of Charleston, than any others. To us in the city of Charleston, easterly winds in the summer and autumnal months, are not only a great luxury, but a general prevalence of them, is almost a sure guarantee of a healthy season.

8th.—The debilitating passions of fear, grief and despair.
9th.—All excessive evacuations.

The two last may also sometimes act as exciting causes.

10th.—A constitution unaccustomed to the climate, or to the remote and predisposing causes, &c. &c.

EXCITING CAUSES.

1st.—Excessive stimuli of every kind, such as intemperance in eating and drinking, &c.

2d.—Cold.

This is a very frequent exciting cause, especially at night,—it operates by producing a sudden check, either of the sensible or insensible perspiration, or both. The yellow fever has been twice excited in my own system by this cause.

3d.—Heat.

I have known several cases produced by exercise, under the influence of the intense heat of the noon-day sun.

4th.—Unusual labor or exercise.

5th.—Violent emotions and stimulating passions of the mind, &c.
A morbid excitement of the liver and stomach, producing a morbid, increased secretion of those organs, and a sympathetic action in remote parts of the system.

This, I think, may be supported by numerous facts and arguments—especially those derived from dissection, but which my present limits preclude me from advancing. I shall, therefore, proceed to your fourth and fifth questions, which may both be comprehended under one head.

Fourth.—‘Have you seen any person attacked with the yellow fever a second time?’

‘Do you believe the human frame liable to be attacked with this disease more than once?’

In reply to these questions, I have only to observe, that, I myself have twice had the yellow fever, to-wit: in 1817 and 1819. On both occasions, I suffered severely, under the calamities of this formidable disease, and to the energetic powers of medicine, alone, sub providentia, I owe my life. My sufferings on the latter occasion, were to me a convincing argument, of the susceptibility of the human constitution, to receive the disease “more than once.” And further, the idiosyncrasy which had been by four years residence in the climate of South Carolina, (to which in 1815, I was a total stranger,) and by once having the disease, could not have been changed, because, I had not slept a single night out of the city of Charleston, during the two years which elapsed between my first and second attack.

Several cases have come under my observation, of persons having the disease twice. Captain Boyle, late of the United States Army, told me he had had the yellow fever several years successively, in the city of New-Orleans; and I have no doubt of the susceptibility of the human constitution to repeated attacks of this disease. But it must be admitted, that by a long residence in a climate or place subject to the yellow fever, and especially by the disease itself, the constitution is rendered less susceptible of a second or third attack, than of the first. It requires, therefore, a greater combination of causes, or the same causes concentrated, so as to act with greater intensity, to produce the disease in a constitution assimilated to the
Having briefly answered your interrogations, I will now conclude my observations, which I cannot do without expressing the high sense I entertain of the noble object of your pursuit, and the pleasure it would afford me, could I contribute more to the advancement of science and of truth.

Too much honor and too much praise, cannot be conferred on that man, who so nobly hazards life, and expends fortune, in the acquisition of truth,—who undauntedly exposes himself to all the dangers arising from an exposure to the remote and predisposing causes of the highest grade of fever,—who enters the abode of the wretched and the distressed, from city to city, in order to seek out the occult causes, and to ascertain the true nature and character of yellow fever.

It is a pursuit important to the commercial, to the moral, and to the scientific world.

It is a pursuit, in every respect worthy of the attention, the zeal, and the indefatigable industry, of the philanthropist and the philosopher.

That your life may be long preserved, and that you may be enabled incontrovertibly to establish the true nature and character of the yellow fever, is the sincere wish of,

Dear Sir,

Your obedient.

And very humble servant,

[Signed]

J. B. WHITRIDGE.

Charleston, S. C.
ARTICLE II.

An Essay, read before the Medical Society of Augusta, on the question, "What are the Characteristic or Diagnostic Symptoms of Cancer, whether in the state of Schirrus or of open Ulceration?" By PAUL F. EVE, M. D., Prof. of Surgery in the Medical College of Georgia.

The importance of the above question, propounded to me at the last meeting of the society, will readily be acknowledged by every one at-all familiar with the various and very different morbid appearances, which have been confounded with schirrus and cancerous ulceration. It is confessedly no easy task, to determine how carcinomatous affections are to be distinguished from all other diseases. Even with the light thrown upon the subject by the recent investigations in pathological anatomy, and which has effected so much in the diagnosis of our diseases in general, still a dark and heavy cloud obscures the one now under consideration.

In order to answer definitely the question before us, I propose a thorough examination of the different symptoms of cancer, as recorded by the most distinguished writers on the subject; for it is alone by comparison that we can arrive at an enlightened conclusion. And that our judgement may be correct, and the views of no author misrepresented, I shall not hesitate when convenient, to employ the language of each, to whom reference may be made. I do this the more willingly, as the opinion of foreigners will be quoted to illucidate the subject of carcinoma.

It is proposed in the first place, to examine the symptoms of cancer, whether in the state of schirrus or of open ulceration, in connection with the pathological anatomy of these two conditions of the disease; and then to consider what is its peculiar, characteristic nature.

Cancer is a Latin word and derived from the Greek carcinos, signifying a crab, and is so called according to most authors from the distended or varicose veins existing over a schirrus tumour, which the ancients compared to the claws of this animal;
but some writers suppose it was employed on account of the disease extending by whitish bands into the surrounding soft parts, which resemble the feet of the crab. Without stopping to enquire into the etymological derivation of cancer, or of even questioning the propriety of the expression as applied to disease, I proceed at once to its symptoms; employing it or carcinoma as a generic term, and intending by schirrus and cancerous ulceration, the two very distinct varieties of this affection. So great indeed are the differences in the appearance of this disease in its two states, that it is necessary to consider each separately. In one we have a tumour, and in the other an ulcer.

In the classification of tumours, Laennec has very properly admitted two distinct orders of accidental or adventitious productions. In the first class, are embraced those which have an analogy to the natural tissues of the body; and in the second, abnormal productions which have none. It is among these latter tumours that schirri are placed.

A schirrus tumour is generally considered the forerunner of open or ulcerated cancer. By the author of the article on this disease in the Dictionary of Practical Medicine and Surgery, published in Paris, 1830, schirrus is described as being composed sometimes of a perfectly white substance, at other times a little blueish or grayish, slightly transparent, of such a consistency that ordinarily when cut it grates, and which varies from the fat of pork to a hardness approaching that of cartilage. Commonly homogeneous, this matter appears to be divided in masses, which are subdivided into lobules, united by compact cellular tissue of a very variable form according to Laennec, but sometimes of a regularity like that of the honey comb. Many schirri moreover, have a great resemblance to the substance of the turnip, others to that of the chestnut.

Boyer, both in his Surgery and in his article on the subject, in the 52d vol. of the Dictionnaire des Sciences Medicales, says, a schirrus is a hard, moveable, circumscribed, resisting tumour, ordinarily indolent or a little painful on pressure. Its almost constant termination is in cancer.

Abernethy in the account of various tumours in his Surgical and Physiological Works, states that schirrus sometimes condenses the surrounding tissues so as to form a capsule; and
at other times the organ in which it originates seems to be a nidus for the diseased action. In either instance the carcinoma commences in a small spot and extends like rays from a centre. This, says he, is a feature that distinguishes it from other diseases, which generally involve a considerable portion at their first attack. Another distinction is, that schirrus does not recede like other tumours to medicinal treatment. It also destroys the skin before it acquires any great magnitude. Abernethy agreed with Baillie in his pathological anatomy of the tumour; particularly in his discription of the peculiar hardness, and intermixture of the firm, whitish bands, having interposed between them a brownish substance, or cells containing a pulpy matter of various colours and consistence. As a carcinomatous tumour increases, it generally becomes unequal upon its surface; and a lancinating pain is commonly felt, though it is not experienced in every case.

Richter says, it is an inadequate and erroneous discription of schirrus, to call it a hard and painful glandular swelling, having a disposition to become cancerous. He even denied the disease to be regularly attended with swelling. But Sir Charles Bell observes, that though the organ in which the disease originates, (as the mamma for instance,) may diminish, yet this is not true of the tumour itself. The general bulk of the breast may be contracted, still the disease is a tumour; there is an increased mass, a preternatural growth, or new matter formed, corresponding to the old definition, morbosum augmentum.

Samuel Cooper, in his Surgical Dictionary, says, the puckering of the skin, the dull, leaden colour of the integuments, the knotted and uneven feel of the disease, the occasional darting pains in the part, its fixed attachment to the skin above, and muscles beneath and in the breast, the retraction of the nipple, form so striking an assemblage of symptoms, that when they are all present there cannot be the smallest doubt that the tumour is a schirrus. He also states, that without risk of inaccuracy, we may set down the backwardness of a schirrus swelling to be dispersed or diminished, as one of its most confirmed features.

When a section, said Sir Edward Home, is made in a schirrus tumour in its early stage, the centre is more compact, harder to the feel, and has a more uniform texture than the rest of the
swelling, and is nearly of the consistence of cartilage. From this, in every direction like rays, are seen, ligamentous bands of a white colour passing to the circumference, as also others of a fainter appearance in a transverse manner; the whole forming a kind of net work in the meshes of which a softer substance is enclosed. In a more advanced stage of the tumour, the diseased part has a more uniform structure, and no central point can be distinguished. According to Sir C. Bell, it is these ligamentous bands which produce the retraction of the nipple in the breast, by extending between the ducts and destroying its spongy texture.

Sir Astley Cooper says, the swelling of schirrus gradually grows from the size of a marble until it acquires two or three inches in diameter; for it rarely happens that the true schirrus tubercle increases to a very considerable bulk, and this circumstance is one of its criteria.

In the article Cancer, by Bricheteau in the Dictionnaire des Sciences Medicales, the physical characteristics of schirrus are defined to be, a demi-transparent tumour, having a linear disposition, often lobular, of the consistence varying from lard to cartilage or fibro-cartilage, and composed of tissue which seems to be of cellular production penetrated with albumen of a white, blueish or greyish colour.

Dr. Gibson, understands by schirrus a preternatural density or induration of the soft parts, not easily resolved and very prone to ulceration. It is also recognised by certain external marks, and by a peculiar internal structure. The whole tumour is unequal on the surface, and uncommonly heavy; the pain is vehement and of a peculiar kind—at first prurient, but afterwards lancinating and compared by many patients to the gnawing of an animal.

Besides these characteristic symptoms of schirrus, and the peculiar pathological changes in the tissues affected, hydatids are sometimes found existing in great numbers, and of different sizes. This circumstance has probably given rise to the theory of the animalcular origin of carcinoma, which was entertained by the late Dr. Adams, and which is still insisted upon by Mr. Carmichael of Dublin.

Mr. Pearson declares he has never yet met with an une-
quivocal proof of a primary schirrus in an absorbent gland.—

He thinks it always commences in the secretory glands, as the mamma, the pancreas, testicles, &c. Should this idea be confirmed by experience, it would assist much in the diagnosis of the disease in the state of schirrus.

By a comparison now of the descriptions given by the distinguished authors referred to, we find but little discrepancy in their enumeration of the symptoms of cancer in the state of schirrus, or of the pathological condition of the tissues affected by it.—All agree there is no one pathognomonic symptom or sign of it; but they also concur in an assemblage of them, which when taken together, clearly and distinctly characterize a schirrous tumour. Thus Bayle and Cayol state that out of a hundred tumours of the breast, all of which are hard, unequal, insensible to pressure, and which have existed for more than a year, about ninety nine are cancerous. Again, if a tumour presenting these signs of schirrus, has resisted a treatment for chronic phlegmasia, for serofulous engorgement, &c. then is it certain that such a disease is cancerous.

With respect to cancer in the ulcerated state, it may be remarked that writers generally coincide in representing it to be an ulcer opening spontaneously, with hard, jagged and reverted edges, of a very disagreeable aspect, from whose surface there flows a fetid, acrid discharge, excoriating the surrounding skin, which becomes of a purple colour, that of the sore itself being of a dark red. The patient complains of darting pain and of a burning sensation over the whole ulcer, and which when examined presents no trace of cellular tissue, nor of vessel, nor of any other normal structure, whatever may be the organ attacked.

Boyer however, expressly states, that notwithstanding the weight, inequality, hardnss, rough irregular shape of the ulceration, the nature of the secretion and the pain, the indiscriminate destruction of all tissues, &c. yet they do not exclusively characterize cancer. It is only positively known by its return when once operated upon; this says he, is the only circumstance which can remove all doubt on the subject of its disease.*

An anecdote in point is told of this old veteran Surgeon; which is, that whenever pressed for a decision concerning the nature of a disease supposed to be cancerous, his uniform reply was, "if it returns after I cut it out, then it is a cancer."
A question has originated of some importance in connection with the characteristic symptoms of this disease, and which it is well to notice at this place; it is whether cancer ought alone to be restricted to schirrus degeneration. Authors generally admit that certain ulcerations may become carcinomatous, without being preceded by the state of distinct tumour. The question too, is I think decided by the admission of two separate classes of cause producing cancer; viz: an internal and an external. For if an external injury or impression can develope a schirrus tumour under the skin when entire, certainly a similar cause can the more readily produce a like effect upon an ulcer. Be this however, as it may, a genuine cancer in the state of open ulceration, is generally preceded by a schirrus degeneration.

As regards now the nature of cancerous disease, it may be stated that we have nothing very satisfactory on this subject. If it be an affection sui generis as it certainly is, this nature is not as yet well defined. It is however, a subject bearing too much upon the diagnosis of cancer, to be omitted, notwithstanding its difficulties, in the present essay.

Cancer is to all intents and purposes a highly malignant disease. None destroys more indiscriminately all the tissues of the body, than it and fungus hematodes, with which it has often been confounded; and they are even to the present day, considered by nearly all French writers, as only a modification of one and the same disease. Cancer, and fungus are placed by them in the same article, and treated as partaking of the same general characteristics; differing only in one being in a state of crudity and the other in the state of ramolissement. But this is certainly a very great error.

Cancer, in its nature, differs essentially from fungus hematodes, in its seat, its progress, the contents of the tumour, and the liability of the period of life to an attack. Cancer most frequently affects the female mamma, uterus, testicle or the secretory glands—fungus, the extremities, internal organs, &c. Cancer is generally chronic in its course—fungus is rapid. Cancer commences with a hard, uneven, knotted tumour, which never acquires any great size before ulcerating; and when opened, the contents are of a cartilaginous durity, even turning the edge of a knife—fungus is a soft, smooth, elastic tumour at its begin-
ning, generally attaining a great magnitude, and when opened, found to contain matter resembling the structure of the brain. Cancer has a central point harder than the circumference of the tumour—fungus has nothing similar to this. Cancer is almost peculiar to old age—fungus to no particular period of life. Cancer, in its progress, contaminates the neighbouring lymphatic glands—fungus never, but affects the internal organs, as the lungs, liver, brain, &c. Two diseases having such differences, cannot be of the same nature.

Cancer differs from gangrene. This latter, as has been said, kills the parts it attacks, or deprives them of their vital properties; while cancer, on the contrary, consumes, devours the tissues still living. Abandoned to itself, cancerous disease never recovers. It often does remain stationary, but when it changes, it is always to increase and never to diminish. These general characters, with those already mentioned, would seem to prove, that cancerous disease is the effect of one and the same cause. In the language of Bayle and Cayol, it remains to be established, what is this cause. Is it black bile, according to Hippocrates—acid black bile, according to Galen—a coagulable lymph become acrid, after Boerhaave—a foetid oil, alkali or acid—or is it a gas, according to some modern writers? The truth is, we are absolutely ignorant what is the cause of cancer.

Boyer supposed that cancer is formed by lymph, arrested in its passage, and in the neighbouring cellular tissue, and that it is the product of an internal constitutional cause. Abernethy said it arose from a disordered state of health, and agreed with Hunter that there is always a predisposition first in the system, before the appearance of the diseased action. Bayle and Cayol declared the disposition to cancer or cancerous diathesis, is the true and only cause why the disease returns after an operation. According to Andral, the fibrine coagulated in blood vessels, constitutes, sometimes in the organs, a whitish mass, resembling tumours called cancerous. Velpeau having had occasion to observe some facts of this kind, concluded that cancer could be developed primitively in the blood. But all this is little satisfactory, and assists us very feebly in answering the question, what is the true nature of cancerous action? Were it true, that cancer depended upon a constitutional diathesis, as these and
other authors, even Sir Astley Cooper, would have us believe, no operation could ever be permanently successful.

While I admit a predisposition to schirrus and cancer, resulting chiefly from old age, still I cannot believe that carcinoma arises in all instances from an internal cause, or that like scrofula, the constitution is primarily affected by it. I must still contend for the local origin of this disease, and that the system is only secondarily invaded by its progress. All that has been well established on this subject, the peculiar nature of cancer, are the chronic induration with hypertrophy of the parts attacked, the difficulty, if not the impossibility, to discuss or resolve such a tumour, its great tendency to malignant ulceration, the destruction of all the tissues indiscriminately of the body, and the occurrence of these circumstances at the age of about fifty years.

Of all diseases, schirrus is most difficult to be distinguished from inflammatory indurations and fibrous tumours. From the first, it has been remarked, that chronic indurations are more or less of a red colour, from the presence of blood invited by irritation, into the part inflamed. They are, moreover, preceded and accompanied by symptoms of inflammation. A fibrous tumour differs from schirrus, in being more isolated from the tissue of the organ affected; in acquiring a large size without ulceration; in its fibres passing in every direction; in the fibrous disorganization being well supplied with blood, bleeding freely when cut into or torn, which is never remarked in schirrus; in never giving pain, producing only inconvenience from its magnitude; finally, in its never degenerating into cancerous ulceration.

The last peculiarity of the disease under consideration, which I shall notice, is its affecting chiefly the white tissues of the body, as the breast, testicle, &c. Now it is this very structure which approaches, in appearance, nearest the schirrus disorganization. Who, for example, does not know that the female mamma, in its healthiest condition, will turn or fracture the edge of the best tempered knife; or who, after a minute examination of its ducts and anatomical arrangement, has not seen some parallel to the whitish bands, &c., of a true carcinomatous tumour?

In conclusion, I define the characteristic or diagnostic symp-
toms of a schirrus to be, at its commencement, a small, hard, moveable, circumscribed, and indolent tumour, ordinarily located in a secretory gland, and occurring most frequently after the forty-fifth year of age. It may remain stationary for years, but cannot be dispersed; generally in about twelve months the tumour increases in size, becomes heavy, unequal or knotty on its surface, forms attachments to the skin and surrounding parts, which are converted to a leaden colour, and the patient will experience darting, lancinating pain through the part affected. So malignant is the disease, that before attaining a diameter of two or three inches, the tumour will have ulcerated. Originating like a local affection, the system soon becomes involved in its progress.

A true schirrus dissected, will present a centre of a cartilaginous hardness, with whitish bands seemingly composed of condensed cellular tissue, proceeding towards the circumference, interposed by others passing in a circular direction, and the whole enclosing a brownish pulpy matter, which exudes or may be scraped from the cut surface.

I define the characteristic or diagnostic symptoms of cancer in the state of open ulceration to be, an ulcer seldom less than two or three inches in diameter; commonly the result of schirrous degeneration; of highly malignant character, destroying every tissue which it invades, and uniformly progressing. The surface of the sore is very uneven, its edges are jagged, very hard, irregular and prominent, the discharge is a foetid, acrid matter, and the pain is acute and lancinating, or described by patients as a burning, gnawing sensation. The neighbouring lymphatic glands are sure to become affected; and the ulceration when removed is very apt to return, especially if it proceeds from an internal constitutional cause.

The anatomico, pathological appearances of such an ulceration are, an indiscriminate disorganization of all the tissues involved by it, and their conversion to an homogeneous, hard, greyish or red, irregular substance.
PART II.

RE VIE W S A N D E X TR A C T S.

A new treatment in a Case of Anchylosis. By J. Rhea Barton, M. D.

In the North American Medical and Surgical Journal for April, 1827, I published an account of a new and successful operation at the hip, which had been undertaken for the twofold purpose of remedying a most serious deformity and lameness, and of establishing an artificial joint, as a substitute for the natural articulation, which had become obliterated by disease, terminating in true anchylosis.

The principles upon which this operation was founded, as well as the circumstances justifying the performance of it, were fully detailed in the publication at that time.

In prosecuting my views for remedying lameness and deformity from the mal-position of limbs in cases of true anchylosis, I have been enabled to present another case successfully treated, under circumstances suggesting a practice of a peculiar character.

In the case of anchylosis at the hip joint, it is to be recollected that the neck of the femur was sawn through, and the distorted limb straightened. The wound of the soft parts was then healed, whilst the reunion of the divided bone was prevented by subjeeting it, from time to time, to motion; such as gentle rotation, flexion, and extension, abduction and adduction. After continuing this treatment for a few weeks, the ends of the bones lost their disposition to unite, became obtundet and smooth, and were held attached to each other by provisional bands or ligaments, and in this manner forming an artificial joint, whose movements were regulated by all the principal muscles by which the original joint had been controlled: thus fulfilling the ends of my operation, and rewarding my patient for his fortitude.*

* The patient, upon whom this operation was performed, enjoyed the use of his artificial joint for six years; during which period he pursued a business (trunk-making) with great industry, earning for himself a comfortable subsistence, and a small annual surplus. Pecuniary losses, however, through the reverses of those in whose hands he had confided his means, sunk him into a state of despondency and desperation, followed by habits of intemperance. This, with all its train of evils, abuse of health, &c. was, no doubt, the cause of the change which afterwards took place in the artificial joint. It gradually became more and more rigid, and finally all motion ceased in the part. With this exception, the benefits of my operation were
In the case now to be described, no attempt was made to establish an artificial joint; as the attending circumstances did not admit of such a consideration. The object of my treatment was to remove deformity, and to restore to usefulness a limb which had unfortunately been suffered to become ankylosed in a mal-position. The following will, I trust, satisfactorily explain the operation and the after treatment of the case, as well as the principles by which I was guided in the management of it.

S—— D——’s, M. D., formerly of Charleston, S. C., but now a resident of Alabama, when a youth of about nine years of age, unluckily had his knee joint involved in inflammation and suppuration so extensively, as to occasion the destruction of the synovial membranes, the ligaments, cartilages, and, in short, every structure peculiarly appertaining to the joint. After a protracted suffering he finally recovered with the loss of the joint; the tibia, femur, and patella having become united to each other in the form of a true ankylosis. The loss of the articulation of knee, however, though a misfortune, did not constitute the sadness of his case. It was caused by the mal-position of the limb; the leg having been flexed upon the thigh to a degree somewhat less than a right angle. Hence the only alternatives of which he could avail himself to aid him in walking were, either to use crutches, or to employ a very high block-sole boot, and to lower his stature by flexing the sound limb in order that both feet might reach the ground. The latter expedient he adopted.—The long continued pressure and weight of the body sustained by this defective limb, acting under such great mechanical disadvantages, had at length caused some projection of the instep, and other irregularities, which it is unnecessary to particularize.

This supposed irremediable condition of his limb, with all its ills, the young gentleman endured during the period of about sixteen years. In the mean time he graduated in medicine, and became a successful and highly respectable practitioner; but as his professional labours increased, he found the condition of his limb to be an obstacle not only to his further success, but also a source of unceasing annoyance and vexation. Whereupon, with...
a resoluteness not surprising to those who knew the strength of his mind, the firmness of his character, and the abundance of his manly courage, he repaired to Philadelphia in order that some relief might be obtained, if it were possible. When consulted by him I found him fully prepared to learn that no benefit was to be expected from any heretofore known practice, and that if he could be relieved it must be by some novel expedient and treatment.

After a candid and full disclosure of my views of his case, and of the means by which I thought he might be benefitted, his own judgment accorded with mine; and believing in the feasibility of the plans, he became urgent for the undertaking. It was accordingly commenced on the 27th day of May, 1835, and pursued as follows:

Two incisions were made over the femur, just above the patella. The first commenced at a point opposite the upper and anterior margin of the external condyle of the femur, and, passing obliquely across the front of the thigh, terminated on the inner side. The second incision commenced also on the outer side, about two and a half inches above the first; and passing likewise obliquely across the thigh, terminated with the other in an acute angle. By these incisions were divided the integuments, the tendon of the extensor muscles of the leg, at its insertion into the upper part of the patella, and some of the contiguous fibres of the rectus and crureus muscles themselves, a greater part of the vastus internus, and a portion of the vastus externus muscles. A flap, composed therefore of this structure, was elevated from the femur close to the condyles. The soft parts were next detached from the outer side of the bone, from the base of the flap towards the ham, by passing a knife over the circumference of it, as to admit of the use of a saw. The flap then being turned aside a triangular or wedge-like piece of the femur was easily removed by means of a small narrow bladed saw; such as was used in the operation at the hip. This wedge of bone did not include the entire diameter of the femur at the point of section; so that a few lines of the posterior portion of the shaft of the bone remained yet undivided. By slightly inclining the leg backward, these yielded, and the solution was complete. This mode of effecting the lesion of the bone was designedly adopted, and constituted what I conceive to be a very important measure in the operation. Important, because it rendered the popliteal artery free from the danger of being wounded by the action of the saw, and subsequently the interlocking of the fractured surfaces tended to retain the extremities of the divided bone in their positions until the harshness of their surfaces had been overcome either by the absorption of their angles, or by the deposition of new matter upon them—a change essential to the
safety of the artery during the subsequent treatment of the case. Not a blood vessel was opened which required either a ligature or compression. The operation, which lasted about five minutes, being thus ended, the reflected flap was restored to its place, the wound lightly dressed, and the patient was put to bed, lying on his back, with the limb supported upon a splint of an angle corresponding to that of the knee previous to the operation. This position was maintained until it was believed that the asperities of the bone had become blunted, and were not likely by their pressure to cause ulceration of the artery beneath them. This first splint was then removed, and another having the angle slightly obtuse was substituted. In a few days a third splint, with the angle more obtuse than that of the second, supplied its place. Others, varying in degrees of angularity, in like manner came in their turn to support the limb until it had attained a position almost straight. It was then unchangeably continued in that line until the contact surfaces of the bone had united and securely fixed the limb in this the desired direction.

During the treatment of the case, especial care was bestowed in protecting the popliteal vessels against any injurious encroachment upon them. With that view, all antagonizing pressure on the soft parts in the ham was carefully avoided. The limb was rested on two long bran bags, laid upon the splint, with their ends apart—a vacancy of four or five inches being left between them opposite the lesion of the bone. This interspace was lightly filled with carded cotton, so as to afford a safe support. Every symptom of pain or uneasiness in this part was promptly attended to. The occasional issue of a drop or two of blood from the corner of the sore, during the process of dressing the limb, caused me some solicitude in this case; whereas, ordinarily I should have considered it as a matter of no moment—it being so frequent an occurrence during the dressing of wounds, owing to the disturbance of the granulations, especially in compound fractures. The wounded soft parts finally healed and quieted his anxiety. The straightening of the limb having been very cautiously and by degrees effected, the first two months elapsed during the accomplishment of this object. Having then reduced it to the desired position, means were carefully observed to retain it so until the re-union of the bone had been fully completed; which occupied two months longer. The constitutional symptoms were such as usually occur in compound fractures—some-what severe, but at no time alarming. Throughout the whole treatment it was not found necessary to bleed him, or to have recourse to any very active constitutional measures. He was occasion-ally indisposed from irregularity in the digestive functions, but was always speedily relieved by resorting to mild and appro-priate remedies.
At the end of about four months from the date of the operation my patient stood erect, with both feet in their natural position, and the heels resting alike upon the floor, although a slight angle had been designedly left at the knee, in order that there might not be any necessity for throwing the limb out from the body in the act of walking, which is always the case when the knee is quite straight. After this period, the use of shoes of the ordinary shape was resumed, and the limb was daily exercised with increasing strength and usefulness. On the 19th of October, the Doctor took his departure for the South, bearing with him the injunction to continue the support of a small splint and the aid of a crutch or cane, until he should acquire sufficient confidence in the strength of the limb to justify him in laying them aside.

I was subsequently advised of his improvement; but was resolved not to give publicity to the case until the full and entire benefit of the operation could be ascertained. The wide distance which afterwards separated us prevented me from obtaining the necessary and direct information until within a recent period. I have the pleasure now not only to afford this intelligence, but to present it in the most satisfactory manner. Having written to the doctor for the information, and to learn from him in what manner it might be agreeable that I should refer to him as the subject of the case the following clear, satisfactory, and well written answer was promptly received. As the letter is full of interest in the case, I must be excused if I publish it almost entire, even though it contains some flattering sentiments for the one to whom it is addressed. That part only has been omitted which is in courtesy to my family.

Charleston, November 6th, 1837.

"My dear sir,—Your letter of the 8th October, directed to me at Mobile, has just reached me at this place, where I am on a visit to my parents. I received a letter from you last winter, some months after its date, and availed myself of the opportunity of a friend going to Philadelphia, and who promised me that he would see you, to send you a full communication of my situation then. I preferred this to writing by mail, as he had been with me, and could answer any particular inquiries you might make. On his return he mentioned that he had arrived in Philadelphia only a few days after you had sailed for Europe. Your letter of the 8th is the first information I have had of your return. I have the satisfaction and pleasure of saying to you now, that the operation you performed on my leg has been completely successful, and has more than realized my most sanguine anticipations. The small abscess, which you dressed the day before we parted at Norfolk, continued open, and threw out, from time to
time, small pieces of bone, until the August after, when the last piece was discharged; the orifice then closed, and I have suffered no material inconvenience from it since. From the January previous, however, I was going about and attending to my professional business; and early in the summer, when our sickly season commenced, I was on horseback daily, riding from thirty to fifty miles a day; without more than the ordinary fatigue or inconvenience. I am at present well; the wound sound; and I feel no other inconvenience in riding or walking, than what arises from my knee joint being stiff, which was the case before you performed the operation. I walk without a stick or other aid, with the sole of the foot to the ground, and my friends tell me, with but a slight limp; and I have great pleasure in adding that the leg and foot have increased considerably in size, so as now to be nearly equal to the other. When I think of what I was, and what I am; and that to your firmness, judgement, and skill, I am indebted for the happy change, I want words to express adequately all that I feel. I will not attempt it, but believe me, my dear sir, I feel it not the less. I shall remain here a week or two longer, and if you wish any further information on my case, do write me and I will give it most cheerfully. After that period I cannot say where a letter would reach me. Adieu.

* * * * * * * * *

and am, my dear sir, very sincerely, your friend, 

Seaman Deas.

To Dr. J. Rhea Barton.

P. S. In the statement you propose publishing of my case, I am quite willing you should refer to me in the manner you suggest, using my initials in the body, and my name at length in the note you propose appending to it."

Remarks.—In the case just recited, several difficulties of a peculiarly embarrassing nature presented themselves as obstacles to the restoration of this disabled limb; namely, the true character of the ankylosis, the extreme angle at which the joint was fixed, the changes of structure which must have taken place during the past sixteen years, and the probable condition of the flexor muscles of the leg. As these had not been called into action since boyhood, it was questionable whether nature had contributed to their growth in proportion to the development of the other parts of the body; or if she had, whether their contracted and inactive state for so long a time had not rendered them functionless and unyielding. It was not improbable, also, that the blood-vessels had acquired adhesions and an organic angular form at the bend of the knee. The operation was devised, and
the treatment pursued, with due regard to all these circumstances.

It must be apparent that, if the tibia and femur could have been disengaged from each other at the point where the original joint had existed, this should have been the selected spot; but it was forbidden by the bulk of the condyles, the adhesion of the patella, by the extent to which the incisions would have been required, and by the disadvantageous position of the popliteal artery, as it lies embedded in the recess between the condyles of the femur.

The most eligible spot for the section of the bone is that available point which is nearest the original joint, and is at the same time free from these objections. Hence the choice which was made.

The flap elevated from the bone was composed of parts which, in a natural state of the knee-joint, it would have been improper to have divided; but as the articulation had been annihilated, the functional importance of the parts appertaining to it had ceased; consequently no material disadvantage was to be apprehended from a division of them. The shape and direction of the flap were believed to be those best suited to the necessary and convenient exposure of that portion of the bone which was to be exercised, and with the least possible injury to the adjacent parts. The profile of the piece of bone which was removed formed a tolerably accurate equilateral triangle. Its shape was of importance to the success of the case; and the angle at which the section should be made became a matter of calculation. If it had been cut at too acute a degree the new surfaces would have reached each other before the limb had been sufficiently extended; if at too obtuse an angle, the leg would have borne full extension without entirely closing the gap. The reunion, consequently, might have been prevented. If a single transverse section of the bone had been made, instead of the above, there would have been a necessity for great elongation of the flexor muscles of the leg and yielding of the other soft parts behind the knee, and if accomplished, there would have been left between the divided surfaces of the bone a large triangular gap or chasm, which would most probably have occasioned a false or artificial joint, without the requisite muscles to control its movements. By the excision of the wedge-like or triangular piece in front, the axis upon which the bone turned was brought so near to the muscles, or their agents, the tendons in the ham, that a slight deviation from their direction only was required, instead of an elongation of their fibres. In proportion, also, as the limb was extended, the chasm in the bone, occasioned by the removal of the piece, became diminished; and upon the restoration of the limb to the nearly straight line, the gap was closed by the approximation to
contact of the sawn faces of the bone, in a manner resembling the corresponding surfaces of an oblique fracture, when accurately adjusted.

It is not the least interesting circumstance connected with the history of the case, that the subject of it was an enlightened physician—one capable of appreciating our profession as a science—and for the undeniable proof which he has afforded of his confidence in it, he fully merits our thanks, as well as all the special benefit which he has derived from the operation.—Amer. Jour. of the Medical Sciences.

Lithotomy.

On contemplating any surgical operation which has proved as dangerous on the hands of the ablest surgeons, as Lithotomy, the practitoner who has been no more accustomed to its performance than the generality of those who are distributed throughout our widely extended country, feels the necessity of something like a practical formula which will afford a clearness of view, and decision and confidence in every point appertaining to ultimate success. However dexterous and successful he may have been in other operations, he feels that, in Lithotomy, surgery has, with the best practitioners, effected a smaller proportion of good success than in almost any other operation. Peddling Lithotomists who have had their day in Europe, have indeed operated boldly by a rote formula, without science, and with various, but not very good success—boldly, because like the steamers, they were most stupidly and culpably ignorant of the dangers into which they plunged. Their formulaæ have been taken up and adopted by men, accomplished in anatomical, physiological and pathological knowledge and surgical dexterity; with but little better success. The ill success which has too generally attended this operation, has been so much believed to be inseparable from the nature and importance of the anatomy concerned, as to lead to a thorough anatomy of the seat of the operation, and finally to the efforts of Civiale and others for avoiding entirely the dangers of the gorget, by substituting other plans of operation on the stone, which have hardly equalled the first expectation. Yet they are more freely ventured, because the dangers of the knife
or gorget are not in array before the practitioner. But Dr. Dudley's success in this operation has been such as is calculated to impart confidence in the general contemplation of lithotomy; and as this operation, apart from its dangers is calculated to be more thoroughly successful, and even less painful than those which spare cutting instruments, we think it important that the whole management of this operation should be fixed in the mind of the practitioner. We are aware that the best showing is made of Dr. D's. success in this operation, and that all kinds of cases which may be numbered for increasing the numerical showing of success have been thrown in and counted; but still, peculiar circumstances have thrown a larger proportion of cases of this kind into the hands of Dr. D. than fall to the lot of most surgeons; and he has consequently become more familiarised to their treatment in this way, and more confident in the undertaking. It should be remarked however, that he who will undertake the management of all cases of urinary calculi, may not expect a nominal success equal to this reported of Dr. Dudley, because, like many other practitioners who have a special eye to fame in naming his proportionate success, he has only operated on favorable cases, or such as he was able to render so by previous treatment. Still we are pleased with his formula, and feel that it is one calculated in its nature to inspire a more than usual confidence in the operation. And in all this, we think there is no point of more special importance than that of due preparation of the patient, and the fact of his being absolutely free from all other pathological conditions.

Dr. Dudley's operation is of itself, simple and easy—having nothing new or peculiar; and from the effect of due preparation of the system, and exemption from other diseases, in many other cases as catarrhal affections, amputations, accouchement, &c., we are bound to attribute whatever little success which may have followed Dr. D's. operations for the stone, more to the due regard to preparation and the excellent nursing and subsequent management, than to all other particulars of his practice: that is to say, that altho his operation is unobjectionable, it is not new, and could not produce a greater proportion of good results than it has before done, without due regard to the previous state of the system, and to the subsequent good management. The importance
of due preparation of the system cannot be too strongly pressed on the attention of all practitioners. It should be borne in mind that cet. par. a due and judicious regard to this will give better success in the practice of midwifery; and that the immense difference in the result between cases of small pox by inoculation, and those from its reception in the natural way, is attributable, not in the least to the fact of inoculation, as this is only another way of applying the same cause; but to that preparation of patients which is duly attended to before the disease is voluntarily communicated.

We cannot do so much justice to this subject in any other way, as by giving the entire formula contained in Dr. J. M. Bush's paper on Dr. Dudley's operation, contained in the Transylvania Journal of Medicine.

"Whether the deranged state of the stomach, in the patients, who come to Lexington for surgical operations," says Dr. D., "be the result of irritation or any other morbid action, (we have not seen a case of inflammation of the organ,) Professor D. has secured his unparalleled success chiefly by the liberal use of emetic medicines. To see the paramount importance he attaches to a perfect preparation of the general system, of all his patients, who are to submit to the knife, is to witness his seemingly tedious course, under such circumstances. While we have known a patient arrive on one day, with cataract, or a tumour, or hydrocele, or stone in the bladder, or any disease small or great, and put upon the table the next day for operation; we have seen another remain several months, undergoing treatment all that time, to change morbid actions of the digestive apparatus to healthy."

Professor Dudley "condemns most earnestly the use of the lancet, as a preparatory mean for the performance of operations; a practice that is reprobated now, we believe, universally, by the most intelligent surgeons of every country."

Professor Dudley does not consider ulceration of the bladder as forbidding the operation, provided, by proper treatment.

"The digestive organs are restored to their natural actions, the agonizing paroxysms of pain retire, the discharges of blood and muco-purulent matter subside, and clear inoffensive urine appears in more copious and natural discharges."

Under such circumstances he has successfully operated.

The following is the course of treatment pursued by Prof. D., as detailed by his friend:

"When a patient applies to Professor D. with the ordinary
Lithotomy.

symptoms of stone; to ascertain its existence, the first step of course is to explore the bladder with a metallic sound. This simple but indispensable operation, however, he never performs, in any case, for several hours after the arrival of the patient; and not even then, if there be pain in the organ or the slightest fever. For it is considered highly detrimental to the sufferer, to disturb the constitution while thus deranged, even with an instrument usually so harmless. Should the general condition of the patient's body not otherwise forbid, the day after his arrival, he is sounded, having taken a general warm-bath the evening previous. But if he be suffering with paroxysms of the stone, and his blood-vessels exhibiting febrile action, with a deranged state of the alimentary tube, more energetic treatment is required before the instrument is passed into the bladder. Nauseating potions of ipecacuanha or tartar are exhibited, and should these fail to reduce the pulse and restore cutaneous action, or a proper condition of the bowels, aided by the bath, an emetic, or cathartic, or both, are then superadded, with light and abstemious living. By these means two objects are effected. The first, is a prevention of any irritating results from the examination of the bladder; the other is that so much is gained in the preparatory management of the general system. Until he is completely satisfied that all the organs are in the healthy performance of their various functions, he will not operate. When, however, it is believed that the patient is ready, having been one or more times sounded, he is placed on the table and tied; immediately the staff, being oiled, is introduced into the bladder, and left resting upon the stone; when the assistant grasps it firmly, to maintain it in the bladder, at the same time holding it perfectly perpendicular to the table, carefully avoiding any inclination of it to the right or left. The convexity of the instrument being distinctly felt in the mesial line of the perineum, the operator, seated in a convenient chair, with his instruments spread on his right, proceeds to the operation. While the left hand controls the scrotum and perineum, the right makes an incision, with a middle size convex-edge scalpel, beginning a little below the root of the scrotum, and terminating an inch, more or less, behind the verge of the anus, in a straight line, through a point midway between the verge of the anus and the inner edge of the left tuber ischi. This cut divides skin, subcutaneous tissues, and perineal fascia. The second stroke of the knife is not so extensive, it divides only the posterior fibres of the accelerator urinae, and transverse perinei muscles. Always at this stage of the operation, if the perineum be remarkably concave, presenting an inclined plane, or if the arch of the pubis be much contracted, Professor D. introduces the left middle-finger into the rectum, and draws off the bowel to the right. The fore-finger then placed in the wound conducts
the scalpel through the membraneous parts of the urethra into the groove of the staff, cutting from the rectum towards the bulb. The scalpel is now laid aside and the gorget is taken up, the beautiful instrument of Mr. Cline; with its cutting edge towards the pubic arch; its beak is made to engage the groove of the staff, while the assistant resigns the latter to the surgeon's left hand, who for an instant playing the two instruments against each other, lateralizes the former, turning its cutting edge to the left, poises it for a moment, perfectly horizontal, before he plunges through the prostate into the bladder. At the same moment the gorget is passed with the right, the left hand depresses the handle of the staff; the bladder being opened, the staff is withdrawn, and the surgeon's fore-finger of the left hand, directed by the gorget, is passed into the bladder, and the instrument withdrawn; the wound in the neck is dilated, forceps introduced, the calculus seized, and by steady, firm, and dilatory movements, from below upwards, and from side to side, the operation is completed by the extraction of the stone. The bladder is now cautiously explored with the scoop, and if found clear is filled with warm water from a syringe; the patient is untied, turned to his left side and thus put to bed, and is required to maintain the position for from two to four days. We have seen professor D., in making this operation, release his patient in forty seconds from the first incision, while upon other occasions, twenty minutes were consumed before the extraction of an enormous calculus could be safely effected. He makes it a principle never to operate in any case against time, but always firm, deliberate and dextereous, he goes through what is before him with a rapidity compatible with circumstances and the safety of his patient. In the operation of lithotomy especially, his incisions are made with the greatest expertness and brilliancy; and notwithstanding we have repeatedly assisted him, we have not realized the moment when the gorget was passed, the staff withdrawn, and the finger thrust into the bladder; these three different points of the operation, always seem to be the work of an instant.

"In all his operations, he has used but two sizes of the gorget, the smaller seventeenths, the larger eight-tenths of an inch broad in the blade. With the latter instrument, he has made an incision through which was safely extracted a calculus, three and a half inches in its long diameter, two and a half in the short, and eleven in the circumference.

"It is evident that the larger size of the two is not wide enough to divide completely the prostrate laterally, in the adult, or even at any age from twelve years to maturity; while with the smaller gorget, the prostrate of the child from three to twelve is entirely safe from the invasion of its capsule. Certainly the opening made into the bladder, by either of the two instruments, is often very
disproportionate to the size of the stone, still in one hundred and forty-three cases, in which those identical gorgets have been used, calculi varying from the size of a pea to that of the magnitude just cited above, have been extracted, with complete success and safety to the bladder, in all cases; and in but four did the subject die before he had time to enjoy the happiness of a cure. Yet in those four cases, which failed to realize the benefits of the knife, in consequence of the supervention, or aggravation, of other diseases, beyond the control of remedies, the bladder healed before death, or they passed the usual period of closing.

"Some surgeons when they have cut into the bladder, and ascertained that the stone is very disproportionate to the extent of the incision, prefer using cutting instruments a second time, with the view of extending the cut in the prostate and neck of the bladder. This practice our teacher has never adopted; but on the contrary always condemns. He contends, it is more philosophic surgery, in such cases, to extract the stone by increased attractive force, risking even a certain degree of laceration. No surgeon estimates more highly than he the advantages of a clean, smooth incision; but his extensive experience, in the operation of lithotomy, has entirely satisfied him, that the danger so generally ascribed to violence done the deeper tissues, in laceration, is not at all comparable with the beneficial consequences of such practice. Indeed the results of his mode of operating, under circumstances of a large calculus, induce him to inculcate the principle, that it is better, safer to extract by force, according to the size of the stone, than to resort a second time to the knife. In every instance, where he was required to remove a stone, which he commanded with forceps in the bladder, he had uniformly extracted by gradual dilatory and tractive force, without in a single case dragging away any of the soft parts.

"Every surgeon understands the great facility with which a calculus may be taken from the female bladder, so dilatable are the parts concerned in the operation. Unless the accretion be of unusual magnitude, Professor D. does not in the female use any dilating means previous to the moment of commencing the operation. After the patient is throughly prepared, and it is ascertained that the calculus is moderate in size, he proceeds directly to the operation. The same position is required as in lithotomy. With a graduated supply of forceps, he first introduces the smallest size, and gently expands the blades, in various directions, until the urethra and neck of the organ will admit the next size instrument; so on until with forceps of a proper kind, he can grasp and remove the calculous body. We have witnessed this operation, made upon a little girl six years of age, completed in forty minutes by the removal of a stone of the size of a pigeon's egg, and the pain did not seem to equal that caused
by the extraction of a similar size body in lithotomy. This patient was perfectly well in five days after the operation, without the loss of the powers of the sphincter vesicae. If the urethra and neck of the female bladder is so extremely relaxable, under the influence of instruments, without the aid of incision, why not expect to find the same accommodation in parts similar in the male, with the addition, to be sure, of the prostrate body; a piece of anatomy, that Nature seems to have constructed with a peculiar fitness to facilitate extraction, doing away the necessity of a dangerous encroachment with the knife, beyond the point of its fibrous envelope. The prostrate gland appears not only to possess the property of ready and innocent laceration, splitting of its tissues, but also of extensive dilation; and indeed it would appear that its strong capsule was also accommodating in relaxing character.

"The operation completed, the patient is put to bed without the slightest dressings of any kind, but required to remain on the left side, until suppuration is established. He is not disturbed, even with sponge and warm water until twenty-four or thirty-six hours after the operation. From his long experience Professor D. does not fear infiltration of urine; nor has he any reasons to adopt means, such as catheter and sponge, or any material for the purpose of plugging the wound, since such a result never has followed his operations. Infiltration unquestionably does occur in many cases, and sometimes terminates in sloughing or mortification of the parts involved, and even in the death of the patient. But we do not believe that either of those undesirable effects do follow, (unless in very rare cases,) as a mere result of the operation. We should rather ascribe such a state of things, nine times in ten, to the ill condition of the general system previous to and at the time of the operation. Under such circumstances serious wounds of any description, are, most assuredly, far less manageable than in the opposite state. Then when the perineum, the urethra, prostrate body and neck of the bladder have suffered a solution of continuity, when morbid actions are existing in the economy, nature too often must fail in her restorative attempts, while healthy progressive inflammation cannot develop itself sufficiently to erect barriers to the diffusion of urine; adhesive lymph is not thrown out in sufficient abundance along the incised parts, thus to restrain the limits of the urinary discharges."

Anatomy of the Lymphatic System.

It is a sign of a healthy condition of the professional mind, when we observe a disposition to cultivate anatomy, and to ex-
tend the knowledge of its facts. The accurate determination of the latter is so inseparably connected with the science of medicine, that the increase of our acquaintance with them must necessarily lead to its improvement.

The diffusion of such knowledge is a great desideratum. It is not, however, very readily effected, for the majority of persons are prone to seek for the minimum, not the maximum, of attainments which will keep them afloat in their respective occupations, and far too many medical men, who have studied anatomy as a task, are apt to regard it with disgust, and to hug the idea that a small quantum of it will serve them in the practical part of their profession.

It is not necessary for us to combat such a notion with elaborate arguments, nor to bring the heavy battery of reason against so obvious a fallacy. One consideration is sufficient of itself to overthrow it. The signs and symptoms of disease are no more than altered states or functions of the organs or the tissues:—it therefore follows as a necessary consequence, that the more intimately we understand the normal condition of those tissues and organs, the more accurately, we shall understand their disordered states, more accurately, in short, we understand disease.

If the man who is content to be a routine, could calculate on always meeting with routine cases, there might be some sort of petty and contemptible rationality in his election. But, in the simplest case, contingencies may supervene, and complications, more or less obscure, may arise. Will any man of honour, will any man of prudence, voluntarily set out in life, with the determination to do just as little as possible, towards making himself master of such complications and contingencies?

If we appeal to experience, every-day experience, we must be satisfied of the de facto superiority in practice of the surgeon or physician who is thoroughly versed in anatomy. Of course we do not say that an excellent anatomist may not be a deplorable practitioner. That is not the point. The position we contend for is, that cæteris paribus, the better the anatomist the better the practitioner, and that the reasonable cultivation of anatomy does not tend in any way to incapacitate persons for practical pursuits. We admit that men who spend all their time in the dissecting-room, who think of little else, work at nothing else—we admit that such men are conversant only with the dead, and cannot be expected to be otherwise; but that is not the kind of anatomical study we are talking of. We wish every surgeon and physician to continue to take an interest in anatomy after his collegiate examinations are over—to look at the best anatomical works—to keep himself at the level of advancing anatomical knowledge. We do not urge the high consideration, that it is pleasant to feel we are not mere laggards in science, but merely
hint the more homely, but perhaps the not less potent one, that it is useful to know as much as other people.

It has been generally stated in most works, on descriptive anatomy that lymphatic vessels may be discovered in every part of the human body, except in the substance of the brain, spinal marrow, eye and placenta. The recent researches however of M. Fohmann, Panizza, Arnold, and some other continental anatomists, have shewn that this assertion is not strictly correct, and that it is to be received with certain limitations. The first of these authors has described and represented with great care numerous lymphatics in the membranes and on the surface of the brain, and also those of the umbilical cord and of the placenta. Arnold says that he has detected lymphatic vessels in several of the tissues of the eye; but this announcement has not certainly been confirmed by the dissections of others; and, in the opinion of many, even the authority of M. Fohmann has not been deemed sufficient to establish the existence of lymphatics in the placenta. M. Panizza, for example, certainly one of the ablest writers on the absorbent system, confesses that he is not satisfied of the truth of this statement.

Leaving therefore these points of dispute, we shall now very briefly detail some of the most interesting and best ascertained facts, relative to the lymphatic system.

According to M. Breschet, the cellular tissue is the principal source or ground work whence the lymphatic vessels take their origin; it is the soil, so to speak, in which their extreme roots strike and ramify.

If we observe lymphatics proceeding from, and seeming to arise out of, the substance of many organs of the body, it is because cellular tissue constitutes the basis and elementary component of these organs; and hence we find that those very parts, into which the cellular tissue does not enter, are destitute of these vessels—as, for example, the nails, horns, the epidermis, the hair, and the enamel of the teeth. Mascagni long ago announced the opinion that all the white tissues of the body are actually formed or composed of innumerable lymphatic vessels—an opinion too vague and general to be received as quite correct—and more lately M. Cruveilhier has stated in his descriptive anatomy that he deems it very probable that the cellular tissue and serous membranes primarily and chiefly consist of a meshwork of these vessels.

There are two methods in which the lymphatics of serous membranes may be exhibited—either by inserting the sharpe point of the usual apparatus, used for injecting these vessels and filled with mercury, into the substance of the membrane, or by introducing some colored liquid into the cavity lined by it, during life, or immediately after death. Those of synovial membranes
may be discovered in the same manner. The lymphatics of the dermoid tissue have of late years been examined with great care by Lauth, Fohmann, and by M. Breschet himself. He has described them minutely in his work on the structure of the skin published at Paris two years ago. They are exceedingly numerous, and appear to be, for the greatest part, more superficial than the vascular capillaries. No trace however of any open orifices on the surface of the dermis can be discovered, even with a strong magnifying glass. Some anatomists indeed have stated that they have observed globules of the mercury ooze out, when pressure was made on the skin at the same time; but this phenomenon is ascribed by Breschet and others to rupture of the lymphatics, and not to simple exudation from them. He contends therefore that there are no discoverable open extremities of the lymphatics on the surface either of the skin, or indeed of any other tissue of the body.

M. Panizza takes the same view of the subject. He details several experiments, in which he made a very successful injection of the superficial lymphatic plexus of the glans penis, and wherein, on carefully removing the epithelium, no trace of any exuded globule of mercury could be found.

The mucous membranes, like the skin, give rise to numerous lymphatics; so numerous indeed, that they, along with minute blood vessels and nerves, appear to constitute the villosities on their surface. Although no open orifices have ever been discovered in mucous membranes, MM. Cruveilher and Magendie are of opinion that such do really exist.

They allude to the results of several experiments, in which the abdomen of a living sheep was opened, and milk introduced into a part of the gut. When the distended portion of the gut was compressed with the hand, the lacteals were immediately filled with the white fluid. M. Breschet however is not satisfied with the details of these experiments; and he very justly says that all forcible compression of the gut should be avoided in conducting such experiments, as it may possibly cause a rupture or laceration of the extreme ends of the tender vessels. However this may be, no person has ever seen the open orifices of the lacteals on the surface of the villi; and our author is therefore quite correct in condemning the practice of assuming the rationale of physiological phenomena, merely because these may be explained in a particular manner. It is exceedingly probable that imbibition or some similar process, has something to do with the introduction of the chyle and lymph into the extreme absorbents. But in truth all that we know with certainty is, that we know nothing certain on the subject. It remains a problem for future anatomists to solve.

Equally gratuitous is the doctrine, that the lymphatics are
continuous with, and are derived from, the extreme capillary branches of the blood-vessels. It is well known to anatomists that, by pushing a fine injection into an arterial trunk, the lymphatics of the part sometimes receive a portion of the injection. This experiment has been deemed quite sufficient by many writers to establish the reality of a direct insinulation of the two sets of vessels; nay, Lippi has gone so far as even to represent such a connection in one of his engravings.

M. Breschet however, and, we believe that we may add, almost all the best anatomists of the present day do not admit the truth of this doctrine. They attribute the passage of the injection in the above experiment to extravasation from the minute arteries, and to consecutive absorption or rather impulsion of it in the lymphatics. Our author closes his remarks on this subject by adopting the opinion of Panizzia, that “anatomy has not hitherto succeeded in determining, with physical certainty, in what relation the sanguiferous and lymphatic systems stand to each other, at their extreme ramifications.” If we suppose that the cellular tissue itself is in fact made up of a meshwork of lymphatic vessels, we perhaps get rid of the difficulty; but even then we cannot state, with any degree of precision, in what manner the lymph and chyle do enter into the vessel themselves. We know that the lymph and chyle contain numerous globules of considerable size, larger even than those of the blood. The question comes to be, How do those gain admission? or are we to suppose that they are developed within the tubes of the lymphatics themselves?

We thus encounter fresh difficulties at every part of the investigation; and it is therefore much wiser to confess our total ignorance of the subject.

Nothing is of greater importance in all physical researches than to acquire exactitude at every step of our investigation. Whenever we begin to frame hypotheses, unsupported by actual observation, merely for the purpose of giving a probable explanation of certain phenomena, we are almost sure to be bewildered in the mazes of our own making. We shall therefore leave the subject, on which we have been discoursing above, merely repeating our own former statement that no anatomist has yet seen any open orifices of lymphatics in any structure of the body, either in man or in the lower tribes of animals.

We now proceed to another topic, in the history of the human lymphatic system, which has given rise to much difference of opinion. We have confessed our ignorance of the relation which exists between the capillaries of the sanguiferous and of the lymphatic systems. It now remains to be determined what knowledge we have respecting the connection between the larger vessels of these two systems.
It has often been conjectured by anatomists, that there are some other communications between the lymphatics and the veins, in the human body, in addition to the thoracic duct, and the lymphatic trunk which proceeds from the right arm and usually terminates in the right subclavian vein. That such communications do exist in the lower classes of vertebrate animals is proved beyond doubt by the researches of Fohmann, Lauth, Panizza and Müller. They are obvious on the intestines and the mesentery in fishes, on the thigh in the frog, and on the lower extremities and the intestinal canal in birds. No such communications however are discoverable in any part of Mammals. Still the question has often been mooted whether any inosculations between the two sets of vessels, besides those above mentioned, do really exist in this class of animals.

Many of the older anatomists believed, that they had traced lymphatics to their terminations, not only in the vena cava, and v. azygos, but also in the hypogastric and lumbar veins; and even as late as 1825, M. Lippi of Florence has maintained that the lymphatics of the digestive organs in men, as well as in other mammals and also in birds, communicate directly with the venæ caveæ, the vena portæ, the vena azygos, and with the renal and internal pudic veins. But all the best authorities, the more ancient as well as the very recent, have denied the accuracy of these statements; and it has been clearly proved that the error of Lippi has arisen from his mistaking branches of veins themselves for lymphatic vessels.* Fohmann says distinctly that, during five years most laborious investigation of the absorbents, he has never detected any direct communication between the veins and the lymphatics, except in the glands, (this, we shall afterwards mention, is a doctrine not admitted by all anatomists) and in the immediate neighbourhood of the termination of the thoracic duct.

But although no communication can be traced with the larger veins, some authors have contended that the lymphatics inosculate directly with the smaller venous branches in different parts, and especially with those of the mesentery.

M. M. Fohmann and Panizza however expressly assert that they have in no instance been able to detect such a connexion.

The only other hypothesis on the subject of venous and lymphatic communication, which remains to be mentioned, is that which supposes that the two sets of vessels inosculate with each other in the substance of the absorbent glands. This opinion was adopted by the first Meckel, and also by Caldani.

*Rossi has shewn conclusively that Lippi's error was that of mistaking veins for lymphatics, by demonstrating that the vessels believed by Lippi to be lymphatics had no valves, a fact which proves them veins; as all lymphatics have valves, and all veins less than one line in diameter are destitute of them.—Ed.
Monro Secundus and Mascagni were opposed to it; but, of recent years, it has been again very ably advocated by Fohmann, Tiedmann, Lauth and Panizza.

It has been long known that, when mercury is injected into those lymphatics which proceed on to a gland (vasa inerentia), part of it passes more or less freely into the neighbouring veins, as well as into the lymphatics leading out of the gland (vasa efferentia). This transmission is sometimes so very free, that, it is actually necessary to put a ligature around the veins, in order that the lymphatics may be well filled.

Various explanations have been given of this occurrence. Some writers have supposed that it was always the result of rupture of the glandular tissue; others have attributed it to a direct communication between the two sets of vessels; and lastly it has been conjectured that it might be owing to a mere transmission from one set of vessels to another.

It cannot indeed be doubted that, in not a few of the experiments, there has been an actual rupture of the substance, of the gland, and that the quicksilver has escaped into the cellular substance, and been forced into the lacerated vessels. But this has certainly not been the case in all. The greatest delicacy has been used by the anatomists last mentioned to avoid all force or compression in their experiments; and we cannot therefore fairly presume that extravasation of the mercury has always taken place. No traces of such extravasation have been observed; and moreover it has been often remarked that the veins were filled with the quicksilver even before the lymphatics, which issued from the gland, were injected. It may also be added, that, if an extravasation had occurred, we might reasonably expect that the small arteries as well as the veins should receive a portion of the injection; and yet this is never the case. M. Fohmann considers these arguments quite conclusive; and he has supported with great ability the doctrine, that there is in truth a direct communication between the lymphatics and veins of an absorbent gland. It must however be admitted that although there are some very plausible arguments in favour of this supposition, no direct observations can be adduced in its support. M. Panizza has mentioned one fact which seems to be very unfavorable to it. He says that he has never succeeded in injecting the lymphatics, when he reversed the mode of performing the experiment—viz. That, when the mercury was injected by the veins, none ever entered the lymphatics. This result might certainly have been expected, if there is really so direct a communication between the two sets of vessels, as M. Fohmann maintains.

Panizza therefore and Müller also are inclined to believe that the passage of the mercury, in the substance of the absorbent
glands, from the lymphatics into the veins, is probably effected by means of minute pores in the sides of the vessels, analogous to those which permit the atmospheric air to act upon the blood in the pulmonary cells.

Breschet however, without giving a decided opinion, appears to lean to M. Fohmann's hypothesis that there is a direct communication between the lymphatics and the minute veins within the substance of the absorbent glands—a doctrine that receives considerable support from the results of comparative anatomy, and also from the carefully performed experiments recorded by Meckel, Fohmann, and many others.

Whatever view we take of this subject, there is every reason to believe that during life, fluids may pass somehow or other from the lymphatics into the veins of the glands. If we admit this doctrine, we can readily understand in what manner certain substances may pass very quickly from the stomach and bowels into the mass of circulating blood, and be appreciable at different emunctories, within a very short time after they have been introduced into the alimentary canal.

"If," says M. Breschet, "after all the facts now detailed, we must admit that the lymphatics seem to have very little action upon substances introduced from without (the chyle excepted), they most unquestionably exercise such a power over those which are generated in the body. The bile, for example, may be absorbed by them—a fact indisputably proved by the researches of the two physiologists named above."

In concluding his review of the conflicting opinions on the subject of venous and lymphatic absorption, our author adds that the solution of the problem is perhaps less obscure, than has often been supposed. He supposes that the veins and absorbents communicate with each other in the substance of the glands, as they are seen to do in some of the lower vertebrate animals in various parts of the body. The chief point of doubt is, as to the manner in which this communication is effected. M. Breschet does not think that it can be by direct communication with each other by open mouths; but rather "par une force absorbante, analogue à celle dont jouissent les radicules lymphatiques, que les veines doivent absorber le contenu de ces derniers vaisseaux dans les glandes."

In fine, he supposes that the veins of every part have the power of absorption, as well as the lymphatics; but he is unable to

* The doctrine—that there is a free communication between the veins and the lymphatics in the substance of the absorbent glands—is maintained by that admirable anatomist, M. Fohmann; and certainly the comparative anatomy of the lymphatic system, as we have described above, affords considerable show of confirmation to it.
specify the limits and the circumstances in which each set of vessels exercises this function.

He alludes to a question of great obscurity, and which in all probability will always elude the search of our best-directed enquiries—viz. how and in what part are the globules of the chyle formed? We cannot suppose that they are already formed before the chyle is absorbed from the food, as they are known to be of considerable volume, and as no open mouths of the lacteals have ever been discovered in the villi of the intestines, even with the aid of the strongest magnifying glass. It is an utter loss of time to form mere idle conjectures on this subject. "Ne craignons pas de dire," says M. Breschet, "que l'on ignore complète-ment!" There are many other points in the anatomy and physiology of the absorbent system, which are little, if at all, accurately understood; and we are therefore compelled to confess that we know but little, and that little only imperfectly, of one of the most important functions of animal life.

The following statement of the chief conclusions, to be drawn from the preceding remarks, may be useful to the reader.

1. In no part of the body have any open extremities or orifices of absorbents been discovered.
2. No direct communication between the arterial or venous capillaries and the absorbent vessels have been distinctly proved to exist.
3. Fohmann and some other anatomists assert that a communication exists between the absorbent vessels and the veins, within the substance of the alveolar glands. This doctrine, although probable, is not admitted by all.
4. The absorbents in fishes are not provided with valves, or with glands. They communicate freely, in different parts of the body, with the adjacent larger veins.
5. In reptiles the lymphatics have imperfect valves; but there are no glands. We discover however along their course certain dilatations which are seen during life to pulsate, and which appear to force the lymph into the adjacent veins.
6. Rudiments of absorbent glands are discoverable in birds; and in them there are several other direct communications between the lymphatics and the veins, besides the chief one at the root of the neck.
7. In mammals, the lymphatic system is more complicated; the valves are more numerous and more perfect; the glands are more numerous; and the communications between the lymphatic vessels and the veins appear to be fewer in number and much less direct.
8. The veins seem to have the power of absorbing foreign substances admitted into the body, as well as the lymphatics; but it is still doubtful whether they ever absorb chyle or any other fluid generated within the system.
The Anatomy of the Skin.

The next of the works upon our list is still from the pen of M. Breschet, an able anatomist—and on the structure of the skin, an important subject. Unfortunately its difficulty is commensurate with its importance, and the discrepancies of observers have corresponded with its difficulties.

M. Breschet, and his colleague in the work, M. Roussel de Vauzème, have used all the means in their power for arriving at the truth. Time has been consumed and labour expended on an inquiry which all must acknowledge to be intricate, and which they confess to be on their part incomplete. They deprecate criticism, indeed they may be said to defy it, for they protest in limine that they will pay attention to no strictures but to those which emanate from persons who have made the structure of the skin a subject of their special investigation. This is a bold, but perhaps it is not altogether an unexceptionable determination.

They treat their subject in the following order. They first give a summary of the constituent parts of the skin, and then examine each in detail.*

Constituent Parts of the Skin.

1. The *Derma* (cutis vera—corium—true skin)—a dense, fibrous, cellular web, enveloping and protecting the sanguineous capillaries, the lymphatics, the nerves, and the other organs contained in the skin.

2. The *Papillae*—the organs of touch, in which the nerves are finally distributed. They are small nipple-like projections, slightly curved, their spines blunted, and concealed beneath several envelopes.

3. The *Diapnogenous Apparatus*, (the organs of the secretion and excretion of the sweat—from *expiare*, traspiratio perspiratio)—is composed of a glandular parenchyma, and of *sudoriferous* or *hydrophorous* canals.†

The parenchymatous or secreting organ is enclosed in the derma, and gives origin to the excreting canals; the latter are of a spiral form, pass between the papillae, and open obliquely on the external surface of the epidermis.

4. *Apparatus of Inhalation, or Absorbent Canals*. These resemble, in many respects, the lymphatic vessels. They are

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* We shall chiefly select such facts, or such opinions, for notice as possess some claim to novelty, or are otherwise important. We must presume that our readers have read Beclard's, or Craigie's, or some other work, which contains what is generally known or believed in regard to the cutaneous tissue.

†From *ίδρως*, sudor.
situated in the corpus mucosum, which forms the most external layer of the skin, for the cuticle or epidermis is only a dependence of the mucous body. The inhalent canals appear to be unprovided with mouths or with absorbent openings. They commence in the most superficial layer of the cuticle, but their mode of origin is very difficult to be determined. By their other extremity they communicate with a net-work of vessels, which our authors believe to be lymphatics mixed with veins.

5. The organs which form the Mucous Matter, or the Blenogenous Apparatus.* This is composed of a glandular parenchyma, or organ of secretion, situated in the thickness of the derma—and of excreting canals, which issue from the preceding organ, and deposit the mucous matter between the papillae.

6. Chromatogenous Apparatus, or that for producing the Colouring Matter. This is composed of a glandular or secreting parenchyma, situated a little below the papillae, and presenting particular excreting canals, which pour out on the surface of the derma the coloring principle. This mingles with the soft and diffusent mucous matter, and it is their mixture that produces the "pretended reticular body of Malpighi," and the epidermis or cuticle. We must attribute to this double apparatus the production of horns, scales, prickles, hairs, nails, hoofs, and so forth.

Our authors have ascertained, that of all the cutaneous expansion in man, the skin of the heel is the best adapted for anatomical examination, on account of the thickness of the derma and the mucous matter. To this part, therefore, their observations must be understood to apply. The anatomical causes of the difference displayed by different portions of the skin will be discussed at a future opportunity.

Such is the sort of table given by our authors of the anatomical elements which constitute the skin. The enumeration will at once convey to the well-informed anatomist the essential differences which exist between it and the ordinary descriptions of the cutaneous apparatus. On this point we need not dwell, but proceed to display the opinions of our authors in detail.

The body of the work is divided into six chapters. The first is on the derma—the second on the papillæ—the third on the diapnogenous appparatus—the fourth on the apparatus of inhalation—the fifth on the blenogenous apparatus, or organs which secrete the mucous and the horny matter—the sixth on the chromatogenous apparatus, or organs for the secretion and excretion of colouring matter. Appended to these chapters are some remarks on the pathology of the skin, and some conclusions.

* From Blenva, μυζ, mucus.
On the Derma.

On this head our authors say little that is new; and we shall confine ourselves to stating that the result of their observations amounts to this—that the derma is a membrane, the fibres of which, firmly interlaced with one another leave interstices, areolæ, or cells, which protect and insulate a great number of organs. On its external surface it insensibly merges into a membrane, which appears to be confounded with the parenchyma of the colouring and papillary organs. The derma, according to our authors, is by no means the inextricable net-work of fibrous tissue which some authors have represented it to be. It contains and supports organs, the precise description of which will come by-and-by.

In serpents, the derma has a remarkable disposition: it is raised into imbricated prominences, covered by a thin layer of epidermis, and their whole constitutes the scales. In fish, on the contrary, the surface of the derma is uniform, and the scales are entirely composed of horny matter.

On the Neurothelic Apparatus, or the Papillary Bodies.

There is no doubt that the filaments proceeding from the different nervous trunks in the subcutaneous cellular tissue divide to extreme minuteness on approaching the derma. However carefully we endeavour to dissect them, we most commonly lose them in the structure of the derma, by reason of its opacity and their delicacy. The only chance of distinguishing them, in the midst of the vascular net-work of the derma and of its excretory canals, is to observe very carefully the point where they enter and to which they tend, and with practice and dexterity the nervous filaments may be picked out as they approach the mucous body, where they are delicate, almost pulpy, and penetrate the base of the papillæ.

The papillæ themselves are ranged in rows, generally bifid or trifid, separated transversely by the interval in which the sudoriferous canals are lodged, and longitudinally by the grooves from which the horny or epidermic matter proceeds.* Their form is that of a small cone, the base of which is lost in the derma, while the summit ends in a blunt point. Each papilla penetrates the horny matter, as a sword enters its sheath, and the internal surface of the epidermis exactly represents, by its symmetrical depressions, the number and disposition of the papillæ.

* The terms "mucous matter," "horny matter," are used almost indiscriminately, because the horny epidermis is supposed to be formed by a kind of inspissation of the external surface of the mucous matter. We beg our readers to bear this in mind when they find the term "horny matter" occur.
The attachment of the papillae to the derma is firmer than that to the epidermis, so that if a little force is used, the papillae separate from the latter, but remain in connexion with the former.

The direction of the papillae in the epidermis is oblique, and slightly inclined. Besides a sheath which they get from the derma, the horny matter supplies them also with a sheath, which covers them in the manner of a hood. This is very dense in the heel. The summit of the papillae is not perforated by any opening.

Our authors refer to the structure of the papillae in the whole, which they say resemble those of man so closely as to make the same description applicable to both—as exhibiting their form and structure on an intelligible scale.

The papillae of the whale, then spring from the derma in innumerable quantities, piercing the horny matter like a sieve, or rather making it resemble a series of organ pipes, perforated to give them passage.

Their length varies with the thickness of the skin in the different regions of the same animal, or in animals of different species.

The nervous twigs, though united sometimes in twos and threes on a common base, are always contained in a particular sheath furnished by the horny matter which is moulded on them. In its thickest part, the body of the nerve presents across the neurilema slight undulated striæ, which proceed from the base, become less distinct, indeed become confused, and wind to a kind of button-like termination, where they appear to unite in concentric semi-circles. This surface is smooth and uniform, and has no communication with neighbouring parts. Examination under the microscope, or in any other manner, discovers in the papillae a white dense tissue, more easily torn than broken, analogous in all respects to the nervous. It is not possible to separate into fasciculi the striæ or undulated lines apparent on the exterior; but our authors have distinguished the opening of a nutritious vessel in the interior. In the papillae of man they have found two, which appear to unite and form an arch. These vessels are distinctly perceived, when the papillæ are injected, and divided by a transverse section. Besides this, in the centre of the neurilema or sheath, there appears to be a whitish pulpy substance.

From these circumstances discoverable in the papillæ of the whale, our authors think that it is not possible to entertain a doubt with respect either to the structure or the sensorial functions of these bodies.

The papillæ of the tongue are usually considered as peculiarly adapted for the sense of taste, and, consequently, as essentially nervous. But if the papillæ of the tongue of the ox be carefully
examined, it will be found that they are enclosed in a horny case of greater or less thickness, which must be opposed to the exercise of such a function as that of taste. In the interstices, however, between the papillæ, there exist below a very fine epithelial true nervous papillæ (tiges nerveuses), similar, or nearly similar, to those which have been already described. The number of these latter is considerable, and it is in them, according to our authors, that the sense of taste exclusively resides. The common papillæ of the tongue are thought by them to be rather devoted to the function of touch, and even to that of triturating the finer molecules of the alimentary mass, and to render it, in consequence, of more easy appreciation by the papillæ of taste in their intervals. On this point our authors have not made up their minds, but intend to examine it with more attention hereafter.

On the whole, our authors conclude that it would be difficult to form any opinion but the one they have advanced on the nature of the tactile organ: On the surface of the derma there are elevations and intervening furrows. The former correspond to the nerves; the latter to the sudoriferous and inhalent canals, while the source of the horny matter is at their bottom. If the papillæ are not the organs of touch in the skin, what are?

The mode of termination of the nerves occupies the attention of our authors. We see the nerves under three points of view: 1. In the subcutaneous cellular tissue they have the usual characters of spinal nerves; 2. In the substance of the derma they become soft, flexuous, and capillary; 3. On the external surface of the derma they are transformed into symmetrical papillæ.

The question—What becomes of the neurilema of the nerves, on their entrance into the derma? naturally forms a subject of enquiry. Our authors suppose that the nerves become divested of it, and that it becomes blended with the derma. It is probable, however, that some sort of envelope still invests the nervous matter. What it is, if any, is dubious, nor is it necessary to follow our authors through their analogical speculations on the subject.

The essential organ of touch, then, consists of—

A. A principal part—the nerve of touch, terminating in a blunt point.

B. Of accessory and protecting parts—1, the derma, enclosing the nerve in its interior; 2, the papillary neurilema, furnished by the derma; 3, of a proper sheath, of peculiar and horny tissue; 4, of a fine layer of epidermia, covering the sheath, and indispensable for the exercise of the function of touch.

If any one of these conditions should be absent, or modified in a certain manner, the sense of touch cannot be exerted. It is evident that the derma, the dermic sheath, the proper sheath of
epidermis, are to the nerves of touch what the complicated apparatuses of sight and hearing are to the optic and acoustic nerves.

If we compare the senses of taste and smelling with that of touch, we must perceive a greater delicacy in the apparatus of the former, and a predominance of the horny covering for the nervous terminations and expansions in the latter. Touch, too, is exercised by nervous bundles, enclosed in a tissue which insulates each, whilst the nerves of sight and hearing are expanded at their termination.

Our authors examine the hypotheses which are entertained with regard to the ultimate disposition of the nerves. We need not go into a subject so intricate, and, we may add, so unsatisfactory. Their observations on the papillæ bear out, in some measure, the conclusions of MM. Prévost and Dumas, who have conceived that nerves end in a loop. In the papillæ, the microscope, if it may be trusted, represents the nerves as preserving their filamentous disposition till they arrive at the top of the papillæ with its horny covering, where they form concentric loops.

**On the Diapnogenous Apparatus, and on the Sudoriferous or Hidrophorous Canals.**

This exhalent apparatus occupies the thickness of the derma, and extends from its interior to the most superficial layer of the epidermis, where it presents an opening.

It is composed of a secreting parenchyma and of an excretory duct.

The parenchyma is situated in the thickness of the derma, and is surrounded with numerous capillaries attached to it. The form is that of a sac slightly swollen, from which issues a spiroid duct, which pursues its course in the derma, and issues from it by the infundibulum, or transverse fissure, situated between the papillæ; the duct then proceeds obliquely through the horny substance, in a corkscrew form, to without the epidermis, where its termination is indicated by a slight depression, or kind of pore, which may be remarked on the prominent epidermic lines.

The duct, as it passes through the epidermis, is rounded. The structure closely resembles that of the horny tissue, from which it is difficult to distinguish it. Its spiral form causes it to open externally by a very oblique opening, almost parallel to the plane of the skin; the opening itself closing by the mutual application of the superior and inferior walls of the tube. If we look at a drop of sweat as it exsudes, we shall see it preceded by an elevation of the epidermis, like a pump valve.

If a piece of skin be macerated, so that the epidermis can be
separated from the derma, we may see, with the naked eye, these excretory ducts indefinitely lengthen, like threads of spider's web. These are the spirals unrolled. When examined by the microscope, they present a surface covered with horny matter, which seems disposed in an imbricated manner on a central canal. We may distinguish very well, in this way, the issue of these canals from between the papillæ, and their penetration into the horny matter, which fills the funnel-like depressions between the papillæ. At their exit from the derma, the spiral tubes are accompanied by an inherent vessel, which enters into the infundibular depression. The spiral, filiform tubes, when taken up with a pair of forceps and placed upon a moistened piece of glass, roll up, and form a sort of homogenous mucous mass, elastic and trembling like a piece of jelly. On stirring them, there are detached a great number of irregular polygonal scales. The sweat found in the secreting organ issues through the sinuosities of the excretory duct, which must also give exit to the insensible perspiration. Probably, say our authors, were we to watch the external orifice of one of these ducts when the body is heated in cold weather, we should find it smoke like the pipe of a stove.

This corkscrew disposition of the sudoriferous ducts in man, explains the anomaly of the epidermis transmitting the excretions, and yet appearing imperforate when examined separately. If the membrane is raised in the living body or the dead, the sudoriferous tubes are torn from the derma, retract, and close the opening in the epidermis, the spiral coiling on itself. If the epidermis is detached in several layers, each contains a fragment of the spiral tube, with its two openings, lying almost parallel with the plane of the layer, and not corresponding with one another.

The walls of the tube come into apposition, and in this case, as in that of the entire epidermis, an opening is no longer discoverable.

If the horny layer be sliced off, and if, before arriving at the papillæ, we squeeze the skin between the fingers as we squeeze a piece of orange-peel, we may perceive drops distilling from the pores, which correspond to the infundibuliform depressions between the papillæ, that is, to the situation of the sudoriferous canals.

Another experiment is mentioned by our authors, as evidence of the tubular nature of the spirals. If a hole is made in the horny matter of the heel, in a direction parallel to the plane of the skin, and if a little mercury is poured into it, and if then a thin slice of the exterior of the epidermis is taken off with a razor, and the mercury pressed over with the handle of the scalpel, the metal may be seen issuing by all the sudatory canals in the spot. We see the same thing in the palm of the hand and end of the fingers, when the sweat oozes from them.
Our authors have carefully examined the epidermis of the whale, in order to discover how liquids traverse it from within. Pressure of the fluid out of the sudoriferous tubes, always occasioned the elevation of a little valve of epidermis before it escaped externally. Our authors insist upon these facts, because, although exhalent vessels have been talked of, they have not been sufficiently described nor satisfactorily demonstrated.

After stating and commenting on the various opinions that have been expressed in reference to the sudatory canals, our authors remark that the recent observations of Eickhorn are among the most interesting and most satisfactory. Yet he has committed several mistakes. He believes them, for example, conical, with an infundibuliform opening and of sufficient diameter to admit of the introduction of a hair.

Our authors themselves allow that it is extremely difficult to ascertain the exact disposition of the sudatory canals, and that it has cost them much time and trouble to obtain the knowledge they have communicated on the subject. It is possible that they may hereafter be proved to be in error; but mere criticism, without a careful examination of the skin, and a repetition of experiments, will be insufficient to do so.

**On the Cutaneous Apparatus of Inhalation.**

In order to examine this most satisfactorily, we must remove a thin slice of the epidermis,* taking care to select a soft and white portion. The slice which has been removed is to be placed on a piece of glass with a few drops of water, and after we have satisfied ourselves that there is no extraneous substance along with it, we may proceed to dissect it with curve-pointed instruments.

The inhalent canals appear to be situated below the most superficial layer of the epidermis. They present the form of separate radicals spread out in the horny tissue, where they anastomose several times, and then penetrate the derma by the infundibulum of the papillæ, near the sudoriferous canals. All these vascular trunks, symmetrically arranged in the fissures between the papillæ, which they traverse, communicate in the derma below the papillæ with canals, forming a common plexus. Our authors confess that they have seen this termination of the inhalent canals very seldom; yet they feel confident that they have seen it.

* Our authors consider all external to the derma as epidermis. The most external layer of it, which we are here directed to remove, is the cuticle, or epidermis of most authors. When the most superficial layer of the epidermis is mentioned, the epidermis, of common parlance, is meant.
These vessels are of extreme tenuity, and ramify in the form of loops in a hard, elastic, resisting substance. They themselves break with great facility. When examined in the microscope, their colour is white and silvery, and they are crossed by diaphragmatic septa; a circumstance which establishes their analogy with the veins and the lymphatics. Sometimes they are knotty, at others they are smooth. They may be distinguished with a very weak lens, and even with the naked eye, on scraping the surface of the epidermis. They are occasionally long, and resemble very fine hairs.

In order to perceive the entrance of the inhalent vessels into the derma, we must lightly raise the epidermis. We may then, with the assistance of a magnifier, observe all the hidrophorous canals, accompanied with an inhalent vessel, the two being very intimately united near the derma, although they speedily separate.

With the microscope we may distinguish the difference between the two sets of vessels. The sudoriferous canal is the larger, covered with small imbricated laminae, serpentine in form, and elastic. The inhalent vessel is smooth, silvery, straight or slightly curved, and traversed by a visible central canal, imperfectly interrupted by small septa. If the epidermis is separated too violently from the derma, the inhalent vessels are broken, and only the sudoriferous canals, which are capable of considerable elongation, remain. Another mark of distinction is this: the inhalent vessels have anastomotic ramifications, and a plexiform arrangement, which the sudoriferous canals have not.

Our authors mention an experiment which appears to them to indicate the existence of a direct communication between the vascular capillary system and the inhalent canals. If an injection is thrown into the main artery of a limb, it always stops at the derma. If, then, the skin is removed in thin slices, while pressure is made with a scalpel on the part that has been injected, the inhalent canals may be seen coloured in the horny tissue, as they ramify and anastomose beneath the most superficial epidermic layer. The sudoriferous and inhalent vessels cannot be dissected throughout their entire extent, on account of the resistance offered by the horny tissue, but the one may be seen in fragments by the microscope, and the other entirely by means of injections.

Whatever may be the colour of the horny tissue, the absorbent canals, the nerves, and the sudoriferous canals are always white.

Our authors have been unable to discover external orifices to these inhalent canals. They are disposed to think that fluids are first imbied by the horny tissue, and then absorbed by the inhalents. Our anatomical readers need not be reminded of the
contradictory statements that have been made with respect to the orifices of the lacteal vessels. The difficulty of distinguishing the apertures of the cutaneous inhalents (should there be any,) must be very much greater. Our authors engage deeply in the examination of opinions on the nature of intestinal villosities, and of the lacteal apertures. But we need not follow them.

We may mention, however, the method of injecting the lymphatics of the skin adopted and recommended by our authors. Sometimes they have introduced the mercurial tube into a lymphatic vessel of the leg, and the mercury has run into the cutaneous lymphatics of the groin. This plan frequently fails. In other instances they have introduced the mercurial tube directly into the cutaneous tissue, at the point where they desire to examine the lymphatics, and to be certain of not mistaking the sanguineous capillaries for them; they have previously thrown very fine injections into the arteries.

On the Blennogenous Apparatus—the Source of the Mucous Matter.*

In order to examine this apparatus properly, it is necessary to select recent skin, injected and reddened with its blood. If the derma is white, either naturally or by maceration, nothing can be learnt from it.

The mucous matter of the skin becomes blended, soon after its secretion, with coloring matter, which occasions the various tints of horn, hair, scales, feathers, and so forth. The mucous matter and the horny matter are, as has already been observed, the same, the horny being originally mucous.

On examining the skin from within outwards, we find—

1. In the derma—
   a. A Blennogenous apparatus, composed of a secretory gland, and of a canal which excretes the secreted matter, the mucus, which becomes horny matter by desiccation.
   b. A Chromatogenous apparatus, composed of a secreting parenchyma, and of canals which excrete the product of secretion (squamous corpuscles).

2. External to the derma, and the joint effect of both these apparatuses.
   a. The horny matter, or epidermis.
   b. The hair, feathers, horns, hoofs, &c.

1. The Blennogenous Apparatus.—At the base of the derma we may distinguish small reddish glands, which, under the microscope or a common lens, appear knotted, irregular, and grooved by blood-vessels. They are enveloped in loose cellular membrane, and surrounded by small, transparent, adipose vesi-

* Blennogenous, from Blenna, mucus, and γεννάω, to produce.
cles. From the summit of each of these glands there issues a canal or tube, which traverses the derma, and opens at the bottom of the furrows remarked on it. The tube is invested by a prolongation of the thin cellular membrane which envelopes the gland. Capillary vessels or filaments may be seen adhering to the tube and to the glandular organ, into which our authors have seen a vessel of some size enter. The canals usually form a regular colonnade in the thickness of the derma. Sometimes the glands are placed at unequal heights, and appear to communicate with one another by means of intermediate canals. The ranges of excreting canals correspond to the furrows in the derma; they are perpendicular to the plane of the parenchyma secreting the colouring matter, to which we shall next direct attention.

2. The Chromatogenous Apparatus.*—This is situated on the external surface of the derma, at the bottom of the furrows seen upon it, below and between the rows of papillary eminences. Superficially it gives rise to a great number of short excreting tubes, which end and pour out a peculiar matter at the bottom of the furrows. On its deep surface it is in relation with capillary vessels, and with the excreting canals of the blennogenous glands.

Its structure is areolar, spongy, and tough. The parenchyma itself, as well as its excreting canals, easily redden on account of their vascularity. Here the arterial system stops; this parenchyma forming, in the natural condition of the body, its limit, though the vessels of the papillae, strictly speaking, do advance a little further.

If we lacerate this tissue, we find in it innumerable little filament, from which escape scales or colourless corpuscles in abundance. This is the only reservoir of such scales in the derma, and this parenchymato-glandular tissue may therefore be regarded as a peculiar organ, peculiarly constructed, penetrated by arterial and venous capillaries, and giving rise to excreting canals. The latter terminating at the same point as those of the blennogenous gland, pour into the mucus formed by this gland granules of pigment or colouring matter.

3. Excreted Products of these Apparatuses.—The joint product of the blennogenous and chromatogenous apparatus is the epidermis, or the horny matter. Our authors examine it as it occurs in the heel.

The inferior or deep surface of the entire epidermis presents inequalities which correspond to the inequalities on the external surface of the derma. It exhibits, indeed, a cast of the latter. This surface of the epidermis has received the name of the reticu-

*Chromatogenous, from κραμά, colour and γενέω, to produce.
lar web of Malpighi. Two divisions (cloisons) may be distin-
guished in it. One fills the furrows of the derma, and adheres
to it by prolongations which issue from the excreting blemo-
genous and chromatogenous canals. By this it is that the horny
tissue is produced and renewed. When we attempt to separate
the horny matter from the derma, we always experience a certain
degree of resistance from this attachment to the furrows of the
derma. Sometimes we may perceive the sort of roots sent into
them, but, more commonly, they are indistinguishable, the horny
matter coming off cleanly, as if only laid in the furrows. On
the sides we may observe small holes, which give passage to the
lymphatic vessels.

The other division of the horny layer, named by our authors
inter-papillary, occupies the interval left by the bifid papillæ, and
is prolonged into the infundibuliform depressions around the
sudoriferous and inhalent canals. We may remark on the edges
of these ridges of the horny matter, a lacerated appearance, occa-
sioned by loose fragments of the sudoriferous canals. On the
right and left may be found the holes and sort of sheaths into
which the papillæ penetrate obliquely.

On the external surface of the epidermis there are visible
prominent lines, slightly concentric or parallel, and separated
by grooves. Examined with a lens, these lines present alterna-
tely small papillary eminences and fissures, or slight depressions,
containing the orifices of the hidrophorous canals. A line
contains usually from four to six. It may easily be ascertained
that the prominent lines have an imbricated disposition, so that
in the motions of contraction, more particularly of the hand,
they advance over one another, like the scales of a fish or of a
serpent, whilst in the motions of extension they separate and lay
open the bottom of the grooves. This imbricated appearance is
very manifest where the skin forms folds, as at the bend of the
elbow, the groin, &c.

The horny matter in man is whitish, elastic, transparent, and
highly hygrometric. Its examination is extremely difficult, for
it gives under the scapae, like caoutchouc; when soft it swells,
and nothing can be made out of it, and when dry, it scales off at
the least touch or pressure.

The epidermis of the whale is more adapted for anatomical
study. The horny matter is secreted by a special apparatus,
and organized like false membranes. It seems then to deserve
the designation of tissue, accorded to it by Bichat.

The *epidermic tissue* then of the *whale* is smooth, spongy,
and generally of a deep slate colour. Proceeding from without

* Again we must observe, that epidermic, in our authors' phraseology, signifies all the horny matter above the derma. Cuticle is only its external layer.
inwards, there may be detected in it with the naked eye two layers—one external, and parallel to the plane of the derma; the other consisting of straight fibres, extending perpendicularly between the derma and the external layer. We may see through this dark tissue the summits of the white nervous papillae enveloped in their sheaths. The internal surface is dotted with apertures for the passage of the little papillary cones.

The respective thickness of the two layers is as follows: that of the external or horizontal layer, one line; that of the deep or perpendicular layer, three lines. As the thickness of the derma on the head of the animal is ten lines, the diameter of the whole skin is about fourteen lines.

In order to analyse the epidemic tissue, we must take a perpendicular fibre, and place it under a magnifier, or a slightly moistened glass.

The tissue is thus found to be composed of small scaly imbricated bodies, on a fine cellular web. The scales are readily detached, and stain water black under appearance of granulations. A fibre is found of a series of these scales inserted one into the other. The fibre is elastic and resistant.

The origin of the horny matter may be well seen in the whale, on account of its black colour. It fills all the space unoccupied by the papillae. The black matter is excreted a little prior to its appearance on the outside of the derma, about half a line internal to which point we find it inclosed in a capsule or dermic membrane, at the bottom of which may be remarked little whitish or filamentous projections, which the capsule embraces; these are the excreting canals of the chromatogenous parenchyma.

The horny matter is formed within, and has then almost a mucous consistence. It is protruded from within outwards, and pushes before it the previously-formed layers, which gradually solidify.

Our authors hang a good many observations on these facts; but we may waive the consideration of them, and having ascertained so much of the horny matter of the whale, return to its examination in man.

In order to examine the horny matter in man, the best plan is to place under a magnifier, in a little water, a portion of the most external epidermis, or of the mucus which is formed on the surface of the derma. If we then separate its component parts with the point of a scalpel, we may observe floating, in the midst of the fragments of inhaled vessels and sudoriferous canals, a number of apparently shapeless corpuscles. They are not really amorphous, this appearance depending on the violence employed either breaking them up, or leaving them partially agglutinated.

In general the scales affect the form of an irregular trapezium,
are of a certain thickness, white, transparent, more or less striated, and disposed on a fine areolar web, in an imbricated order. We may easily recognize, say our authors, in the scales, the product of the chromatogenous or colouring organ; and, in the pellucid web which supports them, the mucus of the glandular blenno- 
genous organ.

The horny matter, excreted in the first instance in a fluid mucous state, moulds itself layer by layer on the papillæ, and envelops and protects the sudoriferous canals and the inhalent vessels, acquiring greater density as it becomes placed externally.

The horny tissue in the negro is black every where, excepting on the palm of the hands and the sole of the feet, where there are only slight traces of that colour. Its structure is the same as in the white race of men. On the heel of the negro the scales are colourless. In the rest of the body, the skin, when examined by a magnifier, is not so black as it appears to the naked eye. Over the papillæ it is white, in consequence of the white nervous matter shewing through it. The areolar web which supports the scales is always white.

On the Chromatogenous Apparatus, or Organs of the Secretion and Excretion of the Colouring Matter.

The mucous rete of Malpighi has been regarded as the sole seat of the colouring matter, which was said to be secreted by a vascular net-work, and collected and preserved in a semi-fluid form. Our authors adopt and present a very different view of it.

They have observed that if the skin is black or white, the free border of the scales is coloured black or white. The attached part of the scales and the cellular web into which it is implanted are always white, as are the parts accidentally contained in the epidermis, the nervous papillæ, the sudoriferous canals and the inhalents. The scales then are the only parts in which colour resides. They were led to compare this with the wings of the lepidoptera, and to examine the colouration of flowers. They discover and dwell on both as analogical cases; but we may pass these considerations by.

Their observations on the colouring organs in man are the following:—

1. They presume that the form of the scale plays some part in the production of the phenomenon. Have the negro and the cetacea, or should they have, a scale of similar form? That of the European is trapezoid. The little articulated pieces which compose the petals differ according to the colour they present and the kind of flower to which they belong.

2. The scale is in more or less intimate communication through its pedicle with its secretory organ, and is nourished by
a true circulation. It may therefore be considered as acting in a special manner on the fluid which is in contact with the pedicle by means of the areolar web to which it is attached.

It is certain that the colours are arranged with art in little compartments, so as to produce their optical effect. Our authors presume that the scale of the Negro is different in form from that of the European. Of course it ought to be so, to sustain their speculations. This consideration makes us wonder greatly that they have not found it so; for really, they have seen so much and so minutely that they might as well have seen this too. Indeed it is inexplicable, if they can determine the exact form of the scale in the white man, in which the corpus mucosum itself is seen with comparative difficulty, why they cannot determine the exact form in the black man. They have shown, we think, a little squeamishness in this instance. The coloring matter itself they admit to be formed by the glandular parenchyma to which reference has been previously made.

Thus the epidermis is not mere inorganic matter, or mucus mechanically expelled, but a tissue of complicated organization. It is connected with the important functions of exhalation and absorption, by the property which it possesses of suffering penetration by liquids; and this imbition or endorsmosis would seem to be the rudiment, or simplest degree of absorption in cutaneous and mucous surfaces.

The surface of the skin is marked with lines affecting geometrical figures, which have a fixed relation to the motions of the particular part. Thus, on the pulp of the fingers they form concentric circles—sinuosities in the palm of the hand—lozenge-shaped figures on the wrist, &c. These forms are such as are best adapted for the movements of the parts where they occur. As all the organs placed on the surface of the derma have an oblique direction, the disposition of all the epidermic coverings must of necessity be imbricated. We may remark this in the mode of implantation of the hairs.

If we look back on the anatomy of the skin, we may give the following brief sketch of its functions.

1. The blood carried by the arterial capillaries to the parenchyma that secretes the sweat, and returned from it by the veins, gives rise to the sensible and insensible perspiration.

2. Inhalents imbibe on the surface of the derma and in the epidermis extraneous fluids, and pour them into the lymphatics and the veins.

3. Nerves placed like sentinels on the surface of the body receive the impressions of touch.

4. Horny matter is secreted and moulded around the papillæ and the inhalent and sudoriferous canals. It is part of the apparatus of touch, a means of ornament and of defence, and...
nently hygrometric. By virtue of the latter property it is penetrable more or less, according to its density, by the fluids with which it is brought into contact, and so performs an important part in the function of absorption or imbibition, which it regulates.

5. The derma is the tissue which supports, insulates, and protects the delicate instruments of these various functions.

A short section, or, rather, a few observations are devoted to the Pathology of the Skin. They are not of sufficient importance to detain us. Nor need we enter on the critical exposé of what other authors have written. All that we shall add before quitting the subject are the conclusions of our authors.

They believe they have shewed that:

1. There exists an apparatus of exhalation, composed of hydrophorous or sudoriferous canals, which have a spiral disposition, and open on the surface of the skin by one extremity, while their other extremity is in connexion with a parenchymatous or glandular body, the diapnogenous apparatus, in the derma.

2. The inhalent canals are situated in the mucous body; they appear to be unprovided with orifices at their external extremity.

3. The medium in which these exhalent canals are disseminated is on the external surface of the derma.

4. The mucous matter, which by its induration forms the different epidermic layers, is the product of a particular apparatus; this is composed of a principal organ comparable to a gland and deeply placed in the derma, and of an excreting canal. The whole constitutes the blennogenous apparatus.

5. The epidermis or horny tissue resulting from this secretion and from its mixture with colouring matter, is traversed by the sudoriferous canals. The inhalent canals and the nervous papillæ enter it, but do not open externally.

6. A second apparatus, situated near the superficies of the derma, is devoted to the secretion of the colouring matter or pigment. This apparatus is composed also of minute glands and excreting canals. It constitutes the chromatogenous apparatus.

7. The matter secreted by the latter apparatus is mingled with the horny matter and its dependencies, and colours it.

8. The epidermis resulting from the secretion of the mucous or horny matter, and its mixture with the colouring matter, is disposed in successive layers. From this disposition results the scales of the superficial layer, or the epidermis, of most authors.

9. The apparatus of sensation in the skin, is composed of papillæ or conoidal eminences essentially formed by the extremities of nerves, enveloped in epidermic layers. The nervous filaments arriving at these new sheaths, throw aside their neurilema, and terminate by anastomosing with one another in order to form arches.

10. A blood vessel, very inferior in size to the nerves, penetrates the papillæ.
11. The nervous filaments, although they lose their neurilema when they enter the epidermic sheaths, still preserve a proper membrane.

12. The derma is a fibrous and vascular web, which contains the organs of secretion, and the commencement of their excreting canals, the origin of the exhalent canals, and many lymphatic and sanguineous vessels. The latter are chiefly found on the two surfaces of the derma especially upon its inner, and form numerous networks, constituting a sort of erectile tissue. The blood-vessels do not penetrate the mucous body, and beyond the derma we only observe them in the papillae, where they are delicate, not numerous, and difficult to be distinguished. We may observe, however, by the aid of injection and of magnifying-glasses, lymphatic vessels on the external surface of the derma, in the deepest layers of the mucous body, and around the papillae. They are arranged in networks, rather close; their terminating apertures cannot be recognized.

This closes the memoir before us. It is the first of three, and the only one which has yet appeared. The second will be appropriated to the description of the accessory parts of the skin—hair, wool, feathers, scales, nails, horns, follicles, &c. The third will be occupied with the structure of the mucous membranes, and with physiological observations on the functions of them and of the skin.

It is not necessary for us to pronounce an opinion on this work. None can deny to its authors the merit of great labour, research, and ingenuity. Whether their statements are strictly accurate—whether they have seen more or less than nature offers, we do not feel ourselves competent to determine. Those only can pronounce an opinion, who have subjected the skin to very close microscopical observations, which we have not done.

**Signs of Death.** At the session of the French Academy of Medicine (7 Aug. 1837) a letter was received from M. Donné, who has of late devoted much attention to the microscopic study of animal fluids, stating that the most positive indication of death previous to putrefaction is derived from the change in the form and appearance of the globules of the blood. M. M. Breschet and Magendie were appointed a committee to examine into the subject and report to the Academy.
PART III.

MONTHLY PERISCOPE.

Intermittent Fever treated by the Endermic Method.

A man aged 38, was received into the Hôtel Dieu, on the 21st March. He had an interment fever which had lasted seventeen days; the first attack was caused by witnessing the sudden death of one of his companions. The fever came on towards eleven and lasted till four.

The 25th March, at nine in the morning, two grains of sulphate of quinine were spread on a small blister applied to the epigastrium. The fit returned, but was delayed two hours and a half, and only lasted an hour and a half instead of five hours. Two grains more of sulphate of quinine were applied to the sore. The 27th and 28th March, no attack; sulphate of quinine was however given internally, and there was no further appearance of fever.

Another patient with a similar complaint was admitted into the Hospital the 21st April; two grains of sulphate of quinine were applied the 25th, three hours before the supposed time the attack came on. No fever appeared. Two more grains were applied the following day, and four grains the day after; the medicine was then administered internally, and the man was cured. In several other cases a stronger dose of quinine has been applied, and the effects obtained have been nearly similar. M. Chomel has always concluded by prescribing a few grains of sulphate of quinine to be taken internally.—Med. Chi. Review.

Cancer of the Uterus treated with Acetate of Morphia applied Endermically.

Madame Detty, aged 53 years, having all the attributes of perfect health; married at five and twenty; mother of five children; her labours were always severe; at the birth of the last child forceps were employed, and the infant was still-born. At the age of 51, she had a dartre (herpes) on the left fore-arm; sulphureous baths were ordered, and brought on an irritation in the interior of the womb, and an abundant menorrhagia. The treat-
ment at the hospital Necker proved fruitless. The womb was explored through the speculum, in the course of July, 1824; there was hard scirrhus, unequal congestion on the neck of the uterus, it bled on the slightest pressure; there was a white discharge, with a fetid odour; great difficulty in voiding the urine; the skin was yellow, and the flesh turgid and bloated. The patient grew worse in November, she was in agonies, rolled in her bed, and loudly invoked death as her only relief. Narcotics and the most powerful antispasmodics, strong doses of spirit of morphia, had no effect. The cruel sufferings were only allayed, and the blessing of sleep procured, by putting two grains of acetate of morphia on a seton. The application proved so soothing that her existence was prolonged till the 20th December, 1824, without any further sufferings. This observation proves that in cases where a cure cannot be hoped for, great advantages may nevertheless be found in external applications.—Ibid.

Rheumatism cured by the Application of Acetate of Morphia to a Blistered surface.

A young man, named Choubert, aged 24, a baker by trade, was seized with such violent pains in his limbs, that he could neither move nor sleep. The patient attributed his complaint to excess of fatigue in kneading the bread, intense perspiration, and sudden cold after his hard work. On the fourth day after the appearance of the disease, the pains settled in the shoulder, the arm and left elbow, the limbs affected were perfectly motionless, a continual burning pain, the patient lost his appetite, and his countenance bore an expression of sorrow and suffering.

The 10th March, a blister was applied to the arm, but gave no relief. The 13th, the surface of the blister was covered with half a grain of acetate of morphia. Spent a good night, slight pain, no function disturbed. The 14th and 15th, same medication, same effect. The 16th, one grain of morphia, complete relief, no change in the functions, excepting slight contraction of the eyelid. The 17th and 18th, the applications having been omitted, the patient spent two bad nights, and suffered considerably. The 19th, one grain of morphia, the patient quite calm, but cannot raise his hand. The same means continued until the cure was complete.—Ibid.
On the Action of certain Medicines on the Heart.

Dr. Lombard of Geneva makes the following observations on the action of several of our most common Medicines:

1. Assafœtida. This is one of the most potent remedies against the irregularities of the heart's actions, when they depend upon a nervous or functional cause. Even when there is organic disease present, the internal use of assafœtida is often productive of very decided benefit, by checking, or at least moderating the palpitations, and by inducing a state of calm. When we prescribe the internal use of the medicine, we may administer it either in the form of pill or mixture. Three grains or thereabouts, taken twice or thrice a day, will be a sufficient dose in most cases. The most convenient form of employing it externally is that of a plaster applied over the region of the heart. The formula recommended by Dr. Lombard is as follows:

\[
\text{Assafœtida, sij.} \\
\text{Gum ammoniac, sj.} \\
\text{Turpentine, gutt. vj.} \\
\text{Yellow wax, q. s.—Misce.}
\]

2. Camphor. Given in doses of from three to twelve grains has a very decided influence in moderating any violent action of the heart. It also assists the heart in expelling its contents, when it is overloaded with blood, and cannot easily discharge it at each contraction of its ventricles. Hence in many cases of dilatation, it is of great and very decided benefit. The following observations are so good, that we shall extract them in the author's words:—"This state of discomfort—from the heart not being able to expel the blood from its cavities, in consequence of their imperfect contractions—which is sometimes temporary, sometimes permanent, appears to be properly modified by camphor. A few days, even a few hours, have sufficed under this treatment to regulate the most violent ventricular contractions and shortness of breath, and irregular circulation ceases after the administration of a few grains of camphor. Is the action of this medicine sedative, or stimulating? This is a question on which I cannot presume to decide; but it is evident after the researches I have made on the treatment of diseases of the heart, that care must be taken not to prescribe lowering medicines; and that the heart hypertrophied, but with obstacles at the orifice, or with dilatation of its cavities, must be considered as a muscle fatigued by the continual efforts requisite to maintain an equilibrium between the arrival and departure of this circulatory fluid; so that it should be strengthened, and its weakness counteracted by tonic medicines, and its action regulated by anti-spasmodic stimu-
lants. Thence the indication of steel and quinine in the first case, camphor and asafoetida in the second.

3. Digitalis. The sedative action of this drug is, at best, most uncertain and unsatisfactory. Its successful administration seems to depend upon attention to a multitude of circumstances, such as the state of the stomach, the mode of living, the amount of the dose exhibited, and so forth. If the stomach is in a state of irritation, digitalis seems to exert little or no influence on the the action of the heart. Under these circumstances it very often induces sickness, nausea, and even vomiting. If this disagreeable effect is induced, we should try to arrest it by effervescing draughts; but if these fail, the use of certain antispasmodic, such as aether, oxide of zinc, the nitrate of bismuth, or even opium itself, may be found necessary.

If digitalis is administered with the view of subduing palpitations of the heart, the doses should be rather large, than small and frequently repeated. A grain, for example, of the powdered leaves, or three spoonfuls of an infusion, (a scruple of the leaves to six ounces of water,) may be given three or four times a day. The infusion is certainly the most potent preparation, and the one on which we can best depend. It is however more apt to induce nausea than the dry powder in the form of pills. As respects the corrigents of digitalis, we find the following remark of Dr. Lombard. "What best succeeds to avoid or allay the symptoms of saturation is calcined magnesia, subnitrate of bismuth, subcarbonate of steel, and oxide of zinc. Several English practitioners have prescribed powder of calcined magnesia. I have always employed it with subnitrate of bismuth, so that I am unable to remark on its action when administered alone. Subcarbonate of steel is the best adjuvant of digitalis; to this medicine may be attributed the absence of accidents to persons who have taken digitalis daily, during several months. Oxide of zinc also arrests the symptoms of saturation of digitalis."

4. Polygala Senega. This drug is perhaps too little used by medical men. Taken as an infusion, says Dr. Lombard, it appears to diminish the circulation, and regulate the ventricular contractions.

In persons affected with disease of the heart, dilatation of its cavities, polygala has corrected the irregularity of the heart's pulsations, and has lessened the sanguine stasis which seemed to threaten the dissolution of the patient. The doses given, vary from twelve to twenty-four grains of polygala in the course of the day; an infusion prepared with a drachm, and four ounces of water, has been given in four-and-twenty hours.
Nervous Headache cured by the Inoculation of Morphia.

A woman admitted into the hospital Beaujon, was very subject to nervous headache. On a former occasion she had experienced great relief by the application of a blister to the temple, the blistered surface being afterwards dressed with an ointment containing some muriate of morphia. Dr. M. Solon made trial of the endermic use of morphia in a different form this time. He made eight punctures in the temporal region with a lancet impregnated with a strong solution. The patient was speedily cured, but felt inclined to sleep during the day.

It is chiefly to the researches of MM. Lembert and Lesieur that the profession are indebted for having illustrated the good effects which may be derived from the application of various medicines to the skin deprived of its epidermis. One very great advantage of the practice is, that the most potent remedies may be sometimes used in cases, where the judicious physician might be unwilling to introduce them, even in the most minute quantities, into the stomach or rectum. Moreover the direct application of anodyne medicines to the seat of a painful affection will often succeed, when constitutional or general means fail of relief. It has been very well remarked—Why have medicines always something uncertain in their action? It is because they reach a surface ever varying in its condition, or the intestinal mucous membrane is irritated and covered with mucosities, or perhaps the stomach and bowels are full. In subcutaneous absorption on the other hand, the absorbent vessels (and perhaps also the capillary veins) are always in a free state, when the skin has been recently deprived of its epidermis.

Hitherto the endermic, or, as some call it, the inoculative method of treating diseases, has not been much attended to in this country. The most potent and available remedies are perhaps the vegetable alkalies; more especially the salts of morphia, strychnine, veratrine, &c.

We are indebted for the facts in this article to the late numbers of the Continental and British Medical Review, edited by Dr. Roffrey, of which we have already spoken favorably. We trust that Dr. R. meets with the encouragement, which he deserves.—Ibid.