Gulstonian Lectures

ON

THE USES OF BLOOD-LETTING IN DISEASE.

Delivered at the Royal College of Physicians.

BY

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MR. PRESIDENT AND GENTLEMEN,—I should have hesitated in bringing under the notice of this learned assemblage, so commonplace—I may say, so trite—a subject as is that of Venesection—the effects of the abstraction of blood in diseases—had I not, sir, received your sanction in doing so. What especially influences me in selecting such a topic as the subject matter of these discourses, is the very strong conviction, founded on a careful consideration of the whole question, which I hold—that the practice of the present day, in respect of the abstraction of blood in diseases, is not unfrequently prejudicial to the interests of the sick; and that it is neither in accordance with the opinions of the profession at large, nor with a scientific consideration of the value of venesection as a remedy. A strange influence, whose source and progress it is not easy to trace, has—as we are all well aware—in these latter days, quietly, and I may say irresistibly, pervaded and passed into the practice of medicine; forcing upon men's actions, if not upon their convictions, the conclusion, that abstraction of blood in disease is, at this period of the world's history, *pro tanto*, something akin to the abstraction of life from the patient's body.

This revolution in therapeutics, which, amidst the many remarkable revolutions in treatment which this day witnesses, may still perhaps be called extraordinary, has, to all appearance, been accepted and acquiesced in by the profession, almost without a question. So far, indeed, from doubting its propriety, men have busied themselves rather in attempting to find excuses for, or explanations of, the change in practice, than in questioning and arguing as to its fitness. And the result is that, in respect of the use of bleeding, the most powerful of therapeutical agents, we find ourselves placed in this position: that our practice is directly opposed to the lessons handed down to us for our instruction, as the results of the experience of all the great master-minds who have ever practised medicine in the past; opposed also to the recorded teachings of all our great modern medical authorities—the authorities of the day; and, what is still more remarkable, opposed also, in a very considerable degree, to the belief and convictions of many who adopt the negative practice solely under obedience to the imperious force of custom.

Moreover, it is to be noted, that this unnatural dread of the evil consequences of bleeding is not now confined to men of medicine. It has, through our teachings and practice, taken firm hold on the public mind;

so that, in truth, patients, under the bias of this new idea, have imbibed as great a horror of being bled as doctors have of bleeding them. The result of all this is, that the practitioner, whose experience and judgment might have led him, in a given emergency, to perform venesection, not unfrequently withholds his hands; dreading lest, should his patient die, the *onus* of the death may be put to the account of his fatal lancet. There are few of us, indeed, who, when the question of the use of venesection as a desirable remedy in any given case comes before us, do not find ourselves under the biasing influence of this anti-venesection epidemic, and allow it to exercise an undue sway over our judgment.

If such be the position of this matter, may I not reasonably ask you to consider with me, whether there may not be some fallacy in the arguments which have brought us to such a remarkable conclusion? Surely, it is hardly credible that a remedy—undeniably the most rapid and powerfully telling in action of all remedies—which has, from the earliest and during all ages of medicine, been ever found a sovereign remedy in the hands of the greatest masters of our art—should have, all at once, become something worse than useless in our hands. Granting even, for the moment, the truth of the idea that some great change has of late years come over the constitution of human bodies; that this man of modern civilisation has undergone some mysterious constitutional modifications; that diseased entities, imaginary or otherwise, or the bodies wherein they manifest themselves, have changed their type; still, surely, ought this remedy, like every other standard remedy (if it were ever an useful therapeutical agent) to hold its own as such. Reason seems to plainly tell us that, so long as this tangible body of man, with all its component parts, remains the same—the same in structure, the same in vital manifestations, the same in its functional operations, the same in its diseases—so long must the effects of the loss of blood upon it in health and in disease be ever the same in kind. Whatever be the nature of those supposed altered conditions of the body, which are represented to us in this change-of-type idea, still the agencies which move the animal economy, and the parts of it, must be ever the same in kind. The kind of effect, consequently, produced by bloodletting now, should be precisely similar to what it has ever been—in those other days when bloodletting was admittedly a princely remedy of diseased humanity. The influence of the remedy must be ever the same in quality; and, if changed at all, changed only in respect of the quantity of blood that requires abstraction for the cure of disease.

But, in truth, sir, from whatever point of view the subject is regarded, the conclusion seems to me inevitable; viz., that blood-letting does not hold in our estimation its rightful position as a therapeutical agent; and that, therefore, and in so far, the interests of the sick are prejudiced. That such is really the case, I hope to be able to satisfy you; and the strong, and I trust rational, feeling which I hold on the subject, must be my apology for discussing this question here. And I may express a hope, that the discussion of the subject within these walls will perhaps induce the profession at large to re-consider the position which blood-letting at present holds in therapeutics. I cannot but think, that a calm survey of the causes of this modern decline and fall of venesection in professional estimation—a nearer consideration of the

actual effects produced by venesection, and, I might add, a truer reading of the lessons taught by modern physiology—will be found to demonstrate, that the practical conclusion adopted by us is not justified by sound premises.

With this object in view, I will, therefore, in the first place, briefly relate the causes, real or supposed, which have brought about this modern revolution in practice, and examine their reasonableness. And having done this, I will then inquire more particularly what are the actual effects over diseases produced by the loss of blood; and shall attempt to deduce from such considerations the conclusion, that blood-letting, rightly applied, is now, as it ever has been, a good remedy in diseases.

The first cause which presents itself for our consideration, as explanatory of this present bloodless epoch of medical practice, is the *change in type* which diseases are said to have undergone; and undergone, indeed, as some assure us, even while the present generation of doctors has been passing from youth to advanced age. This explanation has been accepted by some of the very highest authorities in our profession, and therefore demands our most careful and respectful consideration. With the profession at large, I believe the idea has been admitted without any very critical inquiry as to its merits.

And, *in limine*, without wishing to prejudice the theory, I cannot help remarking that, unless I am much mistaken, this theory has been called in, I will not say as a *Deus ex machina* to explain the difficulty, but certainly after the difficulty had presented itself for explanation. I look in vain for any proof of the assertion, that doctors left off bleeding because they began to note that their patient's bodies would no longer suffer the bleeding as in former days.

Bleeding, indeed, as an universal and infallible remedy in all diseases, appears to have begun its fall into disrepute—I mean its present fall—at the very time when our art, under the enlightenment of modern scientific investigation, was emerging from what I would call the dark ages of medicine. And I, therefore, cannot help calling your notice to the suspicious circumstance, that the asserted facts on which this change-of-type theory is founded—the disrepute of wholesale blood-lettings, the departure of the antiquated humoral theory, and the life of our modern scientific medicine—apparently all occurred at a somewhat similar period of the history of medicine. I think, therefore, that even at the threshold of the inquiry, the theory may be fairly regarded with some degree of suspicion.

I know not to whom is due the credit of first suggesting the term of change of type in diseases, nor when it first appeared in medical literature; nor do I know what was the precise idea attached to it by its inventor; but by the term is meant, as I understand it, not any imaginative alteration in the essential nature of diseases themselves, but some change in the condition of the body which is the subject of diseases. Inflammations, I suppose it would be argued, are the same now, in so far as their characters, anatomical and dynamical, are concerned, as they ever were since diseases fell on man; but the body in which they manifest themselves is somehow changed. The general diathesis of humanity has undergone some gradual transformation, so that the manifestations of the reactions of diseases upon mankind are different now from what they were in former days.

Moreover, this change, some supporters of the theory tell us, is no partial one; the whole civilised human family has come under the novel phasis. Not only here, but at the antipodes, men will no longer bear the bleedings to which they were once beneficially subjected. And I believe that even veterinary surgeons have found, or have thought to find, the same to be true of the constitution of the animals subjected to their care.

Besides this, it is not to be imagined that now, for the first time in the history of nosology, such peculiar modifications have occurred in man's nature. The course of diseases amongst mankind, authority assures us, has ever, at different epochs, been marked by variableness and shadows of change. To use the words of the authority referred to: "There are waves of time, through which the sthenic and asthenic characters of disease prevail in succession, and we are at present living amid one of its adynamic phases." "I share in the belief" (he adds) "which has grown out of the experience of many thoughtful and observant men, that, in this country at least, the human constitution has for several years been suffering a gradual change; that almost all inflammatory disorders assume now-a-days a more adynamic type, and require less energetic treatment than in the early part of the present century."

It must also be remembered, that this supposed change in constitution is something quite distinct from the changes in man's bodily state which result from any of the well defined influences of occupation or climate to which he may be accidentally subjected. This peculiar change is, in fact, the resultant of an influence—an unknown influence—totally distinct from those ordinary influences, whose good or evil operations on the body we can note and appreciate; and, as I understand it, it has fallen alike upon all—upon the ruddy rustic, the corpulent alderman, the thin, sallow, anxious-faced man of business, and the pallid artisan; affecting each of them, and more and less, according to the varying merits of their constitutions.

Let me now ask you to consider the grounds upon which is based this change-of-type theory. Have we, indeed, good reason to believe that the human constitution has undergone some decided and inexplicable modification—the modification being manifested to us in an alteration of the effects produced by venesection in inflammations? for you will remember, that the entire basis of the argument in favour of the theory rests upon the assumed conclusion, that venesection produces different effects now from what it formerly did.

In approaching the subject argumentatively, we at once perceive that the theory rests on very unsatisfactory evidence; that it, in fact, is founded mainly on what I may be permitted to call the fluctuating basis of personal opinion, rather than upon clear and tangible facts. Consider what difficult problems we have to satisfy before we can rightly accept the conclusion involved in this change-of-type theory. The theory rests upon the assumption, that the effects of blood-letting now are different from what they were in other days—rests consequently upon a comparative estimate of blood-letting at different and distant periods of time. Could a more difficult question be proposed for solution? To answer it, we have, in the first place, to determine absolutely what were the effects of blood-letting in certain given diseases at

different periods—in the past and at the present time. We have, in truth, as it seems to me, to compare things which are utterly incapable of any fair comparison; viz., the practice of men of medicine of the past generation with the practice of these times. Or, again, we have to compare the results of our own practice at long and distant intervals of time. We have to satisfy ourselves that the remedy which was a good one in the hands of our forefathers is a bad one—or rather no remedy at all—now; that the bleedings of twenty-five and fifty years ago were good, useful, and justifiable bleedings, and are neither good, useful, nor justifiable now. And we have to do all this out of a comparison of the recorded practice of past days with the practice of the present day; or out of a recollection of what happened to us in our treatment of diseases when we were a quarter of a century and more younger in ideas and younger in experience than we now are, and, of course, completely innocent of all the important facts and theories which the last quarter or half-century has added to our knowledge. No easy task this, I venture to think; and especially when attempted at this present age of fluctuating therapeutical opinions.

It must be remembered, as I have said, that the proof of this change in type of disease rests mainly upon the assumption—rather is, in fact, deduced from the circumstance—that men did bleed in other days, and that they do not bleed now; and that, in both cases, the practice was equally right. But how is it possible for us to compare to any useful purpose the curative (the bleeding) practice of men who lived half a century and more ago with the practice of men of the present day? Where are the data for such a comparison? The clinical medicine, the diagnosis, the pathology, and the therapeutics, of Cullen and Gregory, are not the clinical medicine, the diagnosis, the pathology, and the therapeutics, of this present day. Where are the tangible records left to us by them, from which we may draw anything like a reasonable comparison between what were the effects of bleeding in their hands, and what they are in our own? Where are the clinical records left by Cullen and Gregory to which we can refer, as to tests of the real value of their practice? At this period of day, when our whole therapeutical proceedings are subjected to the rudest shocks—are, I may say, being revolutionised—is it possible to admit, that the mere use or disuse of a remedy at different epochs of time can be any real or proper test of its merits; and especially when the real virtues of the remedy have ever been, and still actually are, matter of dispute? In such a difficult case as this, and in such a fluctuating business, as is the treatment of disease—ever so much a matter of personal opinion—surely, except on the very strongest proofs, no one can reasonably ask us to repose unlimited confidence in the mere assertions even of the very highest authorities who have adorned the practice of our most fallible art. But it is, in truth, on the strength of mere assertion, that the change-of-type theory is founded. And if so, then it follows, that the practice of bleeding, said to have been so largely and universally adopted by those great men in past days, affords in itself no proof demonstrative of the actual value of the remedy, or of its proper application in all the cases in which it was employed by them; any more than does the general abstinence from venesection which characterises the present day absolutely prove its inutility as a remedy now.

And here, perhaps, I may usefully draw an illustration from the pages of an authority highly esteemed by all of us, to show how dangerous it is to adopt conclusions as to the value of a remedy from the mere fact of its being largely employed and highly extolled by a master of our art. Had Andral, like Cullen and Gregory, left us no clinical records of his practice, but merely the general assertion of his admiration of blood-letting as a remedy in acute inflammations of the chest, his authority, like theirs, might have been quoted in favour of the theory that diseases have changed in type. Andral asserted, some forty years ago, that, of all remedies in pneumonia, there was none to compare with blood-letting. The experience of ages, he tells us, has taught physicians to be more prodigal in the abstracting of blood in this disease than in any other. There is no period of the disease, no condition of the pulse, no debilitated state of the system, no age, in which the remedy may not be used. Such was the conclusion which his experience led him to entertain of the value of blood-letting.

And now let us see the results of his practice, as given in his truthful and admirable pages. He records sixty-five cases of pneumonia; and of these, thirty-six, or more than one-half, died. Of nine uncomplicated cases of pneumonia, which reached only the first stage of congestion, two (or about one in four) died. Of thirteen who reached the second stage, five (or about one in two and a half) died. Of seven in the third stage, gray hepatisation, all died; and of thirty-six complicated cases, twenty-two died. I need not stop to say, how little Andral's practice justifies his eulogium of blood-letting. But we may surely judge from this fact of the value of Cullen's, Gregory's, and other opinions, upon which Dr. Alison laid so much stress, in this matter. They, like Andral, bore the warmest testimony to the excellent effects of blood-letting in inflammations; but, unlike Andral, they have left no record whereby to test the worth of their conclusions. But why should we trust their recorded assertions any more than the recorded assertions of Andral? Why may not they, equally as Andral, have misinterpreted the effects of bleeding, the results of their own practice? I have referred to this instance in Andral, not for the purpose of showing that blood-letting rightly used is an improper remedy in pneumonia, but solely to show how ill sustained this theory may be when supported on the assertion of mere authority. Andral's fatal practice, in my view, proves only the abuse and bad application of a most excellent remedy in pneumonia.

Another useful illustration of the difficulty of drawing satisfactory conclusions concerning the change of type in diseases from a comparative consideration of the treatment adopted by our forefathers and by ourselves, may be obtained from the history of the treatment of acute rheumatism. Here we have an acute inflammatory disease, striking and simple in its aspects, which must have been as familiar to our forefathers as it is to us—requiring none of our modern methods of diagnosis for its determination; moreover, a disease bearing with it, in a very marked degree, the characters to which the idea of phlogistic especially appertains; and which, in truth, has in past times ever been regarded as an inflammation demanding, in an especial manner, the free use of antiphlogistic remedies for its cure. But who, at this day, will venture to affirm, from the

mere fact that such was the practice of the greatest authorities in the past, that venesection was ever, at any time, a right and proper remedy in acute rheumatism? I suppose no one thinks it necessary in this case to call in the change-of-type theory to account for the change of practice; though it is hard to see in what particular the case differs from that of pneumonia, which is so especially used as illustrative of the change-of-type theory. The purer pathology and the better knowledge of the nature of diseases which belong to the medicine of this day inevitably bring us to the conclusion, that venesection neither is, nor ever could have been, the right remedy in this disease.

In this case, then, of acute rheumatism, there is no pretence for concluding, from the fact of the change in its treatment, that the disease has undergone a change in type. We know, indeed, that blood-letting neither is, nor ever could have been, a right remedy for the cure of acute rheumatism, even though there are present in it all those signs which, as we are assured, incontestably indicated the propriety of blood-letting to Cullen and Gregory and their predecessors.

Gregory and his predecessors, it has been argued by Dr. Alison, might have been ignorant of the exact condition of the thoracic organs of their patients—whether, in fact, they had to deal with pneumonia, or pleurisy, or pericarditis, or other inflammation of parts within the thorax; but the state of the pulse, and the tongue, and the fever, and the abstracted blood, were facts which they could appreciate as well as we; and that, therefore, they knew when to bleed as well as we do. But, surely, those who thus argue will not assert, that those things—the hard, full pulse, and the coated tongue, and the high fever, etc., of acute rheumatism—were a justification of blood-letting in this disease. And if so, then it clearly follows that the main argument—viz., the propriety of the practice of Cullen and Gregory—upon which is based the theory, that the diseases of humanity in the time of Cullen and of Gregory required blood-letting for their cure, but do not require it now, falls to the ground.

Another illustration of the position I am asserting, may be drawn from the history of the treatment of fevers. There have been times when blood-letting has been a very favourite remedy in the treatment of these diseases; and, in truth, never was it more so than during the first thirty years of the present century—the epoch which is connected in this wise with the names of Armstrong, and Clutterbuck, and Bateman. Never, indeed, has more blood flowed from the veins and arteries of mankind, under the authority of medicine, than during the first quarter of the present century; during the time, remember, when “the asthenic tide” was rolling in upon us. Let me give you an example of what was done about forty years ago, and in the words of a living authority. “I remember,” says Dr. Stokes, “when I was a student of the old Meath Hospital, there was hardly a morning that some twenty or thirty unfortunate creatures were not phlebotomised largely. The floor was running with blood; it was difficult to cross the prescribing hall for fear of slipping. Patients were seen wallowing in their own blood, like leeches after a salt emetic.” Dr. Rush tells us that ninety ounces were at one sitting taken from his friend Dr. Dewees, and of course with advantage. Dr. Dewees, again,

on his part, took eighty ounces from a young delicate woman in puerperal convulsions; and from another like woman, under similar circumstances, he drew 120 ounces within five or six hours, and twenty ounces more on the next day. This patient, however, lost her sight for a fortnight, and did not recover for six months; but do not (says Dr. Clutterbuck, who tells the tale to his students) hastily conclude that the loss of blood caused the blindness; “a much more rational cause is to be found in the affection of the brain which caused the convulsions.” Dr. James Currie of Liverpool was bled to 200 ounces between October and May, and was rescued from pulmonary consumption, says the same author; but he nevertheless died eventually of the same. Another physician gives us the case of a boy suffering from diabetes, from whom 209 ounces (thirteen pounds) were taken in the course of fifty-one days. A taste of his own practice is also given us by Dr. Clutterbuck. In 1836, he visited a Mrs. M., in consultation, who had suffered from various attacks of inflammation. The uterus, the chest, and latterly the brain, had all been in turn violently affected, “so as to call for frequently repeated blood-letting, which always produced great, though only temporary, relief.” A minute account of this lady's loss of blood had been kept; and the sum total is worth note, considering it was taken during the so-called asthenic phase of disease. Fifty-eight was the number of her bleedings in the arms; five times she had been cupped; and at least 1000 leeches had been applied to her body. Dr. Clutterbuck considered himself a very cautious practitioner; and, therefore, has to draw illustrations of what he considered boldness in the use of the lancet outside his own practice. In one of our great hospitals lately, he says, a case occurred where 128 ounces (eight pounds) were drawn at one time, in order, by inducing syncope, to facilitate the reduction of a dislocated thigh. The patient died in a week after the blood-letting; of course, not from loss of blood, but from inflammation of the vein.

I need not occupy your time by giving you other illustrations of the fact, that blood-letting saw its palmiest days during the period I am speaking of—a supposed asthenic phase of time. But does the fact of the frequency of its use in fevers, in any way prove, or satisfy you, that blood-letting was a proper remedy in fevers? Is it not almost clear as the simplest demonstration, that we do not bleed in fevers because our pathological views of their nature show the intility, and worse than intility, of the practice? Is it not certain, that Clutterbuck, and Armstrong, and their numerous disciples, bled because their pathological views led them to see in local inflammations the essential characters of fever? Are we not all ready to predict that, whatever be the future ebbs and flows of diseases in their sthenic and asthenic aspects, never again will blood-letting be a proper remedy in fevers? We recognise, what neither Armstrong nor those others did, the essential nature of these diseases; we see that they have a prescribed course to run; and that to take the whole mass of the blood out of the body would not remove the disease out of it. As these fevers are, so have they ever been, and so may we conclude that they will ever be. True, we do read of wonderful benefits which have attended blood-letting in fevers; but these supposed benefits were obtained by comparing the results of the practice at different periods, by

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

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

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

[To be continued.]

Illustrations

OF

HOSPITAL PRACTICE:

METROPOLITAN AND PROVINCIAL.

ROYAL PORTSMOUTH, PORTSEA, AND
GOSPORT HOSPITAL.

LOCK WARD, No. II.

CASE OF PHAGEDENIC ULCERATION: USE OF
MERCURY.

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

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

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

EPITHELIAL CANCER OF PENIS AND SCROTUM;
REMOVAL: RECOVERY.

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

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

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

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

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

Original Communications.

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

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

[Concluded from page 312.]

THE effects of treatment are worthy of mention.

In the tinea, if the parasite be destroyed, a cure is effected without the use of any internal remedies. It is so in herpes circinatus, in many cases of tinea tonsurans, and in pityriasis versicolor, for instance. Depilation acts in the same way by the removal of the parasite in part, and more especially by enabling us to reach the fungous elements more easily with parasitocides. Mr. Wilson admits that depilation "disposes to the cure of the local disease"; and adds: "But we do and must disbelieve that the removal of the fungus is the proper explanation of the cure." It is partly by removal, and partly by affording facility of access to the fungus; that is the true *modus operandi* of the operation. "We see," says Mr. Wilson, "in depilation a stimulant, and a valuable stimulant," etc.; and "we have been led to regard avulsion as one of the best, indeed, our only reliable remedy, for producing *deep stimulation of the cutaneous tissues*, for setting up a new action in the papillæ of the hair; as the only remedy, in fact, by which we can reach successfully the fundus of the hair-follicle." "It sets up a new action, a healthful inflammatory process, in place of a morbid inflammatory process—a process whose natural course is to end in resolution, instead of one whose nature is perpetuity. For morbid cell-germs, it establishes healthy and plastic inflammation, and sometimes healthy suppuration; when, as Bazin declares, the fungus is starved or drowned." Now, in the very worst forms of the disease, the hairs are perfectly loose, and can be even readily combed out; hence avulsion can do nothing here by stimulation, only by removing the sporules. Indeed, if there be a point of practical importance in the treatment of parasitic disease, it is the removal of all "dark stubs", which are quite loose, but which are literally crammed with "germs" of disease. How the mere removal of a hair from its follicle can set up a healthy process, and alter *in toto* the particular form of nutrition upon which depends the morbid cell-genesis, is a problem which would be indeed difficult to prove; it is curious pathology. The depilation is followed by a reparative action, by which the hair is reproduced in integrity, upon the same principle that all local injuries are remedied. "Even