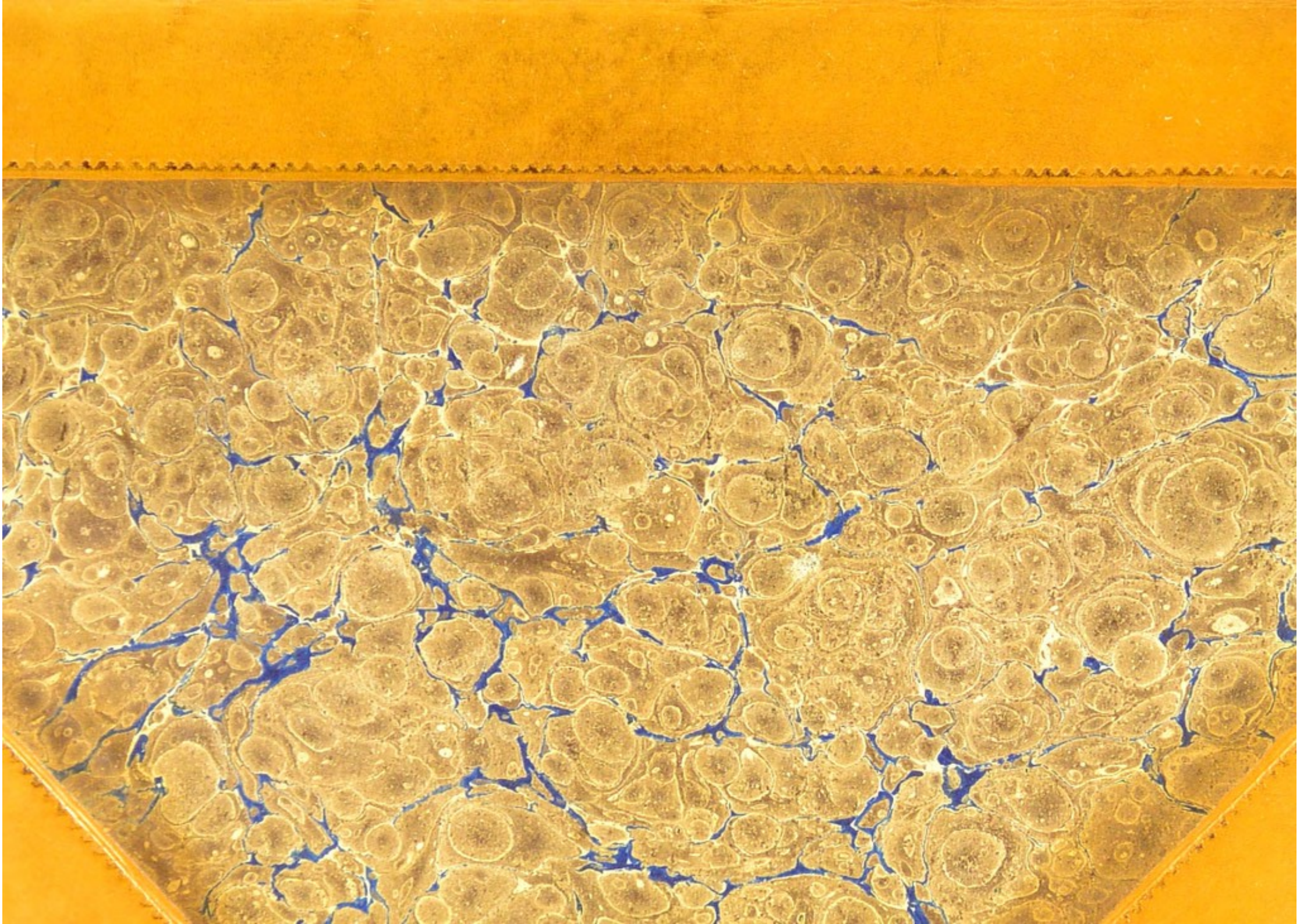
Bleeding and change in type of diseases : being the Gulstonian lectures for 1864 / by W.O. Markham.



BLEEDING

AND

CHANGE IN TYPE OF DISEASES.

BLEEDING

AND

CHANGE IN TYPE OF DISEASES.

BEING

The Gulstonian Lectures

FOR 1864.

BY

W. O. MARKHAM, M.D., F.R.C.P.

PHYSICIAN TO ST. MARY'S HOSPITAL.

LONDON:

JOHN CHURCHILL & SONS,

NEW BURLINGTON STREET.

MDCCCLXVI.

LONDON: T. RICHARDS, 37 GREAT QUEEN STREET.

TO

SIR THOMAS WATSON, BART.,

M.D., F.R.S., ETC.,

These Lectures

ARE DEDICATED

BY HIS KIND PERMISSION.

PREFACE.

THESE Lectures, which I had the honour of delivering before the College of Physicians in 1864, have already appeared in the *British Medical Journal*.

I now produce them in a more distinct form, because I believe they prove that the present rigorous exclusion of bloodletting from medical practice is founded on error and is a misfortune to suffering humanity. Moreover, as I venture to think, they contain arguments from which may be derived a rational estimate of the Uses and Effects and Right Application of Venesection in Disease.

In shunning the fatal extreme of profuse bloodletting, which characterised the practice of former

days, medical men of the present generation have, as it appears to me, allowed themselves to be carried away into the other extreme of an unreasonable neglect of the remedy. Modern practice, indeed—reversing the universal verdict of the past experience of medicine—seems to regard venesection rather as an unmitigated evil, to be carefully shunned, than as a remedial agent.

The explanation given of this remarkable change in practice, is that diseases have changed their type; and the theory, at once simple and fascinating, and, if true, sufficiently explanatory of the apparent contradiction, has been unhesitatingly accepted by the profession at large. Few persons, however, have cared to question the validity of the grounds upon which the theory is based, or even to define the absolute meaning of the idea involved in the term.

But such an explanation, so long as it holds possession of the medical mind, stands an evident stumbling-block in the way of a consideration of the uses of venesection. Men will argue, and

reasonably, that it is sheer waste of time to discuss the value of a remedy, of which human nature will not bear the application.

To clear the ground, therefore, for an enquiry into the uses of bloodletting, it is necessary to show the fallacy, if so it be, of the change of type theory of disease; the fallacy of the idea, that at different epochs, and under the potency of some intangible and mysterious agency, human bodies assume different conditions of sthenia and of asthenia—different states of par and impar. And this is what I have here attempted to do.

I know not how far the arguments which I have adduced may succeed in convincing others as they have convinced me; but this I can assuredly say, that no answer has yet been given to the objections here urged against the theory. Indeed, I cannot but express regret that so high and respected an authority as Professor Stokes, whilst expressly dealing with this subject, should have passed by those objections in silence. Professor Stokes, in his eloquent address delivered before the British Medical Association, at Leamington,

in 1865, did me the honour to refer in flattering terms to these Lectures; but he did not test the truth of the change of type theory by trying the worth of the reasonings which I had employed to show its fallacy. Professor Stokes was contented, as others have been before him, to rest his faith in the theory mainly on the basis of authoritative medical opinion. The highest minds in medicine, it is argued, practised bleeding in other days, and their practice must have been right. Their clear conception of diseases, and of the effects of remedies, necessarily preserved them from error in so plain a case. And if the highest minds in medicine at this day do not practise bleeding, their practice doubtless is equally well founded; and for a similar reason. To have bled therefore in the past was a proper practice, and not to bleed in the present is proper also. The past and present practice is well founded, resting on the infallible basis of the highest professional judgments; exercised at different times, and engaged with matters wherein they could not have been misled. Consequently, in the fact that men

did bear bleeding in other days, and that men do not bear bleeding now, we have the proof that diseases must have changed their type.

But, surely we are not bound, as Professor Stokes's argument would indicate, blindly to accept the opinions of the highest medical authorities unless those opinions will stand the test of arguments such as modern scientific knowledge can bring to bear upon them. Surely we may have the deepest reverence for those who have adorned and who adorn our art, and yet venture respectfully to criticise the justness of their views. The opinions of the very highest authorities cannot be accepted if they stand opposed to facts, or if they will not bear the force of a reasonable criticism. And as the change-of-type theory appears to me incapable of bearing the test of criticism, and to be opposed to facts, I may surely be excused for refusing to accept it, so long as arguments, which seem to me to prove its fallacy, remain unanswered.

Having shown, as I think, that the change of type theory is untenable, and having thus cleared

the way for a discussion of the value of venesection as a remedy; I have in the next place pointed out what seem to me to be the real effects produced by loss of blood in diseases; and in this way have been led to suggest conclusions respecting the right application of the remedy in diseases.

W. O. Markham.

London, June 1866.

Having occasionally during past years heard remarks by Dr. Watson on the change-of-type theory of disease, which led me to think he might have in some degree modified his views, I ventured to address a question to him on the subject. With his usual consideration and high candour, he at once both gave me his present opinion, and also most generously permitted me to publish it here. It would be superfluous for me to anticipate the influence which the opinion of our highest medical authority must exert in the settlement of the question in dispute.

W. O. M.

"16, Henrietta Street, Cavendish Square,
April 19th, 1866.

"My dear Dr. Markham,—Since I listened to your Gulstonian Lectures on Venesection, and especially since you told me of your purpose to publish them in a book, I have felt it to be my bounden duty, as a former teacher of medicine, to re-examine and consider afresh the collateral question discussed in them, respecting the so-called 'change of type' in diseases.

"By that phrase I mean some change in the human body, existing through considerable spaces or cycles of time, which renders it varyingly affected by the causes and by the remedies of disease, and especially of febrile and inflammatory diseases; so that diseases nominally the same shall during one period express themselves in the body more strongly, and during another succeeding period more feebly, and shall accordingly require and bear, now more and now less, of what is called energetic, active, depleting, or lowering treatment.

"That this kind of difference, arising from some obscure outward influence—atmospheric, telluric, magnetic, social, or what not—may really be seen in different epidemics of the same disease, will be admitted, I suppose, by all men

who have had opportunities of noticing the phenomena of epidemic distempers. But I am obliged to confess that the result of my later reading and inquiries, and of careful reflection on the matter, is that my previous belief in the soundness of the doctrine of a general and more abiding variation of type, in the sense now explained, has been shaken.

"In making this confession I desire to protest against the uncharitable imputation (not made by you, nor especially against myself, but thrown out somewhere, as I gather from Dr. Stokes's address before the British Medical Association) against those who hold or have held the controverted doctrine—the imputation that it was artfully invented to conceal former errors of practice in regard to bloodletting.

"Indeed, though I believe that great errors were committed in past years by excess in bleeding, as at the present time by its utter neglect, my own conscience is not uneasy on that score, for I have never been a lavish or a frequent bleeder. I taught, no doubt, because I believed in them, the lessons which I had received from my predecessors, and I endeavoured to explain to my class how it was that my practice was apparently so little in accordance with my public teaching. In the first edition of my lectures I say :—

"'Those among you who happen to be attending the wards of the Middlesex Hospital may wonder indeed, after hearing my estimate of the power of bloodletting over inflammation, that I so seldom prescribe venesection there. The truth is, not that I undervalue the remedy, but that the time for its employment has generally gone by. The poor are unwilling to relinquish the occupations by which they subsist; they struggle on as long as they can, and resort to hospitals only when they are compelled to do so by the exigency of their malady. Many of them, labouring under inflammation, have been freely bled before admission. It is commonly too late when they present themselves to expect that the course of the disease can be so arrested. The first effect of bloodletting is to deplete and relieve the labouring circulation. But when it is again and again repeated, it becomes (as the French say) *spoliative;* it robs the vital fluid of its nutrient and plastic materials, etc., etc.' (vol. i, p. 217.)

"A careful survey of the facts and arguments adduced on both sides of late, respecting the alleged change of type, compels me, I say, to suspect that my previous opinion was a mistaken one. In that survey it was needful for me, looking back, to trace, if I could, what were the

sources of that opinion; and the retrospect has shown me, I think, the main causes to which it owed its origin and strength.

"First, then, I repeat, I had faith in the judgment of the practisers and teachers of medicine at whose feet I had sat as a learner, that the old fashion of free bleeding in certain inflammatory and other febrile diseases was a right practice.

"But as the field of actual observation and experience enlarged itself before me, I soon found that those symptoms and conditions which I had been taught to regard as the warrant for free bloodletting, very rarely presented themselves; and this fact gave birth to a notion, vague enough at first, that the inhabitants of our great and crowded metropolis, and especially the class of persons who form the bulk of the in-patients in our London hospitals, among whom my experience then chiefly lay, had somehow become less likely to receive benefit from, and less able to sustain, the active use of what are spoken of as lowering remedies.

"About the same time came the teachings of the desolating visitation of influenza (the first that I had seen) in 1833, the year after the first outbreak of cholera in this country.

"The cautions enforced by the influenza, as well as its name, outlived the occasion. The ab-

stinence from depressing measures, inculcated by that depressing disorder, was continued to cognate and extended to other disorders, and this by general consent; and thus men learned the safety and the wisdom of a less heroic treatment of disease in general, and thus the doctrine now called change of type got plausible support; but I believe that the main ground for that doctrine was furnished by the differing behaviour under medical treatment of different epidemics of fevers.

"My dear friend, Dr. Latham, one of the most conscientious, careful, and unprejudiced students of disease that I have ever known, had collected (after about ten years' observation as one of the physicians to St. Bartholomew's Hospital) materials for, and was on the point of publishing, a book on fever. He had chosen with care his clinical assistants; he had kept regularly and had periodically digested his case books and their records; and he found that he had bled from the arm one in every four of his fever patients, and had applied leeches to nearly all of them: and the mortality had been seven in the hundred.

"But not long after the first visitation of cholera in 1832, so great a change occurred among his fever patients that he did not dare to

bring out his prepared book. *Now,* he could not venture to draw blood from any of his fever patients. Their condition urgently demanded support, and the mortality among them was doubled. So numerous were the deaths that it was impossible to make any regular inspection of bodies as long as this state of things lasted.

"This looked very like a change of type.

"We know now—but few or none of us knew then—that the diseases with which Dr. Latham had been dealing were, not varying types of the same malady, but two diseases differing in species; the first having been enteric fever, the second typhus.

"In this way, and to this extent, I readily admit that modern improvements in diagnosis have modified, on grounds of reason, our practice.

"I remember, also, to have read Dr. Caleb Williams' address to the British Medical Association on the Change of Type in Disease, in which he adduced his own experience and convictions on that topic, and gave some account of Autenreith's work, to the same effect.

"Again, and more lately, I remarked (and the fact tended to strengthen my faith) a great consensus of opinion among medical writers upon the alleged change of type who had lived and

practised in the period when the change was supposed to have manifested itself—while it was chiefly questioned or denied by younger men, whose personal experience had not included that period.

"I trust that I may now have convinced you that the opinion combated by you (which I should be ashamed if I had not candour or courage enough to renounce, or at any rate to doubt, upon good cause shown) was not formed at random, nor without supposed foundation for it, still less adopted as a miserable cover or excuse for former bad practice.

"I suspect that in a sentence of mine which has obtained an unhappy prominence in this controversy, I ought to have spoken of successive 'waves' of 'opinion' rather than of 'time.'

"Believe me, very truly yours,

"THOMAS WATSON."

GULSTONIAN LECTURES.

CHAPTER I.

The Subject Proposed. — Modern Revolution in Therapeutics. — Unreasonable Dread of Bleeding. — A False Extreme in Practice.

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 the present period of the world's history, 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 rather busied themselves 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, one of 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 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 patients, under the bias of the new idea, have imbibed as great a horror of being bled as doctors have of bleeding them. The result of this is that the practitioner, whose experience and judgment might have led him, in a given emergency, to perform venesection, not unfrequently withholds his hand; dreading lest, should his patient die, the onus of the death may be put to the account of his interference. 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 biassing influence of this anti-venesecting 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 ar-

guments which have brought us to such a remarkable conclusion? Surely, it is hardly credible that a remedy—undeniably most rapid and powerfully telling in action—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 a 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 a useful therapeutical agent) to hold its own as such. Reason seems plainly to 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; That whatever be the nature of those supposed altered conditions of the body, which are repre-

sented 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; That 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; That 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 in 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 bloodletting does not at this time 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. And 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 popular 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 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 bloodletting, rightly applied, is now, as it ever has been, a good remedy in diseases.

CHAPTER II.

Change of Type in Diseases.—Its Definition.—Are there Sthenic and Asthenic Phases of Diseases?—The Practice of the Past compared with that of the Present.—Andral's Statistics of Pneumonia.—Has Acute Rheumatism changed its Type?—Have Fevers, etc., changed their Type?—Bleeding in Fevers always an Error.—Bleeding in Phthisis an Error.

THE first cause which presents itself for our consideration, as explanatory of this present bloodless epoch of medical practice, is a *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 and is still maintained by some of the highest authorities in our profession, and therefore demands our most respectful consideration. With the profession generally, 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 machinâ* 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 patients' bodies would no longer suffer bleeding as in former days.*

Bleeding, indeed, as a universal and infallible remedy in 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 may call the dark ages of medicine. And I, therefore, cannot help calling your notice to the remarkable coincidence, that this change-of-type idea, the disrepute of wholesale bloodlettings, the departure of the antiquated humoral theory, and the life of our modern scientific medicine—all occurred at a somewhat similar period in 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. I much question, indeed,

if all who use the words have the same or a definite idea of their purport. By change of type, however, must be meant, I suppose, 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 have ever been 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.

It must also be understood, that this supposed change 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, etc., to which he may be accidentally subjected. The change imagined must be the resultant of some unknown influence totally distinct from those ordinary influences, whose good or evil operations on the body we can note and appreciate; and 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.

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. "There are waves of time," says Dr. Watson, "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."

Let us now consider the grounds upon which is based this change-of-type theory. Have we, indeed, good reason to believe that the human constitution undergoes at different epochs decided and inexplicable modification—the modification being manifested to us in an alteration of the effects produced by venesection in inflammations?

In approaching the subject, we at once perceive that the theory rests on very unsatisfactory evidence; that it is founded rather upon the fluctuating basis of personal opinion, than upon clear and tangible facts. The theory indeed is based on the assumption, that the effects of bloodletting now are different from what they were in other days—rests consequently upon a comparative estimate of bloodletting at different and distant periods of time. But is such an assumption capable of satisfactory proof? To answer it, we have, in the first place, to determine absolutely what were the effects of bloodletting 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 ignorant 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.

Is it indeed 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 reasonable to admit, that the mere use or disuse of a remedy at different epochs of time can be any true test of its merits; and especially when the real virtues of the remedy have ever been, and still actually are, matter of disputation? 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 confidence in the mere assertions even of the very highest authorities who have adorned the practice of our most fallible art. And if so, then it follows, that the practice of bleeding, said to have been so largely and universally adopted by 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 a highly-esteemed authority, 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 bloodletting 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 bloodletting. 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 bloodletting in pneumonia.

And now let us see the results of his practice, as given in his truthful 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 bloodletting. 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 bloodletting 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 bloodletting 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 proves only the abuse and bad application of a most excellent remedy.

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 the greatest authorities in the past bled freely in this disease, 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 acute rheumatism.

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 states of the pulse, and the tongue, and the fever, and the abstracted blood, were facts which they could appreciate as well as we. Masters of these things, therefore, they knew as well when to bleed. But, surely, those who thus argue will not assert, that the hard, full pulse, and the coated tongue, and the high fever, etc., of acute rheumatism, were a justification of bloodletting in this disease. And if so, then does it not follow 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 bloodletting 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 bloodletting was 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, perhaps, 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, when "an asthenic tide" was beginning to roll 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 young woman, under similar circumstances, he drew 120 ounces within five or six hours, and twenty ounces more on the next day. This patient lost her sight for a fortnight, and did not recover her health 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 thereby 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 bloodletting, 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. But 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 bloodletting; not from loss of blood, we are assured, but from inflammation of the vein.

I need not occupy our time by giving you other illustrations of the fact, that bloodletting saw its palmiest days during the period I am speaking of—and, as some say—asthenic phase of time. But does the fact of the frequency of its use in fevers, in any way prove, or satisfy you, that bloodletting 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 inutility, and worse than inutility, 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 bloodletting 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 they will ever be. True, we do read of wonderful benefits which have attended bloodletting 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 bloodletting is less injurious than it is in others —for example, in relapsing or inflammatory than in typhus fever.

I might apply the same kind of reasoning to the cases of tubercular and many other diseases, and ask if the mere fact that bloodletting was a common remedy with our forefathers in those diseases is any proof of the propriety of their use of it? 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 bloodletting now in their treatment, as our fathers did? Modern pathology inevitably shows that bloodletting neither is nor ever could have been a right remedy for the cure of acute rheumatism, or of fevers, or of tubercular diseases; and no one pretends that the practice of our forefathers, in this respect, was a proper practice.

But if our forefathers admittedly erred in these instances, why may they not have equally erred in the other instance of pneumonia? Why need we call up this change of type to save them from the imputation of an error in one case, while we admit that they were at the very time labouring under half a dozen precisely similar errors in treatment?

CHAPTER III.

Personal Experience.—Can past and present Medical Opinions be compared?—Experientia fallax.—Why did men cease to Bleed?—Where are Clinical Facts to prove Intolerance of Bleeding?—Change of Type Theory based on erroneous Assumptions.—Sydenham's Views of Fevers.—Evil effects of Bloodletting greatly exaggerated.

ANOTHER argument, adduced in support of this theory, is one derived from the personal observation of medical men, of high repute and of large experience, whose opinions rightly claim our respect and full consideration. These authorities do not rely solely upon tradition as proofs of the position they assume. They tell us that they have seen with their eyes the facts on which their belief is founded. They have witnessed the effects of bloodletting in other long past days; and they have also seen and noted its effects in these more recent times, and are therefore able, of their personal knowledge, to contrast the past with the present effects, and so deduce the conclusion, that diseases have passed during the interval from a sthenic into an asthenic phase.

To meet this argument, which bears, on the face of it, strong apparent testimony in favour of the theory, I am necessarily driven to the somewhat presumptuous position of questioning the correctness of the observations referred to. Happily, however, the high authorities who have given their sanction to the theory are the very men who have always taught the profession, above all things, never *jurare in verba magistri* in matters medical; but to try all things by the light of their own reasoning and observation. They, therefore, I may confidently feel, are the last men who will object to the freest criticism of their opinions.

Let me here again remind you that the change in type theory rests solely upon the assertion that bloodletting was found by practitioners of medicine to be well borne and of great service in inflammations some thirty, forty, fifty, or seventy years ago; and that it is not found to be so borne now; and that the assertion is based solely upon a comparison of the effects of bloodletting in past days with its effects at this present day.

I have already attempted to show you how impossible it is for us, practising medicine in the middle of the nineteenth century, to compare the results of our treatment with the results of the

treatment of the past generation of practitioners. I shall now attempt to show, with the greatest deference to those high authorities who have sanctioned the theory disputed, that neither can any safe conclusions be drawn from a comparison of the practice of men of the same generation, made at distant intervals of time.

In the first place, I would ask, Where is the scientific man of medicine, who at sixty years of age sees diseases and treats diseases as he saw and treated them when under the influence of the ideas which held possession of his mind at twenty-five? His belief in his curative powers, his enthusiastic faith in the energetic power of his art to cure diseases, will probably have gradually abated, and *pari passû* with his advance in years. With the development of his reasoning powers, his questioning of the efficacy of his remedies will probably have become stronger. At all events, and most assuredly, the activity, the heroism of his treatment will be found to have declined with his increased age and better experience.

Let any one call to mind the exact state of medical knowledge at the beginning of this century—during the first quarter of it, for example; let him turn to the works from which men of medicine then drew their inspiration in medical

knowledge ; let him compare that knowledge with the scientific knowledge of this day,—and then say if even the calmest judgment and the most gifted observation, so trained and fed, would be able now, at this time of day and under the bright light of all our advanced and accumulated knowledge to calculate and measure to any effective purpose the analogies and differences between the results of the practice of those past and of these present days ? Where is the philosophic and experienced physician of modern days, whose ideal of medicine has not been revolutionised since the day when he first entered upon practice ? Is it possible then that, with these modernised views of his concerning the powers and virtues of his therapeutical agents, and the nature of diseases, he should be able, out of his general recollection of what the effects of bloodletting were some thirty or forty years ago, to draw trustworthy conclusions as to the effects of bleeding at this present day ? With these difficulties before me, and calling to mind the deep meaning and profound truth of the aphoristic words of the wise man, who warned us that, in the practice of medicine, experience was fallacious, and a right conclusion hard to come at—I think I may fairly question the correctness of a theory which rests upon no other basis than one so unstable

and fluctuating. Every page of the history of medicine records the painful fact that, in matters medical, it is most difficult to extract the right conclusion from the lessons of experience; and that, in fact, theory has never been so fertile a breeder of errors in medicine as this so highly extolled experience. Men may observe and record the facts which lie before them most correctly; but still ever is it that some one or more essential items of the case may have escaped their observation—so compound are the phenomena touching disease and its treatment, so many and so hidden or dimly seen the disturbing causes which affect its right reading.

I have said that the change-in-type theory of diseases was not adopted until long after the practice of bloodletting in inflammations had been generally given up; and I must adduce what seems to me the proof of this very significant fact. And first let me ask how comes it, that the authorities who now tell us that this present asthenic phase of disease came upon us soon after the advent of cholera in 1832, did not make the discovery or note it down at that time? How comes it, that the fact of this change in type—the fact that men would not bear bleeding as they formerly did — was not announced until twenty or twenty-five years after the asserted

change displayed itself? Surely the asthenic change in type could not have been more clearly seen now out of a distant memory of past practice, than it was then, at the moment when (as we are told) men manifestly bore bleeding ill—at the moment when they were actually under the lancet, and displayed the signs of the asthenia—at the time when physicians gave up the practice of bleeding, because they saw their patients would no longer bear the bleeding?

We are, in fact, asked to give our assent to a theory based upon conclusions drawn from the observations of facts made a quarter of a century ago, the main significance of which facts (so far as records tell us) entirely escaped notice at the very moment when it should have been most prominently marked. We gave up bloodletting twenty-five years ago; and it takes us twenty-five years to discover that we gave it up because patients would not bear it, or, in other words, because diseases had changed their type! It seems to me inexplicable, except on the supposition that there must be some fallacy in the argument, that Dr. Alison and other high authorities, who believe in this change of type, should have continued, for twenty years at least after the change is said to have occurred, to eulogise in the strongest terms the practice of bloodletting

in inflammations. And, curiously enough, we even find these authorities actually explaining to the student how it is that they, who at that time so highly extolled bloodletting, so rarely resorted to it in their hospital practice. We should have expected that the first reason given would be this change in type; but nothing of the kind. The reason is, as Dr. Alison and others say: When this class of patients come under our hands, the time for bloodletting with them is past; and, besides, they have often been bled largely before they came into hospital. Constantly, in fact, do these authorities in their writings warmly extol the virtues of bloodletting in inflammations; and nowhere do they tell—at least, until a recent late date—that the human body is in an asthenic phase, and will not now bear bloodletting. Nay, years after the assumed advent of this asthenic type, we find authorities, like Dr. Watson, warning the student against being led away by the fallacious therapeutics of Louis, who was misguiding the minds of the French youth by teaching them that pneumonia could be cured without bloodletting.

One remarkable exception, it is true, has been made; but the exception clearly goes to prove the fallacy of the general argument. Fevers, we were long ago told—I mean, told in the recorded

words of authorities—have not borne bleeding well since 1832. But how comes it, I would ask, that it has been recorded as a fact, that fevers did not bear bleeding; and yet no kind of allusion made at the same time to the *now* asserted fact, that neither have pneumonia and other inflammations borne bleeding well since 1832? How comes it, that the physician failed to note the asthenic character of pneumonia, as well as the asthenic character of fevers? How is it that, in 1857 we are told, for the first time, that pneumonia has been for a quarter of a century of an asthenic, non-bleedable type? If the fact could long ago be so clearly predicated of fevers, why was it so undiscernible in pneumonia?

Now, it does happen that we can give a very satisfactory explanation of why bloodletting was said, and no doubt truly said, to be so ill borne in fevers. Firstly, men's theories of fever were completely revolutionised about the year 1830; and, secondly, as Dr. Murchison has so well shown, the prevailing fever in this country at that date was typhus; the inflammatory or relapsing sort, which had previously been so common, having become then very rare. But it is a well known fact that bloodletting is infinitely better borne in relapsing than in typhus fever—*i. e.*, does infinitely less mischief. Consequently,

bloodletting in typhus would naturally produce worse results in practice than bloodletting in inflammatory fever. No doubt, under the benign influence of better theories, men observed this greater injury done by bleeding in typhus, and therefore they wisely abstained from bloodletting; and therefore, I must also add, unreasonably assumed that diseases had changed their type.

It seems to me, therefore, that a consideration of this subject clearly leads us to this conclusion: that the high authorities to whom I have alluded very correctly recorded their views that fevers were injuriously treated by bloodletting, but that they fell into the *ex post facto* error of judging of the use of bloodletting in pneumonia and other inflammations by the light of the ill effects produced by bloodletting in fevers; and that, in truth, the change-of-type theory, as applied to inflammations in general, was in reality based upon what was observed of the result of bloodletting employed in typhus fever?

Sydenham has been called the prince of observers; but who, at this day, can read his lessons, and not note—I might almost say on every page—proofs of the fact, that his observation was trammeled by the preconceived ideas which possessed his mind when he came to engage in

the practice of medicine? Can we believe that Sydenham's observation was based on a true record of what he saw, when he tells of the rapid changes—I suppose he meant what we call change in type—which fevers underwent, according to the particular constitution of the year? And it is interesting to note, that Sydenham appears to have judged, or rather to have learnt the existence, of these changes, just as they have been learnt in modern days; viz., from noting the different effects produced, as he believed, by his remedies at different periods. Thus, he tells us, that he often found a remedy which would cure a fever at the beginning of a year would kill the patient at the end of the year. "This, at least," his words run, "on the strength of a multiplicity of accurate observations, I am convinced of; that diseases of the character alluded to, and more especially continued fevers, differ from one another, north and south; and that the remedy which would cure a patient at the beginning of the year will kill him perhaps at the close." And again, he says, "When once, by good fortune, I have hit upon the true and proper practice that this or that fever requires, I can, with the assistance of the Almighty, by taking my aim in the same direction, generally succeed in my results, respect being always had to the age

and temperament of the patient, etc. This lasts until the first form of epidemic becomes extinct, and until a fresh one sets in. Then I am again in a quandary; and am puzzled to think how I can give relief. And now, unless I use exceeding caution, and unless I exert the full energies of my mind, it is as much as—nay, it is more than—I can do, to avoid risking the lives of one or two of the first who apply to me as patients."

But surely we—with our modern knowledge of fevers—cannot admit that these diseases really changed their constitution in the brief period alluded to by Sydenham? And if Sydenham fell into such error, is it not probable that we also may err in drawing conclusions in a like case from a comparison of what we saw twenty-five years ago with what we see now?

And, again, how comes it, that other equally remarkable changes in the use of certain capital remedies which have occurred during the last quarter of a century, have not been also adduced as proofs of the change in type of diseases? If the conclusion, as drawn from the effects of bleeding at the present day, be good, why should it not be equally valid as drawn from analogous instances—the employment of other remedies? Thus, for example, why should we not argue that diseases have changed their type,

because, at the present day, various acute diseases —peritonitis, pneumonia, meningitis, and carditis —are found to undergo resolution without the induction of salivation? Surely the revolution which has banished the use of mercury, or something like it, from our practice in such cases, is as remarkable as that which has discarded venesection. Why may we not with equal justice and reason say, these acute diseases no longer require mercury for their cure, and have, therefore, changed their type, as say that they have changed their type because we cure them without bleeding.

Moreover, the conclusion which I am combating, that men do not bear bleeding now as they did formerly, seems to be still further vitiated by the fact, that not only are we, as observers, unfitted to make the comparison, but that the actual elements of a fair comparison are wanting. In those other past days to which we are referred, bleeding was a sort of universal remedy. Whatever were their diseases, men were let blood for the cure of them. Surgeons prepared their patients for operations by a seasonable bleeding; and, in fact, men in perfect health had themselves periodically bled in order to anticipate the possible occurrence of disease. Hence, the medical man of long experience, who has such a vivid re-

collection of the excellent effects of bloodletting in his youthful days and in the supposed sthenic times of inflammations, derived his esteem of it from what he saw of the effects in cases which he counted by the hundreds: whereas he now condemns its use from what he notes of its effects in a few rare and isolated instances. He forgets that, in most of his ancient cases, the bleeding was never required at all; and that, being frequently practised on persons who were but little injured by disease, it was not likely to produce injurious effects. I would venture to ask this learned assemblage, how many there are of you who have given up the practice of bleeding through a personal witnessing of its pernicious and depressing effects on man at the present day? I think, indeed, I may safely assert, that the profession as a body has accepted the suspicious conclusion solely on the "word of the master"; this word of the master having been accepted without examination, and re-echoed again and again so repeatedly, that at last what was originally suggested hypothetically has been generally accepted as an undeniable and unanswerable fact.

Besides this, I might add that, in those other days, and under the lingering influence of the old humoral theory, men did not note the injuries

inflicted on the human constitution by immoderate bleeding, as we, in our wiser generation, have been enabled to do. And they, therefore, never thought to lay to the charge of their lancet any ill effects of their bloodlettings; but put them all down to the credit of the disease for which the bleeding was practised. The data, therefore, for any true comparison between the effects of bleeding as the physician noted them in his youthful days, and the effects as he notes them now, are completely wanting; leaving out of the case all consideration of personal incapacity for making the comparison on the part of the observer, resulting from the revolution which his ideas must have undergone whilst passing from youth to advanced age.

Then, again, it must not be forgotten, that there are practitioners who to this moment refuse to admit the truth of the proposition, that the human body will not now bear losses of blood well, and who still practise bloodletting. Certainly, arguing from my own limited personal experience of the effects of bloodletting during late years, and from what I can learn on the subject from others, I cannot help saying, that the evil effects of bloodletting have been greatly exaggerated at the present day; indeed, beyond all proof and reason. Where are the proofs of the

facts asserted? Who can produce them? I fancy there are few of my auditors who could give a satisfactory reply to the question out of his own personal knowledge. Is not the idea, in truth, so generally accepted, of the dangers of bloodletting, based rather on some vaguely floating notions—received from authority and accepted without inquiry—than on any tangible and real valid facts? And on the other hand, have we not strong proofs to show that the human body still bears large loss of blood with as little prejudice as ever it did? Have we not the experience of those who still practise bleeding? Have we not also the constant fact before us that men lose large quantities of blood in various diseases, from the lungs and the stomach, etc., and yet note that they recover rapidly and well from the hæmorrhage? Again, women after parturition, at times, lose enormous quantities of blood now as formerly; but accoucheurs do not complain that women bear these losses less well at this time of day. Patients, in the hands of the surgeon, also, after accidents and under operations, often suffer severe hæmorrhage; but I never heard of a surgeon who invoked this change-of-type theory to account for any evil results of the same. How is it, one cannot but ask, that no illustrations of the change-of-type

theory have been afforded us from these quarters? or rather how is it, that the discussion of this, as it seems to me, unanswerable argument has been so carefully avoided by Dr. Stokes and others who support the change-of-type theory?*

* I am happy here to quote the words of Mr. Syme, in his Address, delivered before the British Medical Association, since these Lectures were published. (See *British Medical Journal*, Aug. 12, 1865, p. 142.)

"Commencing with the treatment of inflammation and its consequences, I may notice a most remarkable difference between the old and present practice, in the almost entire disuse of bleeding instead of its nearly constant employment. On looking back, it is indeed difficult to realise the reckless and indiscriminate profusion with which blood was made to flow. When I was one of the dressers of the Royal Infirmary of Edinburgh, two of us went every evening, at a stated hour, to bleed the patients whose names were entered in a book, with the respective quantities due from each. On one occasion, I recollect of sixty-five ounces taken at once, and followed by thirty-five next day. At present, few surgeons carry a lancet, and still fewer ever employ it; so that venesection, instead of being the most frequent, has become one of the rarest operations in surgery. The reason of this is generally said to be a change in the type or condition of the human system; but may, I think, rather be attributed to the influence of more correct ideas in regard to the treatment of disease; since, it is certain, that operations no less bloody than those of the old time, are now performed without any evidence of less ability to bear them."

CHAPTER IV.

Asthenia an Old Idea. — Mr. Lawrence's Opinion.—Dr. Bateman on Bleeding. — Supposed Signs of Debility in Hunter's Day.—Bordeu.—His Estimate of Bleeding. Different Treatment of Fever equally successful.—The late Dr. John Reid.—Modern Humoral Theories.—Change of Type Theory not accepted in France.—No Evidence of the Change.

Moreover, I must also call your attention to the remarkable fact, that the idea of the human body bearing bleeding ill is not an idea of the present generation. There, probably, has never been an age, from the days of Galen to our own, in which bloodletting has not been denounced as injurious to the constitution. But, to keep closely to the case in hand, I will content myself with showing you that, for the last hundred years at all events, there have always been authorities ready to affirm, that man's constitution was growing weaker; the proof and test of their assertion being founded on the very fact which forms the base of the modern argument; viz., that men did not bear bleeding as formerly. The opinion of those physicians was, in truth, just the opinion which is now held; only, instead of saying that

diseases had changed their type, they said that the human body was less tolerant of loss of blood than it had been formerly. I will sketch you, rapidly, a few proofs of this.

I have already given you the words of those who affirm a present existing asthenic type of disease. I will, therefore, take you back about thirty years, and produce the high authority of Mr. Lawrence at that time, who refutes the theory, let it be observed, while referring to its existence. "A notion," he says, "has prevailed, that people who live in London and other large towns, do not bear depletion well. I consider that this opinion is supported neither by experience nor argument. The dread of depletion has been transmitted from one writer to another without examination or inquiry, and has led to an inert practice." Dr. Southwood Smith and Dr. Tweedie strongly recommended bleeding in fever in 1830; and at that time—the asthenic period, remember, of modern authors—Dr. Tweedie tells us, "No remedy in the treatment of fevers has been more abused than wine." Writing in 1816, Dr. Armstrong deprecates the stimulating practice of erysipelas "still followed in some metropolitan hospitals; for the sake of science and humanity, he earnestly hoped that it would soon be abandoned:" thus rebuking the

dread of asthenia which at that time existed amongst certain physicians. Dr. Bateman, again, writing in 1818, thus speaks of bloodletting: "The other active remedy which I have mentioned as capable of abridging the course of fever, if employed early, is bloodletting. I believe there are few physicians, who, like myself, commenced their professional career impressed with the doctrines that prevailed in the schools *at the close of the past century, in which the term of debility was certainly predominant,* who will not acknowledge that their subsequent practice has been a continued struggle between the prejudices of education and the staring conviction of opposing facts, which were continually forcing themselves upon their observation; and that they have more especially been compelled to a gradual, but material, change in their views respecting the use of the lancet, not only in fever, but in other diseases. I am fully convinced of the extent to which my own practice has been cramped by this prejudice." Why, here, at the very time when, as we are told by some authorities, the asthenic phase of disease was in full operation, we have the learned Dr. Bateman assuring us that bloodletting is the remedy, and rebuking the ignorance of those who, in his youth, had such a dread of debility!

Still further back, we have the testimony of John Hunter that the physicians of his day, just like physicians of our day, laboured under the idea that inflammations were more asthenic than formerly—*i.e.*, would not bear bleeding as well; the two phrases being then, as now, exactly convertible. "It is observed," he says, "by some of the ablest physicians of this day, that the fever called inflammatory is not now so common in this country as it was formerly represented to have been; that it is now seldom that in fevers they are obliged to have recourse to the lancet, at least to that excess which is described by authors in former times. They are now more obliged to have recourse to cordials than evacuations; and, indeed, the disease called the putrid fever and putrid sore-throat are but of late date. I remember when the last was called Fothergill's sore-throat, because he first published upon it and altered the mode of practice. I remember when practitioners uniformly bled in putrid fevers; but signs of debility and want of success made them alter their practice. I believe we have much less occasion for evacuations in inflammations than there was formerly. The lancet, therefore, in inflammation, and also purgatives, are much more laid aside."

Why! are not these the very words which are

met with in our text-books at the present day? Are not these arguments of John Hunter the very arguments now used against bleeding? And yet men actually base their theory of the change of type on the assumed fact of the greater tolerance of bleeding in men of Hunter's day, and even of days long subsequent to his time!

Another quotation from Hunter I must give, because, curiously enough, he finds in the fast-living of men of his time a reason why they bear bleeding so badly in inflammations; whilst, on the other hand, we find writers of the present day suggesting that we don't bear bleeding as our forefathers did, because we don't live as freely as they did! Never was a more remarkable illustration of the bending of facts to make them fit theory. Hunter says: "We certainly live now more fully than they did formerly. We may be said to live above par. At the full stretch of living, therefore, when disease attacks us, our powers cannot be excited further, and we sink, so as to require being supported and kept up to that mode of life to which we have been accustomed."

Hence we see that for a hundred years past, at all events, men of authority have continually maintained the same doctrine; viz., that men's constitutions were weaker than formerly; and

that, therefore, they would not bear bleeding; that this theory of change of type, which now exercises so powerful an influence over our practice, is the very same theory which has existed for at least a hundred past years; and that John Hunter said the very same things about the incapacity of men to bear bleeding in his day, as are said at this present moment.

Surely, then, I may conclude that the practice of physicians at different periods tells little either in favour of or against bleeding. The remarks of Bordeu—the most sagacious Bordeu—delivered about a hundred years ago, express, as I believe, the actual facts of the case. They are golden words. "It has ever been thus with physicians," he says. "In past ages we find them, sometimes storming against bleeding, and desirous of banishing it altogether from medicine; and sometimes using it as a cure for all diseases —counting their victories by the number of their bleedings. The public, too, sometimes take a part in these quarrels. We have even known whole towns divided into sections—the friends of the bleeding doctor, and the friends of the doctor opposed to bleeding. Truth to say, bleeding has ever been a subject of dispute, and of most surprising dissensions. But let it be said, to the honour of medicine and of those who cul-

tivate it carefully, that there have always existed physicians of judgment—*médecins d'elite*—who have joined neither party, rejecting the extreme ideas alike of the ultra worshippers of bleeding and of its enemies. There have always been, and always will be, patricians of this high class in medicine." Elsewhere he sums up, and still in golden words, the actual value of bleeding. "It may be said that, as a rule, the partisans of bleeding overestimate the value of its effects and the necessity for its use; and that, on the other hand, those who rarely resort to bleeding have an unreasonable dread of its consequences. It is not true that numbers die from bleeding, as some would have us believe; and still less is it true that those who rarely ever bleed lose as many patients as the partisans of bleeding say they do. Daily observation in hospitals shows the fallacy of both these conclusions; and the truth is, that it is not easy to distinguish between bleedings which are useful and necessary, and bleedings which are hurtful and injurious."

To show the difficulty of rightly appreciating the real effects of bleeding in disease—at all events, when the mind is occupied with preconceived views of the nature of the disease for which the bleeding is practised—I think I may well mention a fact which has left a lasting im-

pression on my mind. I refer to the treatment of typhus fever in the Edinburgh Royal Infirmary some twenty-five years ago.

At that time, the fever-patients were massed together in separate wards in one wing of the hospital; the wards being one above the other, and containing a similar number of beds on each story. Dr. Alison, at the time I refer to, was physician of the first floor; Dr. Craigie of the third floor; and Dr. Shortt, whose house-clerk I was at the time, ruled in the second floor. The fever-patients were all of them contributions from the famous, or rather infamous, wynds of Edinburgh; and, as they were indiscriminately introduced into these wards day after day, I suppose nothing need here be said of any difference of type in the typhus itself.

Now these patients were subjected to the following treatment. Dr. Alison, from first to last, carried out a sustaining treatment; stimulants and wine and nourishment being his main therapeutical agents. On the second floor was followed no one particular line of treatment; there was bleeding and blistering and wine-giving, according as the patient's actual state seemed to require one or the other kind of cure. The treatment was, in fact, something of the expectant; for Dr. Shortt had no particular theory

about the nature of fever. On the third floor, Dr. Craigie reigned; and, indeed, a very sanguinary government his was. In his wards, bleeding, leeching, cupping, blistering, were the order of the day, and, I may say, of the night; and a Maine liquor-law was rigidly enforced. I need not add that each physician was thoroughly satisfied of the correctness of his method of cure, and found nothing in the results of it to induce him to change his plan.

Now here we have physicians of the highest repute practising, not at different times and in different places, but at the same moment, in the same hospital, and on identical cases of disease, diametrically opposite methods of treatment, and each fully convinced of the excellence of his method—of the use and of the hurtfulness of bleeding. No one, surely, would conclude that bleeding was the right method of cure for typhus, merely because it was so extensively used by one of these physicians. Yet this is just what they have done, who support the change-of-type theory on the strength of the practice of certain admirers of bleeding in other days.

Perhaps some of my hearers may be curious to know what was the actual result of these different methods of treating typhus; and I will just mention the sequel to show the fallacy to which we

are exposed in judging of the effects of remedies from the statistical method point of view; and therefore, I may add, the double fallacy into which we may fall through concluding, because men of high repute in other days employed a remedy, that therefore it was a right remedy; and that, consequently, if men of high repute employ it no longer, diseases have changed their type. It happened, at the time to which I am referring, that Dr. John Reid was pathologist of the Edinburgh Infirmary; and perhaps I may be pardoned for saying that the mention of that name brings before me the memory of one of the most profound, accomplished, and philosophic minds with whom it has ever been my good fortune to hold intercourse: so kind, so gentle, and so generous to his inferiors in age and knowledge —himself with the most masculine and powerful of intellects, as simple as a child in the acquisition of knowledge; and, above everything, calm, honest, and truthful in research, and in the story of his researches.

Dr. John Reid's duty at that time was to record the *post mortem* examinations of hospital patients, and his attention was particularly drawn to the facts I have mentioned here; and he thought that, from the very large number of fever-patients who were then crowding the hospital wards, some

authentic results, as regarded the effects of treatment, might be obtained. He, therefore, carefully noted the number of deaths in the fever-wards of the three physicians alluded to; and he assured me that the year's account showed, as regards the mortality, there was not a pin to choose between the three methods of treatment. That such was Dr. J. Reid's deliberate conclusion I can have no doubt; for, within the last few months, I heard Sir D. Brewster say that Dr. Reid had, many years afterwards, made a similar statement to him.

A well-known writer (Buckle) has said words to this effect, that there is no example in the world's history of a theory having been put away through observation of the practical evils which it produced. Change in knowledge, he says, comes and upsets the theory; and not till then do men see the viciousness of the practice. I believe his saying finds an apposite illustration in the case before us.

In advanced physiological knowledge, and in the birth of a more scientific humoral theory, must we seek an explanation of the change in practice which occurred about the year 1830. Let me remind you that this change in practice began coincidently with the rise of a new era in medicine. The researches of Prout, Magendie,

Andral, Liebig, and a host of other observers, touching the blood, etc., revolutionised our notions of diseases, and created quite a sensation in the minds of physicians. The theories which sprang out of those researches undoubtedly had a most powerful influence in arresting the bloodletting treatment. By these theories, men were taught that bleeding neither was nor ever could have been a proper remedy for many of the diseases in which it had been used so freely. They, therefore, began to doubt of its use in other diseases.

Let it also be remembered, that the time preceding the rise of our modern humoral theories was one of the most sanguinary epochs of English medicine: so that physicians, under the light of their new theories, had an especial occasion to be impressed with the evil effects of the indiscriminate bleeding which had been previously practised by them. Their theory was there to lead them to a distrust of bloodletting; and then were their eyes opened to the evils which now showed themselves so flagrant to their observation.

It may be added, as worthy of note, that this change-of-type theory has not been adopted by those of our French medical brethren who have altered their practice in respect of bleeding. Such an explanation of the change of practice

never seems even to have entered into their heads. Trousseau, in rejecting bloodletting as a remedy in apoplexy, says:—"For many years I did what most practitioners still do, and thought my treatment very rational. In spite of ourselves, I am forced to confess it, we all feel the influence of fashion. Led away by the doctrines of Broussais, I was once accustomed to order leeches, in cases where I never do now, and only because every one else did so. I bled also in apoplexy, because everybody else bled. But now that I have advanced in years and am in a position to carry out my own opinions freely, I reject bleeding in apoplexy."

[It is curious to note here how even a man like Trousseau may rush into extremes. In warning the student against bleeding, he relates a case of apoplexy, in which about three ounces of blood were taken, and he unhesitatingly attributes the death, *which took place some days afterwards*, to this loss of three ounces of blood!

"The patient was hemiplegic, with distorted face and difficult speech, but clear in intellect, and conversed freely. After the bleeding he fell into a state of complete resolution, in which he remained until his death, which occurred some days later."]

With the deepest respect, therefore, to the

great authorities to whom I have referred, I must, and as I think most reasonably, ascribe this change in practice—this non-bleeding in inflammations—to our better scientific knowledge; to the rise of modern animal chemistry and physiology; and partly also to the observation of the fearful mischief thereupon noted as done by the murderous bleedings of the first quarter of this century. I see no necessity for the calling in of this change-of-type theory; and I can find no sufficient evidence on which to satisfy myself of the validity of the theory.*

* Buckle's words (above alluded to) are:—"There is no well attested case on record of any theory having been abandoned because it produced dangerous results. As long as a theory is believed men will ascribe its evil consequences to any cause except the right one. And a theory which is once established will always be believed in until there is some change in knowledge which shakes its foundation. Every practical change may, by careful analysis, be shown to depend, in the first instance, on some change of speculative opinions."—*History of Civilisation.*

CHAPTER V.

The Real Effects of Bleeding not recognised. — Direct Abstraction of Blood. — Indirect Abstraction of Blood. — Bleeding and Leeching are different Remedies. — Bleeding abandoned in External Inflammations. — The Right Uses of Venesection.

I HAVE lingered long on the theory of a change of type in diseases, not only because I believe it to be an incorrect theory, but chiefly because it is certain that its rejection would remove the greatest obstacle which exists to our taking a fair measure of the value of bloodletting. If it be satisfactorily shown that the theory is erroneous, or unworthy of credit, then the profession would reconsider the whole matter, and would, I believe, once again take the lancet in hand and bleed their patients, not certainly as our forefathers did, without rhyme or reason, and under the blinding influence of false theories, but rationally—to the benefit of the sick, and in accordance with the teachings of modern medical knowledge.

I now turn to the second part of my thesis, and shall show that another obstacle to our em-

ployment of venesection in diseases is to be ascribed to the erroneous notions usually adopted concerning the uses and actions of the remedy; that we have ascribed to it virtues which it does not possess; and have, in consequence of its failing to effect what our false expectations had awaited from its use, allowed it to lapse into neglect.

I have, therefore, now to ask you to consider with me whether we may not arrive at a truer estimate of the uses and value of bleeding from a basis of considerations other than those which have hitherto been taken into account, and which are founded on the unstable basis of uncontrolled personal experience and of medical statistics. It seems to me that there is a safe and firm ground, whereon we all stand much together in agreement, and from which the matter may be usefully discussed; that there are, in truth, a series of facts fitted to serve our purpose in explaining the uses of venesection in disease, so clear, simple and positive as, when clearly expressed, to press themselves home upon the conviction of all of us. It is from the consideration of plain-speaking facts of this kind that the conclusions here suggested, concerning the influence exercised by venesection over diseases, are derived.

It is necessary, in the first place, in considering the effects of bloodletting in diseases, to bear in mind that a marked distinction must be drawn between the effects produced by the local —*i. e.*, the direct—abstraction of blood from an inflamed part, and the effects produced over such inflamed part by venesection—*i. e.*, the indirect abstraction of blood. Systematic writers pay no attention to this all-important difference. The mode of abstraction of blood, in respect of its influence over the inflammation, is usually regarded as much a matter of indifference; the main item of consideration being the quantity of blood extracted. Thus, for example, in pneumonia, we are told, that the condition of the patient must decide us as to how the blood should be removed; if the patient be feeble, let the blood be taken by leeches or cupping from the skin in the neighbourhood of the inflamed lung; but if the patient be strong and vigorous, then let the blood be taken from the arm; it being assumed that the effects produced by the abstraction of blood are alike in both cases, and in both cases have a similar control over the inflammatory process.

But a nearer consideration of the subject infallibly shows us that there is a marked distinction to be drawn between the effects

of the direct abstraction of blood from an inflamed part, and the effects exercised over that part by venesection — *i. e.*, by the indirect abstraction of blood. *A priori* reasoning suggests that such should be the case, and experience proves that it is so.

In the one case—the direct abstraction of blood —the blood is drawn directly from the inflamed part, from blood-vessels which are directly in communication with the inflamed part. The abstraction of blood has consequently a direct and immediate influence over the seat of inflammation. It not only draws blood, but it draws inflamed blood—*i. e.*, blood deteriorated under the action of the inflammatory process, altered in quantity and altered in quality, and therefore no longer duly fitted for its purposes—from the part affected. It may, indeed, abstract blood which in its altered state acts poisonously, both locally on the inflamed tissues, and generally on the system at large, as it passes into the circulation. The local action of the leeches or the cupping, moreover, may, and I believe actually does, exert by irritation an influence through the vaso-motor nerves over the vessels of the inflamed part to which they are distributed. But venesection manifestly neither does nor can exercise such influence over the inflamed part.

Moreover, the distinction here maintained is confirmed in the most marked manner by the teaching of experience. Experience demonstrates to us, in the case of external, visible inflammations, that the direct abstraction of blood from an inflamed part, during at least the congestive period of the inflammatory processes, exercises a powerful influence over the progress of the inflammation. Thus, for example, the application of a few leeches around an inflamed ankle-joint or an inflamed conjunctiva almost invariably reduces the main characteristics of the inflammation—the pain, the heat, the redness, and the swelling; and it does this more effectually and certainly than the abstraction of a pint of blood from the arm would do.

Venesection, on the other hand, exercises no direct and immediate influence over the local inflammatory process. It removes no inflamed or locally altered blood from the body. It can have no direct derivative action; it can exert no nervous influence of that kind referred to by Hunter under the term of contiguous or continuous sympathy, such as may be effected by the local taking of blood. Its influence on the local inflammation can only be transmitted indirectly through the general circulation, and therefore operates only in a secondary way. Venesection

affects the locally inflamed part, just as it affects all other parts of the body, and only as—*i. e.*, not more powerfully or otherwise than—it affects them; its main influence being a reduction of the heart's force. The effects resulting from this reduced force of the heart are divided and distributed equally through the whole body. By venesection, the heart's power is diminished; diminished, therefore, is the stream of blood which is forced through all the channels of the circulation into every part of the body. The amount of blood abstracted by venesection is deducted equally from all parts of the body—surely not more from the inflamed than from every other part; so that, even if the venesection be very large, the amount of blood withdrawn from the seat of inflammation must be small, probably not so much as would be drawn by a single leech directly applied to it.

Hence we see that the direct abstraction of blood acts immediately upon the seat of inflammation; that its benefits are sure and immediate; and that, as usually practised, its influence over the system generally is scarcely perceptible; that venesection, on the other hand, has no such influence over the local inflammation; but acts only indirectly and through the system at large. The good effects of the direct abstraction of

blood from an inflamed part are positive and manifest, and are admitted by all; and they are obtained at a small cost to the system at large. The good effects of venesection over the inflamed part are not easily demonstrated; they are disputed, and are more or less costly.

I have insisted on this distinction between the effects of direct abstraction of blood and of venesection, because it is most strangely overlooked in practice, and because it seems to me to afford a clue to an explanation of the beneficial effects actually produced by venesection in inflammations. I have also attempted to remove the false idea, so generally entertained, that venesection is felt and resented in some especial manner by the part inflamed; affecting that part differently from what it does the other parts of the body. And I rest these conclusions upon the plain every-day lessons demonstrated to us by direct observation—I mean, upon observation of the effects of venesection, and of the direct abstraction of blood, in the case of *external* inflammations.

It is a fact that venesection has long since been, both by physicians and surgeons, completely discarded, not only practically, but theoretically, in the treatment of all external inflammations — *i. e.*, in all those inflammations of

whose course and progress the eye can take cognisance. Equally is it a fact that we have never either abandoned in practice or denounced in theory the direct abstraction of blood from external inflammations. Surgeons doubtless abandoned venesection in external inflammations because, under the enlightenment of modern medicine, they failed to see its utility. I am not aware that any surgical authority—and I need not say that surgeons were once as free bleeders as physicians—ever called in the change of type theory to account for his change of practice in the matter of external inflammations. Nor do I find that any physician in renouncing venesection in the cure of *external* inflammations has put on record that he abandoned the practice because he found his patients would not bear the loss of blood as they once did. Under the lessons of modern pathology, men learnt that venesection never could have been a proper remedy for the inflammations which are met with on the surface of the body; and therefore they abandoned the practice, and because, so instructed, they were no longer able to note the utility of the practice.

Venesection, then, in external inflammations, let it be noted, was given up before bleeding in internal inflammations was denounced; and ob-

viously because men could far better note the results of the operation in the one case than in the other. The change-of-type theory of disease has been called in by physicians, and by them only, as explanatory of the abandonment of bleeding in *internal* inflammations.

From a consideration of the foregoing particulars, I am led to inquire, What, then, are the real uses of venesection in diseases? What good ends does it effect? Is it credible that a remedy which, through evil report and through good report, has steadily held its own in the catalogue of curative agencies from the days of Hippocrates to our own, can all at once have ceased to be of service to humanity? Must we believe that all the great minds who, through the long ages of past medicine, have resorted to this remedy, have been using it under a complete delusion? Surely the very fact of the antiquity of the remedy, its universality, and its persistence during all times as a cure, is strong *à priori* evidence of its possessing some value and excellence as such.

But, in truth, no living physician, I believe, denies its abstract value as a remedy. Physicians of the highest authority in modern days have said of it, that "of all remedies in acute inflammation, the only one on which absolute reliance can be placed is bloodletting."—"The great re-

medy in acute and dangerous inflammation is bloodletting." This, then, admittedly excellent remedy, if rejected, is rejected not through any recently discovered fault in itself, but on other grounds. It is a capital remedy we are told; but cannot now be used, because the human constitution is at the moment in one of its periodical phases of asthenia. When the sthenic tide again rolls in, then the remedy will once more do great service in the cure of diseases.

Others say venesection is an excellent remedy, that it ever has been and ever will be an excellent remedy, that men have too often abused instead of rightly using it, and that therefore has it fallen into so great discredit. They also add that it was no asthenic phase of diseases which arrested venesection, but the advance of knowledge bringing with it a wiser and a better theory of diseases. Thus enlightened, men at length saw the evils inflicted by their indiscriminate bloodlettings. Moreover, the idea of a change of type—of the advent of an asthenic phase—in diseases, was suggested, as an explanation of the altered practice, not at the time when venesection was abandoned, but long afterwards, and in some sort as an explanation of the altered practice. But unfortunately, and with a reaction common enough in medical

practice, men have rushed to the other extreme of regarding bloodletting under all circumstances as injurious. My present attempt, therefore, is, by defining the true uses and actions of venesection, to give to the remedy its rightful position in therapeutics.

CHAPTER VI.

Uses of Venesection.—Venesection relieves Pulmonary and Cardiac Congestions.—Venesection has no direct Influence over Inflammation.

I SHALL now proceed to apply the remarks made in the last chapter concerning the effects exercised by venesection, and by the direct abstraction of blood, over *external* inflammations, to the case of *internal* inflammations; and then consider the conclusions which result from such comparison.

You must, I think, admit that whatever is true of the effects of venesection, and of the direct abstraction of blood, upon an *external* inflamed part, must be equally true of its effects over *internal* inflammations; and for the reason, that the essential characteristics of inflammation are everywhere alike in all parts of the body. If, for example, venesection have no directly beneficial influence in the cure of an inflamed ankle-joint, neither can it have any directly beneficial influence over an inflamed lung. And so, again,

if the *direct* abstraction of blood is beneficial in the case of external inflammations, equally beneficial should it be in the case of those internal inflammations, in which it can be practised.

Now, this *à priori* reasoning of what should happen in the case of internal inflammations is, I believe, thoroughly corroborated by the results of practical experience. This I will now endeavour to show you. And, perhaps, I may make the matter clearer if I at once state the conclusion to which, as it seems to me, reason and experience inevitably lead us. It is this: That venesection has no directly beneficial influence over inflammations, either external or internal; that venesection is useless in the cure of all external inflammations; that its beneficial effects are distinctly marked in those cases of internal inflammation only in which the play of the organs of respiration or circulation is seriously impeded; and that, in such cases, its beneficial effects are not to be ascribed to any direct influence exercised by it over the local inflammatory process, but to its power in relieving obstructions to the play of the respiratory and circulatory organs. In other words, the bleeding operates, not primarily and directly on the inflammation, but secondarily—*i.e.*, upon the con-

sequences which arise out of, are incidental to, and, in fact, the results of, the inflammation.

I arrive at this conclusion for the following reasons. In the first place, we see that venesection has no markedly beneficial effect over external inflammations; and we know also that venesection can neither remove the cause of a local inflammation, nor remove any of the products of the inflammation. And it is a significant fact, that to this day no one has ever been able to tell us in clear and satisfactory language what that local beneficial effect is which is said to be produced by the bleeding over the inflammation, or in what way the hypothetical good is effected by the bleeding.

Moreover, it has ever been admitted that the beneficial effects predicated of bleeding are most marked in those diseases in which there is obstruction to the free play of the heart or lungs, and in proportion to the degree of the obstruction; and it is a fact that, at the present day, bleeding is never resorted to, except in those cases in which such obstructions exist. Bleeding has, in fact, been most highly lauded in that inflammation—viz., pneumonia—in which the obstruction and its consequences come on most rapidly and are most marked. Again, when bleeding gives relief, the relief is immediate,

comes while the blood is still flowing, and is just of that character which would result from removal of obstruction to the pulmonary or cardiac functions. The relief is just that which is given by bleeding in cases of chronic valvular disease of the heart, or in local injuries of the chest, where no inflammation is present. And the relief is given in pneumonia, even though the state of the inflamed portion of the lung remain absolutely unchanged; as, for instance, when it may be in a state of consolidation. It may be added, that no one has ever proposed to resort to bleeding in pneumonia in cases where the respiration and circulation are not obstructed. Such cases, it is said, do not require bleeding; though it is hard to understand why they do not, if the hypothesis, that bleeding has a direct influence over the progress of the inflammatory process, be true; for the destruction of a small portion of a lung may be as fatal as the destruction of a whole lung.

And it may, I think, be very fairly suggested, that the relief given is just the very same as probably would be given by bleeding in that state which is apt to come on in some persons after violent running, and from which probably all of us have one day or other suffered acutely; I mean what is called "stitch in the side." I

think it is more than probable that the pain we often ascribe to pleuritic inflammation in cases of pneumonia is rather the local nervous expression of the obstructed play of heart and lungs, of the character of the stitch alluded to, of the pain of angina; for it is a fact, that wherever be situated or discovered the signs of the pleurisy, the pain referred to is almost invariably localised at the lower part of the side of the chest.

These are the reasons which lead us, I think, to the conclusion that, in those cases of pneumonia in which bleeding is found of so much service, the good results mainly from the freedom thereby given to the play of the obstructed thoracic organs. There is no single argument, of a clear and satisfactory kind, which can be adduced in favour of the idea that the relief comes from any directly beneficial influence exerted over the inflammatory process.

And it is, I think, a very suggestive fact, that at this time of day, at all events, in this age of scientific medicine, there is neither thought nor question of bleeding, except in those cases in which the play of the lungs or the heart is impeded. Thus, for example, we are recommended to bleed in pneumonia and in heart-affections, and peritonitis, and certain head-affections; and here, I think, ends the catalogue of disorders in

which we are now advised that bleeding may be practised. But, in all these affections, the respiratory and cardiac functions are more or less hindered; and it is just in proportion to the degree of their hindrance that we measure the need of, and mark the benefits derived from, the bleeding. And it is in pneumonia, above all other diseases, in which the benefits of bleeding have been pre-eminently noted; and it is in this inflammation, in a pre-eminent manner, that we find the play of the cardiac and respiratory organs impeded.

CHAPTER VII.

When Leeches, etc., are of use in Internal Inflammations. —Leeches, Cupping, etc., may produce reflex actions. — Modern Ideas of Inflammation. — Inflammation not affected by Venesection.—Congestion.—Hunter's views of Bleeding, Local and General.

With regard to the effects of the local abstraction of blood in internal inflammations, I believe the very same facts hold good as in the case of external inflammations. I think that the experience of every physician will lead him to the conclusion that leeches and cupping are not found so invariably beneficial in internal as they are in external inflammations; and that, in fact, they not unfrequently fail to give the relief expected of them. May we not fairly seek for an explanation of this in the anatomical relation of the part inflamed to the surface of the skin whence the blood is taken? Thus, for example, leeches and cupping, applied over the thorax in pneumonia, sometimes do, and sometimes do not, give relief. When relief is given through loss of blood, may we not fairly conclude that in such cases the leeches have drawn blood directly from

the inflamed pleura, which so frequently accompanies pneumonia? Just so, again, in pericarditis, I would suggest that the removal of the pain, etc., by leeches or cupping, results from the direct abstraction of blood from the inflamed pleura; local pleurisy (as we know) being an almost invariable associate of severe pericarditis. I might add, also, what I have not seen noticed in this sense, that a branch of the internal mammary artery, which supplies the skin and intercostal arteries over the præcordial region, is distributed to the pericardium, forming a direct vascular connexion between the skin and the pericardium.

Leeches, again, give relief in peritonitis; and, I believe, for a precisely similar reason. In all these cases, there is a direct capillary communication between the skin from whence the blood is drawn and the parietal layers of the serous membranes spoken of. Leeches, again, applied over the liver, often give ease to pain felt there; and what is a more common *post-mortem* sign in old liver-affections than adhesions between the anterior surface of the liver and the parietal layer of the peritoneum—clearly indicating that this parietal layer had once been inflamed?

Medical men generally have some indistinct idea that, when they apply leeches or cupping

over the seat of an inflamed lung, the abstraction of blood has some specific or peculiar influence, other than what it would have if the blood had been taken from any other or distant part of the body. It is forgotten that, in such case, although the leeches may be near the seat of the lung-inflammation, the abstraction of the blood can only operate upon it just as venesection would—viz., through the general circulation; so that, in truth, the blood might just as well have been taken from the thigh or the back of the neck, so far as the mere abstraction of blood is concerned in its operating upon the inflamed lung. But we can readily understand that the leeches may give relief in such case, when they draw blood from an inflamed parietal pleura, accidentally accompanying the pneumonia. What is the relief given in such cases? Surely, in the main, it is the removal or diminution of local pain. And what more reasonable, than that the pain of pleurisy should reside in the inflamed parietal layer of the membrane? and that the direct abstraction of blood from the skin over the part should remove the pain, just as we observe it to do in the case of external inflammations, as already spoken of?

Let me give another example in this kind. We have generally a very firm belief that the

abstraction of blood, through leeches or by cupping applied over the loins, is of great service in certain kidney affections; but I think a careful examination of all the facts touching the matter throws great doubts on the opinion.

In the first place, it is manifest that no drop of the blood which is taken comes from the kidney; the blood is removed from the mass of the blood in the system, and its removal can only operate on the kidneys indirectly—*i. e.*, through the general system. In fact, as far as the mere abstraction of blood is concerned, the kidneys cannot be affected otherwise than if the blood had been taken from the arm or from the nape of the neck. I must, indeed, confess surprise that the practice of local bloodletting in cases of this kind has not been more severely questioned, merely out of anatomical considerations. I shall, of course, be told that experience here excludes any necessity for theoretical arguing; and that the benefits derived from the practice are too manifest to be doubted. But to this experience I must demur. I must, for example, beg those who insist upon the advantages of drawing blood by cupping and leeching from the loins in these kidney-affections, to remember—1. That patients who are thus treated are also invariably subjected at the same time to the most powerful of all

remedies in such cases—viz., rest, warmth of bed, baths, equable temperature, and so forth; and 2. That it is a fact also of experience, that the lumbar pain and other bad symptoms attending kidney affections, will disappear under the influence of the treatment referred to, and without either cupping or leeching.

Time forbids me pursuing further this most interesting subject; but I would earnestly ask my auditors to test by serious consideration the propositions here laid down, and not hastily to consign them to oblivion, as unworthy the attention of practical men.

The general propositions, summed up, are these: That in all those cases of internal inflammations in which there is a direct capillary connection between the skin and the internal part inflamed, the local abstraction of blood is of manifest service, just as we see it to be in external inflammations; but that, in all those inflammations in which there is no such capillary communication, the benefits of the local abstraction of blood are neither clear nor positively ascertained.

In saying this, however, I do not deny that leeching and cupping may have some beneficial influence over internal inflammations, other than that which is supposed to result from the ab-

straction of blood. In truth I believe that it has such influence. The discoveries of Claude Bernard, and the experiments of Brown-Séquard and others, have opened to us an entirely new field of pathological observation in the matter of inflammation—a field which is as yet almost untouched. The main—the prominent—fact in inflammation is the enlargement of the blood-vessels of the part inflamed; that is to say, in other words, an increased amount of blood supplied to the part. And the experiments referred to have shown that the vessels of a part may be influenced, as regards their contraction and dilatation, through the vaso-motor nerves. If the mere dipping of one hand in water will so alter the calibre of the vessels of the other hand as to change its temperature, why may it not be argued, that the application of leeches, etc., to the thorax sets up a reflex action, not by abstracting blood, but by the local irritation which they produce, and such a reflex action as shall influence through the vaso-motor nerves the vessels of the inflamed lung—for instance, produce a contraction of them, and so, it may be, tend to reduce the supply of blood through them and in the inflamed part, to its natural amount? If there be not some such action as this exerted over the vessels of the lungs by irritation of the skin,

how are we to account for the undoubtedly beneficial effects of plaisters, blisters, etc., applied to the thorax in bronchitis and chronic affection of the lungs? Dr. Brown-Séquard has shown that external irritation will produce, through reflex actions, contraction of distant capillaries within the body; and Claude Bernard has demonstrated, that redness and swelling and heat—*i. e.*, enlargement of capillaries—may be produced by injury of the sympathetic nerve. By division of the splanchnic nerves, he has, we are assured, produced pleurisy and other visceral inflammations.

With such facts before us, I think it may be very fairly suggested, that any good effected by leeches, etc., and local irritation of the skin, over those internal inflammations—in which there is no capillary communication between the skin and the inflamed part—may be ascribed to the excitement of a reflex action of the vaso-motor nerves of the part, producing contraction of the inflamed capillaries. An agent, said Hunter, having the power to contract the vessels, would probably be a specific in inflammations.

It is not my intention to enter into the interminable tale of inflammation; but I must observe, that no distinct and comprehensible explanation of the *modus operandi* of bleeding in

inflammation, in accordance with our modern pathology, has yet found its way into general acceptation. In the minds of most of us, I believe, the idea attaching to the old phlogistic theory of inflammation is that which generally prevails—the idea, viz., that the focus of inflammation is a fire; and that, by cutting off the supply of blood, we reduce the fuel, and consequently the conflagration.

But we err, I venture to think, through not having modified our views in accordance with the teachings of modern physiology. Physiology and pathology have certainly not yet bared to us the arcana of an inflammatory process—the *fons et origo mali;* but surely the experiments alluded to have given us some additional information, and that the kind of information—the best of all—from which the man of practice may glean a hint while engaged at the bedside of his patient. We have learnt, for example, that lesion of certain nerves which supply the blood-vessels destroys their contractility, and so allows their undue dilatation under the influence of the heart's pressure, and consequently an undue amount of blood to flow through them; and that thus, by nervous lesion alone, all the main characteristics of inflammation may be produced—viz., heat, redness, and swelling. And, again, we know

that excitation of certain nerves will produce contraction of those blood-vessels — the very opposite condition. Hence, then (as we know not what the element is which occasions the nervous lesion in inflammation), we may reasonably consider that this nervous lesion is, practically, the anatomical starting-point of inflammation.

Also do we know that an increased action is going on at the part inflamed—not a natural formation of healthy structures, but an excessive production of crude and imperfectly elaborated material; and fairly may we conclude that this excess of action depends upon the excessive supply of blood. The object of the practitioner, then, is to reduce this excessive production of ill-formed materials; to direct the inflammatory process through its natural course (if I may use the term), so that the parts shall be left in their original state of entirety when the inflammatory action has ceased; and reasonably enough he concludes that, by regulating the supply of blood, he can in some degree regulate the inflammatory action.

And here it is, in teaching us how to attain this object, that physiology has given us new knowledge. If we cannot remove the original exciting element of the inflammation—the cause,

we suppose, of local nervous lesion—still we may direct our art to the removal of the next link in the chain of the diseased process; viz., the relaxation of the blood-vessels—the result of the nervous lesion, and the main and most efficient element in the inflammatory process; using those methods of cure for the purpose which will tend to the increase of the contractility of the blood-vessels. For example, cold is a well known remedy in inflammation; and cold is an excitor of the vaso-motor nerves, and thus it is of service in external inflammations.

Now the excellent effects of the local abstraction of blood, as I have said, may be in part accounted for in a similar way. Besides relieving tension, the leeches and the cupping, by the irritation which they occasion, may also excite contraction of the partially paralysed blood-vessels of the part inflamed. But there is no proof that venesection does anything of the kind. By diminishing the contractile force of the heart, it temporarily diminishes the supply of blood to the inflamed part, as it does to all other parts of the body; but there is no proof that it, in any other way, influences the inflammatory action—tends to its cure—*i. e.*, tends to the contraction of the blood-vessels. The blood-vessels are emptied; but their loss of contractility is not repaired. A

striking illustration of this is given by M. Marey from Gerdy's method of treating phlegmonous inflammations by raising the inflamed limb. "Frequently," says he, "have we seen the limb, when brought down to the horizontal, rapidly resume an exaggerated size, and its coloration intensified, and its heat increased, in consequence of the atonic relaxation of its vessels." (Marey, 321.) Position here, like venesection, had merely assisted in preventing the temporary flow of blood to the part; it had, like venesection, done nothing towards improving the tonicity of the blood-vessels. As Hunter truly said: "Neither bleeding, sickness, nor purging can possibly lessen the original inflammatory disposition; by lessening the power of action of any disposition, you only lessen or protract the effects." We read, it is true, of the cutting short of inflammations by venesection; but I apprehend that no one can produce an example of the fact. In the flourishing days of bloodletting, men were bled till the congested eye became pale, and the rheumatic joints and erysipelatous limbs lost their vivid red colour; but I believe that the most heroic (as the term was) bleeder never pretended that he had thereby for once and all removed the inflammation.

The action of venesection over the vessels of

the inflamed part is totally different from that of the local abstraction of blood. True, a temporary emptying of the partially paralysed vessels may be produced, or rather the blood disappear from these vessels; but such emptying of the vessels results solely from the lessened force of the heart's action, and from the diminished quantity of blood which is consequently projected into them. No contraction is produced by any excitation of the vaso-motor nerves; there is, in fact, no increase or induction of the healthy and natural contractility of the vessels, such as is effected by cold, or may be effected by leeches, etc., but merely a less distension of them. And the action is merely temporary, lasting only until the heart has regained force enough once again to distend those morbidly dilatable blood-vessels.

But by the local abstraction of blood, not only is blood, and depraved blood, drawn from the part inflamed, but also, in all probability, the action of the vaso-motor nerves is excited, and a healthy contraction of the blood-vessels thereby occasioned. Was not this, indeed, the effect which Hunter's wonderful sagacity anticipated when he tells us: "Bleeding in the part inflamed, I can conceive, does more than empty the vessels mechanically; it acts by continued sympathy." To remove the blood from the distended

vessels of an inflamed part, large bleedings, as a rule, are requisite—such bleedings as have an effect upon the system—make the man faint; and, to maintain the vessels in such a state, the bleeding must be again and again resorted to. Moreover, we must keep in view the fact that the inflammatory process has a local excitant; and that the distension of the blood-vessels (for the relief of which, be it remembered, the bleeding is especially employed) is due to some morbid influence exerted at the part, and still present and operating there. And, therefore, again to use the very words of Hunter, "the lessening of the quantity of blood, considered mechanically, can never remove a cause which neither took its rise from, nor is supported by it."

From all this, it would appear, that we cannot by bleeding remove either the cause of the inflammation, or the inflammation itself; and that to attain the object desired by venesection, the removal of blood from the inflamed parts, large and repeated bleedings are necessary; such as may tell injuriously upon the constitution of the patient, and so, indirectly, upon the cure of the inflammatory process. And, moreover, that, when we have removed the blood by venesection and have empties the vessels of the part, we have done nothing towards the cure of the inflammation, as we

have done nothing towards producing a healthy contraction or tonicity of the blood-vessels. Modern physiology, to use the words of M. Marey, "indicates that, in the treatment of inflammation, we must follow a new path. Instead of combating local inflammations by bloodlettings which exhaust the whole system, it points out to us that we should ever, as far as possible, resort to local remedies."*

* M. Marey speaks of following a *new* path! He forgets that John Hunter had already long ago described this path. "When bleeding is necessary," says Hunter, "it should be as near the part as possible, in order that it may have the greatest effect on the part with the least damage to the constitution." And again: "In all cases where it can be put in practice, bleeding in or near the part will answer better than taking the blood from the general habit, for certainly less may be removed in this way, so as to have an equal effect upon the part inflamed. Bleeding in the part inflamed, I can conceive, does more than empty the vessels mechanically; it acts by continued sympathy."

CHAPTER VIII.

The Disuse of Bleeding an Error.—Venesection only of use in Pulmonary, etc., Congestions.—It relieves the Heart and Lungs.—Cases illustrating the Benefits of Bleeding.—Concluding Remarks.

No one pretends that bleeding is of service when it weakens the constitution of the patient. On the contrary, wise physicians have always warned the profession against such abuse of the remedy. In truth I might say that we admit bleeding to be of service in inflammations, then only, when it produces no injury to the constitution; or, in other words, that our masters permit us to practise venesection to the extent in which it can do no harm. Wherein can we find, indeed, a more demonstrative proof of the uselessness of venesection as a cure for inflammations, than the positive fact that we of this day cure them without resorting to it? Men have, in all ages, warned us against the dangers of venesection, as well as told us of its virtues; but we of this advanced generation speak much of its injurious effects, and but little of its beneficial uses.

Surely all these considerations narrow into very small limits the value of venesection as a cure for inflammation, and throw the greatest suspicion on the praises which have been showered on the remedy as such. From whatever side we consider the subject, historically, practically, or physiologically, we still find the proof of the proposition, that venesection is of use in the cure of inflammation, most defective.

But, on the other hand, when we come to regard bleeding as a remedy, not of the inflammation, but of some of the secondary consequences which incidentally arise out of inflammations, and of certain diseases not inflammatory—then all the clouds of difficulties which obscure the subject vanish, and clear light breaks in upon us. We then see how it is that the wise men of past days have lauded and resorted to the remedy; we then find, physiologically, an explanation of its mode of action; and practically can positively note its uses. And, I may add, we are then driven by the force of demonstration to the conclusion (which it is my object here to enforce), that bleeding must at all times have been an excellent remedy; that it is so now; and that its general disuse is to be deplored—history, physiology, and practice, all coming in and reading us lessons corroborative of the conclusion.

Venesection is, I maintain, not a remedy of inflammation, but a remedy of the accidents which accompany, or, rather, arise out of, certain inflammations and non-inflammatory diseases; viz., those inflammations and diseases which are accompanied with obstructions of the cardiac and pulmonary functions. *It is, therefore, of service, in those inflammations only which are attended with such obstructions.*

Let me repeat, in brief, what I hold to be the proofs of this proposition. In all ages of medicine, venesection has ever been most especially lauded as of service in those affections which are attended with such obstructions to the play of the thoracic organs. As science advanced, and men came to understand better the special character of diseases, they began to bleed less; first, they gave up the practice in external inflammations, under the teaching of facts *oculis subjecta;* and then, at length, in all other inflammations, excepting in those attended with pulmonary or cardiac obstructions. At the present time, there is never any thought of bleeding, except in the class of affections alluded to. Is not the conclusion obvious?

The very symptoms, let me remind you, for which venesection is practised in such cases, are the very symptoms which are representatives to

us of pulmonary and cardiac obstructions. The very relief given by the bleeding is just the kind of relief which, we know, experimentally results from venesection in such cases. The relief given by bleeding, when practised in pneumonia, is the very same relief as that which is given by bleeding in chronic cardiac and thoracic aneurismal affections. It is precisely the relief which naturally flows from the removal of mechanical impediments to the respiratory and cardiac movements; and the relief is given, although the physical conditions of the local inflammation in the lung remain unchanged. The relief, again, is in all cases immediate; while the blood is still flowing, it is profoundly felt, if the bleeding be of service at all.

Moreover, no one thinks of resorting to venesection in pneumonia, if the inflammation be so limited as to produce little or no impediment to the respiratory or cardiac functions. Yet, if bleeding be a good remedy for the cure of the inflammation of the lung *per se*, why is it not as requisite when the lung is very partially as when it is very largely inflamed? The destruction of a small portion of the lung may be as dangerous to life as the destruction of an entire lung.

But the mere removal of the obstruction to the play of these organs is not the only good which

results from the venesection. I need not stop to tell all the long series of beneficial consequences which follow the bleeding. The impeded functions are all of them relieved through the freedom of action given to these vital organs. To interfere with the play of the heart and lungs, is to interfere, *pro tanto*, with the proper functional activity of every organ of the body. Nor is it needful for me to discuss more nearly how the relief given in such case is effected by the bleeding. Enough for my argument is the fact that it is given. I will only observe, that it seems to me to be, in the main, of a mechanical character; by the withdrawal of blood, relief is given to the distended, oppressed, and, I may say, of necessity, partially paralysed heart. When pneumonia extensively pervades a lung—and it is only, remember, in such severe case that bleeding is ever needed as a remedy—suddenly a large extent of the pulmonary aërating surface is rendered useless; and as the amount of blood in the body remains unchanged, there is all at once established an undue relation between the mass of the blood and the aërating surface. The result is obvious. One lung is suddenly called upon to do the work of two; the blood is obstructed in its passage from the heart to the lungs, and the right side of the heart begins to

labour and becomes engorged. But engorgement and dilatation of the heart is, of necessity, *pro tanto*, obstruction to, and partial paralysis and loss of natural force, of the heart; and all these evils increase *pari passû* with the degree and duration of the obstruction.

Leaving out of consideration all those other evil consequences which necessarily flow from this one, may it not be reasonably argued, that the relief of this condition is the main, the starting-point of the benefits accruing from venesection in pneumonia? The suffering which finds relief through the bleeding in pneumonia is, I take it, the very same in kind as that which is the expression of the agony of angina; and in both cases, as I argue, it takes its origin in an oppressed and partially paralysed heart. In angina, the cause of the paralysis of the organ lies within its very self—in its enfeebled and degenerated muscular walls; whereas, in pneumonia, it finds its source from without; viz., in the obstructed pulmonary circulation. And just so, likewise, do we find that venesection, which relieves the one affection, is worse than useless in the other.

We find also, indeed, that in cases of heart obstructions, whatever their cause, and that in pneumonia, there are periods when venesection will

no longer avail to give freedom of action to the enfeebled heart; then, indeed, when its partially paralysed force is paralysed beyond redemption.

The experiments of Dr. John Reid, touching asphyxia, also surely corroborate the view I am maintaining. He showed that when the heart had ceased to beat in asphyxiated animals—had become paralysed through over-distension—its action might once again be renewed, and even so as to preserve life by giving exit to the blood from the engorged right side of the heart.

It may seem strange that, in the latter half of the nineteenth century, I should offer within these walls illustrations of the uses of bloodletting; but, in consideration of the prejudice existing against the remedy at this time, I may, perhaps, be pardoned if I relate a few examples of what I cannot myself do otherwise than regard as demonstrative of the life-preserving effects of venesection—examples showing the incorrectness of the theory, that diseases will not bear bleeding now as they did formerly; and examples of the fact, that a dread of bleeding is at this moment prejudicial to the interests of the sick.

Not long ago, I saw a female suffering from severe pneumonia, so that her life was considered in danger. To my view, she was in that condition of body, and presented those signs which

strongly indicated the propriety of venesection. The gentleman in immediate attendance upon her received the proposal with dismay. He at last, however, gave his consent to the operation, on the condition that his hands were to be washed of the expected results. I will only say, that the patient was largely bled, was thereupon immediately relieved, and made an excellent recovery; the marked change in her condition admittedly dating from the moment of taking the blood.

The next case I will give is one of chronic disease of the valves of the heart. The subject was a large and powerful man. When I saw him, his legs were œdematous, his abdomen dropsical, and from the great oppression of his breathing, it was manifest that his lungs were already invaded with effusion. This was his first attack in this way; and all these severe symptoms of heart-affection had come rapidly on him. During a week he had been subjected to treatment; but neither diuretics, nor purgatives, nor diaphoretics, etc., had availed to relieve him. Indeed, if ever there were a case of the kind, in which early dissolution might be predicted, this surely was one. I then and there had him bled, or rather bled him myself, as the young surgeon in attendance had never seen the operation performed. About thirty ounces of blood were

taken. The relief was unmistakable; it was immense and immediate; and I will only add, that on the very next day he was up, walked about on the third day, and made a complete temporary recovery from the dropsical attack in question.

The next two cases I will give are from the surgical practice of my colleague Mr. Walton, who has, like myself, no prejudice in favour of a present asthenic phase of humanity. A man of seventy-eight, hale and temperate, fell from a cart, and did or did not break a rib or two. In two or three days, pain on inspiration was felt in the back; purgatives and opium gave no relief. On the eighth day, the pain was intense and killing, the dyspnœa great, and the face distressed. Mr. Walton now saw the man for the purpose, I understand, of deciding and taking the responsibility of venesection. The patient was at once largely bled; the blood being allowed to flow until the breathing became easy. The relief was immediate and permanent.

In another case, the ribs were also broken; and as the symptoms, in the course of three days, became of an alarming character, Mr. Walton was called in to decide whether venesection might be resorted to, the patient being sixty-eight years old. When Mr. Walton saw

the man, he had been sitting for two days bolstered up in a chair; his legs œdematous; his lips blue and congested; and the gentleman in attendance upon him considered him to be dying. Mr. Walton immediately bled the man largely—to about thirty ounces. "The result was most satisfactory," the report runs, "enabling the patient to resume the horizontal position; and, from being asphyxiated and at the point of dissolution, gradually to improve and recover."

I could, sir, give you other cases of a like character, and of quite modern date, if it were necessary; but it would be merely repeating the same tale. I know that, in cases of this kind, many members of the profession would at once resort to the remedy; but I am equally certain that the majority of practitioners, however much their better judgment might incline them to the right conclusion, do hesitate, perhaps fatally hesitate, at the present moment, in resorting to venesection in such cases. The phantom of asthenia rises before them, and paralyses their actions. I have attempted to show that this phantom—this change of type in diseases—is a theory unsupported by satisfactory and trustworthy data; that the idea so widely spread and so generally accepted, that men will not bear bleeding now as formerly, is unfounded in fact, and contrary

to experience; and sincerely do I hope that I may have in some small way assisted in removing a delusion which, as I believe, banefully weighs upon the practice of our art.

I have also attempted to define the right limits and the true effects and the proper application of bloodletting in diseases; pointing out that, as it seemed to me, physiology, pathology, and experience all concur in teaching us, that venesection has no directly beneficial influence over inflammations; that it is only of service during certain stages of those inflammations in which the action of the heart and lungs is impeded; and that its use lies in removing the obstructions which arise incidentally out of such inflammations.

In dealing with this wide and unlimited subject, I have, of necessity, been forced carefully to avoid the temptations which have beset me on every side; I mean the discussion of the many interesting topics, incidental to the tale of bloodletting, which present themselves to him who treats thereof. I have strictly confined myself to two especial points; viz., the attempt to show the fallaciousness of the change-of-type theory; and to the explanation of the real uses of bleeding in inflammation; believing firmly that the removal of the theory and the accept-

ance of the explanation would necessarily involve, with all reasonable people, a reconsideration and a readoption of venesection as an ordinary remedy in diseases. This, sir, must be my apology with those of my hearers who may think I have treated the question in too exclusive a spirit.

Allow me, sir, in conclusion, to say one word of apology. I fear that I may seem at times to have expressed my own views too dogmatically, and too lightly to have estimated the value of the opinions which I opposed. Should I have fallen into this error, let me assure you that it was not from any want of due respect to those whose opinions I naturally regard so highly, but solely from the earnestness of the conviction with which I have attempted to impress my own views upon your notice.

THE END.

LONDON: T. RICHARDS, 37 GREAT QUEEN STREET.