

Illustrations of the enquiry respecting tuberculous diseases / by John Baron.

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ILLUSTRATIONS
OF THE
ENQUIRY
RESPECTING
TUBERCULOUS DISEASES.

BY
JOHN BARON, M.D.

PHYSICIAN TO THE GENERAL INFIRMARY AT GLOUCESTER.

LONDON:
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TO
EDWARD JENNER, M.D. LL.D. F.R.S. M.N.I.F.
&c. &c. &c.
AND
PHYSICIAN EXTRAORDINARY TO THE KING.

MY DEAR FRIEND,

RECEIVE, AS A MARK OF MY RESPECT AND GRATITUDE, THE FOLLOWING ILLUSTRATIONS OF A SUBJECT WHICH HAS LONG AND DEEPLY ENGAGED YOUR ATTENTION. AIDED, AS I HAVE BEEN, BY YOU IN MY ENQUIRIES, IT WILL AFFORD ME THE TRUEST SATISFACTION, SHOULD THIS, OR ANY OTHER UNDERTAKING OF MINE, CAUSE YOU TO FEEL THAT YOUR KINDNESS HAS NOT BEEN MISPLACED.

THAT YOU, WHO HAVE BEEN FAVOURED, ABOVE ALL YOUR BRETHREN, BY THE UNPARALLELED BENEFITS WHICH YOU HAVE BEEN ENABLED TO COMMUNICATE TO YOUR FELLOW-CREATURES, MAY LONG ENJOY THIS YOUR MOST JUST AND PRECIOUS DISTINCTION, IS THE FERVENT WISH OF

YOUR DEVOTED AND GRATEFUL FRIEND,

JOHN BARON.

NOVEMBER 26, 1822.

TO

HOWARD JENKINS, M.D., LL.D., F.R.S., M.I.R.

Esq.

AND

PHYSICIAN EXTRAORDINARY TO THE KING

My dear Sir,

I have the honor to acknowledge the receipt of your letter of the 14th inst. in relation to the case of the late Mr. [Name] and to be assured that you will be satisfied with the result of the examination. I have the honor to be, Sir, your obedient servant.

I have the honor to be, Sir, your obedient servant.

Yours faithfully,

JOHN HARRIS

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ERRATA.

Page 9, line 6, for insinuarant *lege* insinuarunt.

Page 11, line 19, for containing *lege* contained.

INTRODUCTION.

IN bringing forward the following Illustrations, the Author feels it but justice to himself to observe, that he has no attachment to any of the doctrines which they are intended to explain and confirm, but because they appear to him to be founded upon sound physiological principles, and accurate pathological details.

The opinions at one time embraced by him, were different from those which he now advocates. This avowal can give them no additional weight; but it may at least convince the reader, that they were adopted, not hastily, but on solemn conviction, after mature and impartial enquiry. What has happened in his own case, possibly may occur to others; and he seeks for nothing more from him who may peruse these pages, than a patient and unprejudiced

examination of the facts which may be presented, and such a moderate degree of attention as may enable him to comprehend the reasonings which are founded upon them.

Many things, doubtless, will remain obscure and unexplained, partly from the difficulty of the enquiry, but more, perhaps, from the deficiencies of the Author. The candid searcher after truth, it is to be hoped, will not be discouraged by such obstacles, nor turn aside from seeking further information on a very interesting subject, even though he may find some matters difficult to be understood, and others seemingly at variance with his previous knowledge. If he should have resolution enough to persevere, in opposition to such hindrances, no fears need be entertained as to the result.

In the progress of more advanced investigations, some of the Author's opinions may be proved to be erroneous; but let not that affect the subject on which he has presumed to write. It will still remain in all its vastness; and it will be to him a source of sincere thankfulness, if even his errors and deficiencies shall awaken the attention of abler

enquirers, and thereby accomplish what it is his object to attain.

In attempting to establish his own opinions, he has been called upon to examine with minuteness those of others. This part of his task has afforded him little satisfaction. It has shewn him the numerous obstacles which retard the advancement of medical knowledge. Too many of these are connected with the nature of the science itself; others are unquestionably to be referred to faulty methods of pursuing our investigations, and recording our experience. The evils arising from the last mentioned source, are of greater magnitude, and have a much more extensive influence, than at first sight might have been conceived. They are connected with some of the "*diseases and distempers*" of the understanding, which have been justly held to be, of all others, most fatal to the cause of science. All medical men who regard their own reputation, and who are desirous of rendering their observations useful to those who may come after them, ought to look well to this: there is a personal, as well as a general interest involved in it, which should cause it to be well considered.

It may still, perhaps, be said of “medicine, that it is a science that has been more professed than laboured, and yet more laboured than advanced; the labour having been rather in circle than in progression.” We have not, it is true, of late neglected to trace the footsteps and impressions of diseases; but our meditations, opinions, and doctrines, have too often been infected with some conceits of our own, which have withdrawn us from the right contemplation of nature. But most of all, perhaps, have we been deluded by the false appearances which are imposed upon us by words. They mightily entangle and pervert the judgment; so much so, that “if, in our controversies and disputations, we were to imitate the wisdom of the mathematicians, in setting down from the very beginning the definitions of our words and terms, that others may know how we accept and understand them;”^{*} many needless disputes might have been avoided, and the progress of our art rendered much more certain. As, therefore, “*idola fori†* omnium molestissima sunt,” it behoves us, in

^{*} Bacon. Advancement of Learning, *passim*.

[†] Sunt etiam idola tanquam ex contractu et societate hu-

an especial manner, to be on our guard against them. They seem, in a surprising degree, to have enslaved the minds of medical men, and to be the parents of those unsound and unlawful applications of words and phrases, which are ever and anon springing up to confound our reasonings, and to vitiate our observations.

It is not without cause that these remarks are delivered. They may be more applicable to times that are past; but there are too many instances to justify their introduction, even though we do not look beyond the writers of the present day. Without presuming to think that he is blameless in this matter, the Author has had, in the following pages, many occasions to allude to the faults in question. It was very needful to notice them on their own account; but it was more so, perhaps, in

mani generis ad invicem, quæ *idola fori*, propter hominum commercium et consortium, appellamus. Homines enim per sermones sociantur; at verba ex captu vulgi imponuntur. Itaque mala et inepta verborum impositio miris modis intellectum obsidet. Neque definitiones aut explicationes, quibus homines docti se munire et vindicare in nonnullis consueverunt, rem ullo modo restituunt. Sed verba plane vim faciunt intellectui, et omnia turbant; et homines ad inanes et innumeras controversias et commenta deducunt.

Id. *Novum Organum*, Lib. i. sect. 43.

as much as they occur at a period of boasted advancement in pathological science. We may well question the reality of this advancement, when it is accompanied by such tokens of unsoundness; and if we expect to secure, for the benefit of posterity, that knowledge which we may actually acquire, we must take heed that it be delivered in a manner more consonant with the rules of sound judgement, and better adapted to facilitate the progress of science.

When we look at the various hypotheses which rapidly succeed each other in our profession; when we see the same phenomena presenting themselves in perfectly different shapes to different individuals, one man contending that they denote one thing, and another something of an opposite nature; when we see the accounts of diseases, not regulated strictly and faithfully by actual occurrences, but by some pre-conceived and conjectural notions touching their nature; when we see what is correct, and what is false, so intermingled together, as to render it impossible to extract the truth, we may well feel some degree of anxiety for the character and usefulness of medical records. Let those, therefore, who are zealously bestowing their

time and their talents in the prosecution of professional knowledge, bethink themselves of what is required for the success of their designs ; let them remember how they turn away, with aversion and disgust, from the ponderous pages of some of the older writers, because they are disfigured by such faults as we have pointed out ; and that possibly the lighter productions of the present day, though less encumbered by misplaced erudition, and better fitted by their number and variety, to gratify for the moment our inconstant tastes and judgments, may nevertheless be fraught with elements which will as surely destroy their influence with our successors, and will as little aid the cause of genuine knowledge, as the many writings which have now sunk into oblivion.

It is impossible to tell how much that is really valuable, has been lost, from being thus incorporated with perishable ingredients. The nature or “ method of tradition, is therefore not only material to the use of knowledge, but likewise to the progression of knowledge ; for, since the labour and life of one man cannot attain to the perfection of knowledge, the wisdom of tradition is that

which inspireth the felicity of continuance and proceeding.”*

There is something in this “wisdom of tradition” which medical men, in general, it is to be feared, but little consider. If they did, fluctuations of opinion would be less frequent and violent than they are, and medical facts and medical conjectures would not stand so much upon the same footing. We have many words, and not much advancement of real knowledge; we have much diversity of opinion, and, perhaps, but little solid enquiry. There still seems to be a kind of “contract of error between the deliverer and the receiver; for he that delivereth knowledge, desireth to deliver it in such form as may be best believed, and not as may be best examined; and he that receiveth knowledge, desireth rather present satisfaction than expectant enquiry, and so rather not to doubt, than not to err.”†

How much soever this most just reproach may have been removed from some sciences, it cannot be denied that it still is, in a striking degree, ap-

* Bacon. Advancement of Learning, Book ii.

† Idem.

plicable to medicine. While searching for truth in that branch of knowledge which is to be in part discussed in the following pages, too many opportunities were afforded of proving this statement. The Author has in vain looked for that "wisdom of tradition," which Lord Bacon has so much insisted on. Instead of this, he has seen, in the conflicting statements of living authors, much cause to suspect that our progress in knowledge is not so great as has been supposed; and that in the school, where such boasts are most frequently made, the branch of philosophy which we are now considering, is less esteemed than it ought to be.

It is astonishing, how much the true method of interrogating nature is misapprehended by many, who think they are pursuing the path which is pointed out by an enlightened philosophy. A recent writer tells us, in his preface, "that the art of making researches in medicine is almost reduced to a sort of mechanism; and that for this purpose, a man needs no more than patience and the use of his eyes."* The character of this statement, and the fate of M. Bayle's re-

* *Vide Bayle, Recherches sur la Phthisie Pulmonaire.*

searches, will afford a good opportunity of illustrating my present argument.

The French showed an early desire to pay respect to the author of the inductive philosophy, and to follow his precepts; but were this a fit occasion, it would be easy to prove that they have greatly deviated from that spirit which it was Lord Bacon's object to infuse into every branch of human knowledge.* The words which we have just quoted, afford a striking proof of the little familiarity that has been acquired with those helps, which had been devised by that great author for aiding the judgment, and for purifying it from those errors by which it is apt to be infected. The passage gives a most erroneous and defective statement of the principles that ought ever to guide the philosophic observer. It would teach us to abandon altogether the *philosophia prima* "magna ista scientiarum mater;" it would prevent us

* The work *De Augmentis Scientiarum* was, soon after its publication, translated into French. This translation, it is affirmed in the *Baconiana*, contained many errors and omissions, and perverted many passages, especially those pertaining to Religion, and the right application of the Sciences. I have never seen this version; but, judging from the recent character of French Philosophy, and from the works of those

from reaching that height which commands a view of the field of human enquiries, and cause us to forget that it is impossible to “discover the more remote and deeper parts of any science, if you stand but upon the level of that science, and ascend not to a higher science.” Moreover, it is to be remembered that there are two sorts of experience, one of which “*nihil aliud est, quam (quod aiunt) scopæ dissolutæ, et mera palpato, quali homines noctu utuntur, omnia per-tentando, si forte in rectam viam incidere detur; quibus multo satius et consultius foret præstolari, aut lumen accendere, et deinceps viam inire.* At contra, verus experientiæ ordo primo lumen accendit, deinde per lumen iter demonstrat, incipiendo ab experientia ordinata et digesta, et minime præ-postera aut erratica, atque ex ea educendo axioma-

who treat exclusively of Lord Bacon’s system, there can be no doubt that they have most grievously misapprehended the spirit of his writings, and, by a daring perversion of his precepts, have realized the very evils which his Lordship dreaded, and which, in a prophetic appeal, he most solemnly deprecated. The passage to which I allude occurs towards the conclusion of the preface of the *Instauratio Magna*; and when we consider the period at which it was written, and the events that have occurred in our own times, it may well arrest the attention of every serious enquirer.

ta, atque ex axiomatibus constitutis rursus experimenta nova, quum nec verbum divinum in rerum massam absque ordine operatum sit.”*

We may discover to which of the foregoing classes the observations of M. Bayle belong, by estimating their influence upon his professional brethren in his own country. Had the mechanism of medical research, as he calls it, been as complete as he imagines it to be, it certainly would not be a “difficult matter, with suitable opportunities, to compose a useful book.” It is not meant to be denied, that his is a work of that kind; the question at present to be decided, is merely, whether the author’s principle is sustained by the result of his observations, whether it has conducted him to enlightened and satisfactory delineations of the subject which he had undertaken to discuss; or whether, in short, the additions which he has made to our knowledge, are such as will abide the scrutiny of after-times. This is a subject of very great consequence. It regards not mere differences of opinion in the minor details of professional investigation; it reaches to the very foundations of our art, and determines

* Bacon. *Novum Organum* Lib. i. sect. 82.

the value of all efforts that may be made to render it more perfect. Did it not appear to be highly expedient and necessary to bring these things to the recollection of medical men, they would not have been exemplified by a reference to the work of any living author. But as the errors in question, rest upon a principle that has been declared by the highest authority to be inaccurate in theory, and very pernicious in its application, it seemed wrong to pass them over unnoticed. At all times, and under all circumstances, it is needful to endeavour to protect the intellect against them; and there are abundant evidences, in the present state and past history of medicine, to prove how very much they still cling to our investigations.

M. Bayle was in a situation where almost unlimited opportunities of enquiry were afforded to him. His observations, it is to be presumed, were conducted upon the principle which has been already noticed; and what has been the result? He has given six species of Pulmonary Consumption; but so little of accuracy is there in this arrangement, that another observer of the same school, whose researches, it would appear, have been not

less minute and patient, tells us, a few years afterwards, that, of these six species, two are identical, or merely varieties of the same affection, and that the other four have nothing in common with Tuberculous Phthisis, farther than as they exist in the same organ; and that they rarely produce the effects from which that malady takes its name—that is to say, Consumption.

Contrarieties not less perplexing will be pointed out in the following pages, not only as they occur in the writings of different individuals on the same subject, but in those of the same author. To such an extent, indeed, are they to be met with, that it will defy the subtlest wit to extract any thing like consistent or intelligible doctrine from many celebrated works. It is not a slight difference that may be detected, but diversities of statements, peremptory, and irreconcilable with each other; and these, too, from men who profess an unbounded reverence and attachment to solid enquiry. We cannot but conclude, therefore, that the observers themselves had been misled; that the impressions, which they had received through the senses were fallacious, and their axioms and deductions not less so.

It cannot be necessary to repeat how much all wisdom in tradition is outraged in these instances. The desire of improvement is great, and the zeal of the enquirers is most praiseworthy; but whether it be always directed in the best manner, is a point that may be much questioned.

Very possibly, in making these remarks, the Author is supplying means of passing a heavy condemnation upon his own work. This he would be willing to bear, if he could satisfy himself that any thing which he has said, would rouse all who cultivate medicine, to a due sense of the importance of those principles of philosophy, which he has ventured briefly and imperfectly to call to their remembrance.

How different were the feelings and principles which actuated the minds of some of the distinguished men who have, in times past, successfully cultivated either physiological or pathological science, from those which are most generally adopted at the present day, and of which we have selected a specimen from a French writer! The great Haller, distinguished not less by his genius and his virtues, than by his patient and laborious re-

searches, knew well the caution that was required to correct the testimony of the senses, and the spirit in which observations ought to be made. He recognizes no such mechanism as M. Bayle alludes to ; and recommends a better and more scientific method of conducting our labours. “ Est in his omnibus ars quædam inveniendi, quæ breviter dici non potest, et quam paucis mortalibus natura concessit. Oportet absque præjudicio ad opus venire, non eo animo ut videas, quæ classicus auctor descripsit ; sed ea cum voluntate, ut ea videas, quæ natura fecit.”* To the same purpose are many of the remarks of our own Sydenham, and of other eminent writers. No one seems more fully to have felt their importance than Sauvage. His introduction to his Nosology is conceived in the genuine spirit of philosophy ; while the work itself, in spite of some faults, which could not be avoided at the time when he wrote, contains descriptions of diseases more accurate and more copious, than any other work of the same class. He justly appreciated the value of philosophy in enlightening

* *Elementa Physiologiæ, Præf.* p. 4. Edit. Lausannæ, 4to, Vol. i.

our path, and teaching us how to distinguish between a false and a true experience. He rightly estimated the value of a pure and simple method of tradition; and joins heartily with Sydenham, in deprecating that unnatural alliance between experience and conjecture, which so often vitiates the writings of medical men. “Error autem familiarissimus est judicium suum cum experientiâ confundere; hic vero admittitur error quoties phænomenum talibus verbis exprimimus, quibus aliquid vel tanquam causa, vel tanquam effectus prædicatur, imo tanquam principium alterius.”*

The extent to which the last-mentioned error exists at the present day, will scarcely be credited by those who have not been in the habit of using a logical method in their own pursuits. To expose it fully, would require greater time and space than can be here afforded; but since it has been met with so often, while these Illustrations were preparing, and it has been necessary to speak of it as a source of much confusion, it would be wrong to let it go unnoticed in this place.

* Nosologia Methodica, Vol. i. Prolegomena, p. 8.

One writer tells us, that tuberculous masses are formed by a species of "impregnation, or infiltration," of the pulmonary tissue. The words, "impregnation," and "infiltration," stand in the middle of a description of the mode of developement and progress of tubercles. As every word, in a scientific discussion, which has not a precise and definable meaning, must tend either to obscure the sense altogether, or to convey inaccurate and inadequate ideas to the mind, it is fitting to enquire, how the words just quoted perform the office which they ought to serve in their present situation.— Suppose, therefore, we were to request that they might be defined. The definition, it is to be presumed, would embrace that collection of simple ideas which each term shall stand for. The Author gives no such definition; his readers, therefore, must endeavour to extract the best meaning they can from them. We cannot foretell what that meaning might be; but, doubtless, it would be as various as are the significations of the terms employed. But even if we had definitions of the terms, though all obscurity on that head were completely removed, it would remain to be en-

quired, whether they are conformable to the things to which they are applied. In the examples before us, it may safely be affirmed, that they are utterly unconformable. They are obscure and equivocal terms, (at least in their relation to pathological subjects;) they can convey no precise knowledge to the mind, and can lead to nothing but error.

Another writer of the same school tells us, that the same disorganizations which, according to the former, are brought about by impregnation and infiltration, are accomplished after another manner; that they are the effects of chronic "irritation" of the lymphatic capillaries. It is quite sufficient to place these statements in opposition to each other, to enable the attentive reader to detect their deceitfulness and unlawfulness. It is not a little mortifying to observe them at this period. They are used by men, who profess to have obtained all their information from patient and accurate researches into phenomena which they undertake to describe. The term "irritation" has called forth a few remarks, which will be found in a subsequent part of this work; and, supported as they are by

the authority to which an appeal has been made, they doubtless will command the attention of the reader.

Seeing that medicine still labours under such imperfections, that, in our conversations, our discourses, and our arguments with each other, we are so much addicted to an ill use of words, it surely cannot be presumptuous to enquire, whether the alleged advancement in pathology be so great as some contend for. This question often obtruded itself upon the Author in the ensuing discussions; and, compelled as he was to endeavour to find truth amid many opposing assertions, he was naturally led into the train of reflection which gave rise to what has just been said. It is a subject which has much occupied his mind; and he has ever regretted that the rules of sound reasoning, which have been justly appreciated by some writers in medicine, should at this day be overlooked in a very surprising degree.

What has been delivered, seemed necessary to explain and justify the principles on which the Author's enquiries have been conducted, as well as to account for any appearance of a presumptu-

ous or controversial spirit, that some possibly may think is to be found in the strictures which he has deemed it expedient to introduce on the writings of his contemporaries. He earnestly deprecates any such interpretation of his conduct. He is quite unconscious of having written any thing but what was prompted by the most sincere love of truth. It was utterly impossible to pursue this object without noticing the errors that lay in his path. As was required, he has done this with freedom, but, he trusts, with perfect fairness; and if he has misunderstood, or unwittingly mistaken, any one, he will have much satisfaction in correcting any such error. Should it, on the other hand, appear that he himself has been guided by a false light, and that his reasonings are not sanctioned by subsequent researches, or that they have been inconclusive and fallacious, he will with regret, but not less sincerely, abandon them.

In his descriptions, he has endeavoured to avoid every figurative, every ambiguous expression; and every term of art that may be found, is, as much as possible, restricted to its own peculiar and exclusive meaning. He has honestly attempted to

acquire clear ideas himself, on the subjects of which he treats; and should he succeed in communicating them to others, his object will be gained. To prevent the chance of misconception from mingling points that admit of distinct and satisfactory elucidation, with those which are of a more doubtful nature, he has, on the present occasion, restricted his illustrations entirely to the progress of tubercles, without touching, except in an incidental manner, upon the question of their origin. That question, in his own mind, admits of very little doubt; and the direct proofs by which, at no distant period, it probably may be made clear to the comprehensions of all, are very likely to be obtained. He has spoken, in the Enquiry, of the indirect evidence; it is very strong, and comes much nearer to a demonstration than many parts of medicine, about which there is much less hesitation. This, however, and many other points, he, for the present, leaves as they are, being anxious, in the first instance, as far as in him lies, to fix securely the foundation on which the pathological doctrines rest. That object being gained, the subordinate parts of the discussion will all naturally

fall into their proper situations; and the obscure and contradictory notions, which still exist as to tuberculous disorganizations, may thereby be removed.

The time is not yet arrived for speaking with confidence, either respecting the prevention or removal of these diseases; but if their nature be really such as it appears to be, a reasonable hope may be entertained that we may acquire such knowledge as shall guide us in their treatment, with much greater certainty and success than heretofore. In the last chapter, a few observations will be found on this subject. They are far less complete than the Author could have wished; but they may supply hints, which others may carry to perfection. He has repeatedly alluded to the subject of Comparative Pathology; and he again takes this opportunity of bespeaking attention to it, in the most earnest manner. It will be seen, that many of the domestic animals are afflicted with tuberculous diseases, in all respects analogous to those of man. By taking advantage of the information which may be derived from such occurrences, very important accessions may be made

to the resources of our art. By having the animals much under our controul, we may not only be able to ascertain the causes which are most influential in generating or propagating such diseases; but we shall, at the same time, acquire knowledge of the means by which they are most likely to be counteracted.

In conclusion, the Author is desirous to obviate unfavourable impressions, which perhaps might be excited by the recurrence of corresponding statements in the following pages. Such repetitions could not very well be avoided, in treating of appearances which, from their multiplicity and variety, are apt to occasion confusion in the mind, and which cannot be properly understood without a frequent appeal to principles. During an examination of the opinions of different writers, a reference of this kind was especially called for; and if he has succeeded in extracting a consistent testimony from apparently conflicting witnesses, it is entirely to be ascribed to this process. Such a method of eliciting truth, he has deemed it proper to adopt on this occasion. Statements advanced by those who may be interested in a subject, are naturally enough

received with a certain degree of distrust; but when they are corroborated by the testimony of adversaries, or when the issue of the question is permitted to rest on the evidence of those who are perfectly unbiassed and disinterested, it is to be presumed that the cause may meet with a fairer investigation. The Author has been very much guided by this feeling, many of the proofs in the following pages having been purposely selected from the writings of gentlemen whose sentiments do not coincide with his own. By pursuing this plan, various important ends were gained. It enabled him to adduce evidence, which must be held to be unexceptionable, and by it to expose error, and, he would hope, likewise to substantiate truth.

Once more he has to beg, that no opinion may be formed of this subject from a partial or hasty examination, but that all its parts may have their due consideration; and that, without prejudice, they may be allowed to make their just impression upon the intellect. In a question of considerable obscurity, where an intricate process of reasoning is required, the Author can scarcely flatter himself that his expressions have been at all times so pre-

cise and perspicuous, as to exclude inaccuracy and ambiguity. He can only say, that he has been earnest in his desire to avoid such errors: should they nevertheless be found, he entreats that the blame which *he* thereby may incur, may not be permitted to influence the reader's judgment, but that he may investigate patiently, and be guided in his decision by the merits of the subject alone.

CHAPTER I.

ON THE PROGRESS OF PULMONARY TUBERCLE.

IT will facilitate the understanding of what is now to be advanced, if I recapitulate some of the propositions, which were printed in my Enquiry, and which were faithfully drawn from a comparison of the progress of disease in man and in the lower animals. It is only by such a method of investigation that truth, in a matter of this kind, can be discovered. Indeed it cannot be doubted that the knowledge of structural diseases would have been much more complete, than it is at this day, if this mode of enquiry had been more extensively followed. Let it not be supposed that information derived from the sources alluded to is inapplicable to man. In all the essential points of organization, the identity is so great as to leave

no reasonable ground for questioning the accuracy of inferences that may be drawn from diseases, which may take place in the muscle, the membrane, the lung, or the bone of a cow or a horse, and disorganizations of a similar character, which may occur in similar textures in the human body.

So much am I convinced of this, that I cannot admit that our Hospitals and Dissecting-rooms are alone the best places for acquiring a knowledge of the progress of morbid changes. And true it is, that those who have obtained all their information of diseases in such situations, are sometimes very unwilling to believe that vast disorganizations, which they seldom witness till they approach toward their conclusion, can have arisen from the small beginnings from which we know they spring. Individuals who employ no means to correct the deficiency of information which must necessarily arise from viewing a mutable object in one stage of its being, are compelled to adopt the only method, that is left to them for denoting diversity of appearances, which is to assign a different name and a different origin to each. Such, I am persuaded, has been the cause of many of the mis-

apprehensions of professional gentlemen, both on the Continent and in this country.

As the whole of the doctrines which we are now about to discuss must, in a very considerable degree, rest on such evidence as has been referred to in the preceding paragraphs, it seemed well to claim for it a greater share of consideration than it has yet obtained. Unless I should be so fortunate as to gain this object, I feel it impossible to expect a due comprehension of the proofs which may be adduced, or that an enlightened and scientific view can be entertained of a very interesting and extensive pathological question.

I am induced to dwell with earnestness upon this subject, not merely as it regards any thing in which I am personally interested, but because it affords the greatest opportunities for gaining an acquaintance with almost every disorganization. If the reader of these Illustrations be not prepared to grant me what I request, I fear it will be in vain to hope to convince him. He refuses the best evidence that the subject admits of, and I see no other means of removing any misconceptions which he may entertain.

The following propositions* are selected from among others, which I have published in my Enquiry. They will, I trust, be confirmed by what is to be now brought forward, and at the same time aid the reader in comprehending details, which would otherwise prove obscure and perplexing.

First, then, I affirm, "That tubercles exist in almost every texture of the body, and that their origin and essential character will probably be found to be the same, wherever they are discovered."†

II. That tubercles in their commencement, are small vesicular bodies (i. e. hydatids) with fluid contents.‡

III. That these bodies subsequently undergo transformations, on the nature of which their tuberculous character depends; that these transformations are progressive, but not uniform, and that it is only in the larger bodies of this kind that

* It may be proper to state, that all of these propositions are not *literally* transcribed from the Enquiry. They are collected from the places to which reference has been given, and condensed into their present form, for the sake of distinctness and precision.

† *Vide* Enquiry, p. 75.

‡ *Id.* p. 214.

they can be accurately traced. That they commence with an opaque spot, which advances with different degrees of rapidity, and ultimately converts both the contained and containing parts into substances very different from what they were at first.*

IV. That on the size and relative position and structure of the tubercles, which are thus formed, depend the characters of many of the most formidable disorganizations, to which the human body is exposed.†

V. That considering the transmutations, which these bodies undergo, the condition in which they may be found will be modified by the time at which they may happen to be examined.‡

VI. That it is rarely that we can have an opportunity of seeing the first steps of these morbid phenomena in the human subject, because the tubercles are generally formed, and the elementary character of course lost, before death permits us to make enquiries respecting altered or morbid structure.§

* *Vide* Enquiry, p. 93. *et cet. loc.*

† *Id.* p. 215.

‡ *Id.* p. 217.

§ *Id.* p. 241.

VII. That some tumours are formed by the aggregation of tubercles, and that the characters of such bodies are materially influenced by the relative position and contents of the elementary parts, of which they may happen to have been composed, or in other words, that “varieties in the arrangement of the elementary parts of morbid growths, will of course cause corresponding varieties in their appearance.”*

VIII. That, therefore, diversity of appearance in tubercles or tumours does not imply diversity of origin, for it has been demonstrated that substances and textures of very different properties may be found even within the same cyst, thereby merely denoting different gradations in the changes, to which these bodies are liable.†

IX. That the disorganizations above referred to are not the product of any species of inflammation, and that though inflammation may attend their growth, and modify the symptoms, which they occasion, yet that it is very different both in its origin and consequences from that species which at-

* *Vide* Enquiry, p. 218. *et cet loc.*

† *Id.* pp. 221, and 231.

tacks a part unaltered by previous disease; that in the first instance it is to be considered as the consequence, and in the latter as the cause of altered texture.*

Whatever sentiments may be entertained respecting the above propositions, they will at least, I trust, be found to be so free from ambiguity as to render my meaning not liable to be misunderstood. I do not expect to be able to illustrate all of them fully at present, though I have no doubt of being able to present evidence enough to sustain their general accuracy. I have placed them in their present situation that the reader may know at once what he is to expect; that he may have a view of the field over which he is to travel, and that he may with greater facility be able to detect me, if, by any chance, I should attempt to lead him from the right path. I do not seek his assent, till we have passed through the different stages of our journey together, and till he has ascertained for himself that the ground has been accurately explored. I have only further to beg, that he will come to the investigation with an unprejudiced

* *Vide Enquiry*, p. 120.

mind, and not hastily reject any thing that may appear at variance with his preconceived opinions, till he has candidly weighed the evidence by which it is supported. Let us now proceed to examine the progress of tubercles in the lungs.

It has been usual to class these bodies under various heads and to describe them in different stages. Some benefit may have arisen from this method of treating the subject; but there can be no doubt that it has also been the cause of much confusion; writers by no means agreeing in the characters or number of the different species; and there is as great a variation in their sentiments as to their stages and their progress.

Should I be successful in establishing the propositions above enumerated, these ambiguities or contrarieties will at once disappear; and a series of phenomena, consistent with each other and with the structure and functions of the parts, where the tubercles are generated, will be presented in their stead.

It may on many occasions be convenient to talk of the stages in the formation of tubercles, and also to designate those of different appearances by dif-

ferent names ; but it is my intention to abstain as much as possible from doing either, till the real progress of these disorganizations is clearly ascertained. This being accomplished, we shall be more likely to avoid the evils "*quæ ex fœdere verborum et nominum se insinuarant in intellectum.*"*

On entering upon the description of tubercles in the lungs, it will be necessary to make one other remark in addition to what has been already said. Should a small number be generated, the symptoms and morbid appearances may be very different from what they are, when a large quantity are evolved. In the former case, they, for the most part, attain a much larger size than they do in the latter; and then they may produce either a vomica, or a tumour, or both. In the other case, which is by far the most common, the size which a tubercle may attain, is necessarily limited by the number and position of those, by which it may happen to be surrounded. If they advance simultaneously, no one can much outstrip the other in its growth; and the consequence is that we gene-

* *Novum Organum*, Sect. LIX.

rally find under such circumstances a number of tubercles not differing very much in magnitude from each other, either approximating or in actual contact, and with qualities varying according to the nature and period of their progress. It is that progress in the common tubercular phthisis, that we are now to attempt to trace.

When tubercles are first formed in the lungs, they are not cognizable by the touch, by reason of the delicacy and elasticity of their structure, but they are visible on careful inspection. They are very small vesicular transparent bodies, and shine amid the unchanged texture of the surrounding lung. Should any of them happen to have been generated on the surface of the membranes, they there may be seen clustering together, and resemble both in size and general character the beautiful globular incrustations, which beset the stalks and leaves of the ice plant. In the human subject it is *very rarely* that we can have an opportunity of detecting them in this their primary state; and consequently most of the descriptions which have been given, commence at a period somewhat later in their progress. At that time the softness and

delicacy of the vesicle is lost, its transparency is diminished, and its size is increased. On examining the lung, where they may exist, by the touch, a distinct granular sensation is communicated to the fingers. The progress from this period is evinced by an augmented size, a firmer texture, and a complete loss of transparency, a yellow opaque body being perceptible. In this state they sometimes fall into ulceration and prove fatal. But before such an event takes place, it occasionally happens that many of them advance further and exhibit other appearances. Except where they are in contact with each other, they go on increasing in bulk. The coats of some become thick and hard and almost cartilaginous, while their contents may vary both in colour and consistence. Others proceed in a different way and are condensed into solid bodies of an uniform texture, the cysts and the containing parts being scarcely discernible from each other.

The appearance then of the lungs of those who die in this state is, as follows: some tubercles, when cut through, will be found to be firm and solid, others with thick dense coats containing

curdy, cheesy, or purulent-looking substances; others will be found to have been in part destroyed by the progress of the ulceration, and to shew the firm and almost cartilaginous remnant of the emptied cyst, conspicuous amid the surrounding disease. Should a great number of contiguous tubercles have fallen into this state, deep and extensive and irregular shaped fissures and excavations are thereby formed.

In the progress of the tuberculous disease, there are corresponding changes in the surrounding lung, which it is necessary now to note. At the first developement of tubercles, whether in the lungs or elsewhere, the surrounding texture seems to undergo little or no alteration. The lung retains its fresh pink colour, and its light elastic feel, and there appears to have been no interruption either to the circulation of the blood or air.

As the tubercles increase in size and in density, and approximate each other, they cause greater disturbance in the system. The blood is impeded in its circulation, and respiration is of course rendered quick and laborious on slight exertions. The consequences are obvious, the lung becomes

firmer and of a darker colour, and ultimately exhibits that appearance, which has been supposed to be indicative of a particular species of disease.* Whether this be an idiopathic affection or not, it is not at present material to enquire, as in such examples as we have described it is manifestly occasioned by the growth of foreign bodies in the lungs; and the darkness of its colour, and its induration increase in proportion to the obstruction which is offered to its functions.

The dark and indurated lung, which occasionally surrounds the tubercles, may, under the circumstances which I am about to describe, be obliterated. This takes place when the tubercles increase in size and coalesce, a dense solid structure being thus formed, with here and there partial traces of the original tuberculous character, to the total exclusion of every thing like the pulmonic texture. This progress may be in part seen by referring to the 3d and 4th Plates.

In the last mentioned one, an appearance will also be found, which has been designated "*melanosis*" by the French writers, and treated of like

* Hepatization.

hepatization, as a distinct variety of pulmonary disease. I am not prepared to speak decidedly upon this point. It certainly does exist occasionally in conjunction with tubercles, and tubercles much more frequently without it; I suspect therefore that it ought rather to be considered as a casual deviation from ordinary occurrences, than as a distinct species of disease.

The changes of structure above described, are indicated by corresponding symptoms. Tubercles, in their incipient state, may exist without producing much disturbance in the system, and they may pass onwards towards consolidation, if they be not very numerous, without affording almost any signs of their existence; and in this consolidated state they may continue, and not in any material degree tend to abridge life. The unexpected occurrence of solid tubercles or tumours in the lungs of those, who had not previously manifested any symptoms of such disease, bears me out in this assertion. When tubercles are fully consolidated, there is the strongest reason to believe that they do not subsequently fall into a state of suppura-

tion. This occurs chiefly in those that were not destined to arrive at this point.

The consolidation therefore just referred to, may in some measure be considered as a favorable termination to tubercle, as life has been found to be compatible with their existence, except in cases where they occupied a large proportion of the lung, or produced accretion of the membranes. It is in that period of their progress, which is intermediate between the state last mentioned, and their first developement, that all the symptoms, characteristic of tuberculous phthisis occur. This will be apparent by attending briefly to the ordinary progress of the disease.

In a person who has tubercles, a frequent cough without any expectoration, but with occasional oppression about the chest, and hurried respiration on slight exertion, may exist at intervals for many months, or even a longer period without any other sign of disease. What is commonly called a fresh cold, may increase these symptoms and render them more permanent, and then the patient, who never expectorated before, may perhaps be surprised by spitting up a yellowish or

whitish globular shaped mass, tinged with blood, or a gush of blood may precede an occurrence of this kind. I have known the last mentioned symptom repeatedly happen, to a most alarming extent, in a case, where there was great destruction of the pulmonary tissue by the consolidation of tubercles, but where, though the case proved fatal, there was never any expectoration of the matter from tubercles. It is from this and other kindred cases, that I infer that tubercles once consolidated do not subsequently suppurate or ulcerate.

Such an expectoration, as I have above described, is a very sure token of tuberculous disease. One of them has given forth its contents, and it plainly tells us that there may be more in a condition likely to do the same thing. In proportion to the number of these bodies, and the rapidity with which their texture is broken down, is the progress of the disease.

It may happen that considerable intervals of time may exist between the ulceration of each tubercle ; and even after several successive events of this kind have taken place, the patient may recover. The inference to be drawn from such cases, is, either,

that there were not a great number of tubercles in a state to undergo the ulcerative process, or that, in consequence of favourable circumstances or judicious treatment, the tubercles may have been brought into a quiescent state, and subsequently proceeded onwards to consolidation.

It would be well for the many individuals, who are afflicted with this disease, if we could more frequently bring about such terminations; but in by far the majority of cases, the destruction of the tubercles, when once begun, is followed by successive events of that kind, and by the usual train of symptoms, which accompany the disease to its fatal termination.

The appearance and the quantity of the matter expectorated differ much at different periods in the same case; that which is discharged from a tubercle, strictly so called, varying from that which may be excreted from the disease of the mucous surfaces, which has been excited by the tuberculous affection. The appearance of pus, by no means, as is generally supposed, necessarily indicates the presence of tubercles, for the contents of these bodies are very often far from being purulent. It is

manifest therefore that this test, which has been looked for to determine the existence of tuberculous disease, may be fallacious. The reader must remember that I am here speaking of the matter, which is contained in the tubercle itself, in contra-distinction to that, which is yielded by the surfaces of tubercles that have ulcerated, and discharged their contents, as well as of that which is afforded by the diseased condition of the surrounding parts.

I have already said that the dark-coloured indurated state of the lung is probably more frequently a symptomatic than an idiopathic affection. It does not attend tubercles in their early state, though it more or less accompanies them as they advance. When it exists to any great extent around tubercles, which are undergoing a process of dissolution, the dark-coloured and diseased lung itself seems also to fall readily into decay, and the appearance of the tubercles in such a medium, gives a character to the disease, which by some has been deemed essential and specific, whereas it is only casual and adventitious. When it occurs to any considerable degree, the difficulty of breathing is

generally greater than when it does not exist, and there is, for the most part, likewise a livid appearance about the lips and countenance, which is not seen in other cases.

Considerable variety with regard to the nature and degree of pain is perceived in the progress of the tuberculous disease. It sometimes runs its course, little or no pain ever having been experienced; at other times the disruption of every successive tubercle seems to be accompanied by deep-seated and acute darting pains. It is impossible to prove that these pains are occasioned by the dissolution of the tubercles. I believe that it is so, because I have not observed them, but where tubercles were in this state. It is nevertheless true, that tubercles pass on to consolidation, without having ever been attended by pain.

When tubercles attack the membranes of the pleura, without pervading the lungs to any considerable extent, I have shown in my Enquiry, that a variety of disease is generated, which leads to a fatal issue, but without all the symptoms characteristic of tubercles in the lungs. In both instances the disease is the same, though the part af-

fects be different. The combination of the two produces some peculiarity in the symptoms, which it may be proper to allude to.

Tubercles in the pleura cause sometimes effusion into the cavity, more frequently accretion. When the latter event takes place, there is cough and dyspnœa and a rapid pulse, but no expectoration. But when tubercles in the lungs, in a state of ulceration are added to it, we have in conjunction with the symptoms already enumerated, the expectoration of tuberculous matter, hectic fever, &c. The mode of breathing in cases, where accretion of the pleuræ has taken place is different from what it is when the lungs are free within the cavity. In the first mentioned instance, "the shoulders are drawn forwards, the ribs do not move as in the natural state, the whole chest heaves at once; and most of the muscles on the trunk of the body seem to be called into action."* On striking the chest of a person in this state, the sound emitted is like that produced by the percussion of a solid body, very different from that which a healthy

* Enquiry, p. 170.

chest affords,* or when disease exists in the lungs without accretion of the membranes.

There are other appearances to be met with in the lungs of those who die of pulmonary consumption. I have abstained from saying more on these points, than was absolutely necessary, my object having been to bring forward a true and faithful delineation of the progress of tubercles themselves, disencumbered, as much as possible, of every thing that could obscure the representation.

When an individual affected with tubercles happens to be cut off by another disease, before the tuberculous affection had run its usual course, we may sometimes be presented, in the same lung, with examples of all the progressive changes which I have described. Such examples of course cannot often occur in the human subject. It has happened to me to meet with several of this de-

* This mode of investigation is at least as old as Hippocrates. I have constantly employed it, and in most cases it may be relied on, as an auxiliary in forming our diagnosis. The principle has been extended recently by several French writers, and an instrument has been invented to render the observations more accurate. I have never used this instrument, and am therefore unable to speak of its merits.

scription, and I submit the following one to the reader's attentive consideration.

A boy about 13 years of age came under my care with symptoms of pulmonary disease. Though they were of a threatening nature, they were by no means such as to excite any immediate alarm for his safety. His countenance was pale and emaciated, his breathing rapid, and the pulse very easily accelerated on slight motion. He coughed frequently but expectorated very little. While in this condition he was suddenly seized with the symptoms of an affection of the head. He moved about apparently unconscious of surrounding objects. He soon afterwards became comatose, and although very active means were used to save him, he died in a few days. He was however sensible and talked, and knew people about him just before he expired on the 10th day of December, 1819. I examined the body on the following day. My principal attention was directed to the state of the thorax, and there I found most interesting illustrations of the descriptions given above. There were accretions nearly of the whole of the right side of the chest ; but they were not so firm by any

means as they are in the more advanced stages of tuberculous disease. On examining the pleura, particularly towards its upper portion, it was studded with innumerable small bodies, many of them not so large as the head of a pin. They were perfectly transparent and glistened on the surface of the membrane. On another portion of the pleura pulmonalis I found a tubercle pendulous and as large as a pea, with thickened coats, and containing cheesy matter. This body is represented in Plate III.

The transparent vesicles pervaded the substance of the lungs as well as the membranes, but they did not all remain in this simple or elementary form. They exhibited every gradation in the progress, which has been already described. In their first state, neither lungs nor membrane, where they occurred, were much altered. But the condition of the surrounding lung became changed with that of the tubercles themselves. Some had lost their transparency, and were of the size of millet seed; others were considerably larger, and were of a firm uniform consistence. Others were less uniform both in colour and texture;

some had discharged their contents, and the empty cysts appeared; others which were consolidated, had nearly coalesced and formed a dense yellowish structure, quite foreign to that of the original pulmonary tissue.

The surrounding lung, as has been already said, at one part was nearly healthy. Where the tubercles were most numerous, it was condensed and of a purple colour, and lastly where the tubercles had come in contact, the pulmonary texture was not to be found. Plates II. and III. give accurate representations of these appearances, and I trust will aid in rendering this description clear and intelligible. On opening the head, water was found in the ventricles, the veins of the pia mater were much enlarged, and there was some appearance of thickening of the dura mater over the right hemisphere.

In the foregoing case, we have an example of tuberculous disease of the lungs interrupted in its progress by the supervention of a fatal affection of the head. To this unexpected occurrence are we indebted for being enabled to contemplate the first-mentioned disorder in a state, which was pe-

cularly fitted to elucidate and confirm the history of the progress of tubercle, which has been delivered.

It is impossible to describe minutely all the varieties of appearances which may arise from peculiarities in the structure and arrangement of tubercles in the diseased mass. There is one however, which is so very common, that it requires to be noticed. It occurs when tubercles, originally soft and circular, grow in size and mutually press upon each other's boundaries. The globular character is thereby destroyed, and the divisions between each, so far as they can be traced, are angular, so that instead of circles we have squares or figures of different kinds. This appearance is clearly represented in a portion of Plate V. and the same thing will be more fully detailed in a case which I shall hereafter describe.

The extent to which tuberculous diseases may proceed is much greater than, *a priori*, could be expected. The 4th Plate is taken from an example, which satisfactorily illustrates what I mean. The lady never had exhibited any of the characteristic signs of pulmonary disease: the symptoms

were rather those of an affection of the stomach and of stricture in the œsophagus, and on the latter account it was often necessary to pass a bougie. Some weeks before she died, she was seized with symptoms of pneumonia. Very frequent and large bleedings produced little effect. She expectorated pretty freely, but the dyspnœa became exceedingly distressing, and she died in spite of every effort. On examining the lungs, I found strong, but unlooked for proofs of long previously existing disease. Almost the whole of the posterior portion of each lung was transmuted into a dense and nearly cartilaginous substance. Where the density was greatest, the character of tubercles was obliterated, but it gradually re-appeared as we proceeded from this point, till at last their figure and boundary became perfectly distinct. All these appearances may be accurately seen on the Plate above referred to. Some of the distinct tubercles were hard, others were not completely consolidated, and others had given way during the inflammatory attack, and discharged their contents, leaving their firm remnants in the shape of excavations, which were visible in different parts of the disease. In

another portion, bodies of a dark purple or almost black colour were seen. There was no stricture in the œsophagus. I am convinced, therefore, that the difficulty of deglutition was occasioned altogether by the pressure of the tuberculated and consolidated mass above described, at that point where it was contiguous to the œsophagus.

The only other sign of morbid structure was found in the right kidney. It was much increased in size, and shewed an undulating surface. When divided longitudinally its original organic properties were entirely changed, and it presented to our view a cluster of cysts, filled with a white unctuous feeling substance, resembling very much softened pipe-clay.

The foregoing case not only illustrates the progress of tubercles, but it substantiates likewise the position which I wish to impress on the reader's mind, viz. that, that progress may be very far advanced, without affording any signs of their existence. Of course, if a very large portion of the lung be obliterated by solid tubercles, death must sooner or later be the consequence; but it will not arrive after the manner of pulmonary consump-

tion. There is neither expectoration, nor hectic fever, nor any of the other signs which attend the ulceration of tubercles.

Except in what relates to the first developement of tubercles, I am abundantly supported in all that I have said by every accurate pathological writer. It is true that the inferences which I have presumed to draw from these appearances, do not accord with those which are most generally adopted. This, I trust, will not of itself be a sufficient reason for refusing assent to them. The candid enquirer will duly examine the facts, and not permit the authority of names to carry him away, till he has estimated the value of the testimony by which my sentiments are supported. If this be done, it is all I can ask or desire, for truth cannot fail to be the result of the investigation.

I doubt not that it will be said by those, who examine pulmonary tubercles merely after they have finished their course, that such appearances, as I have described in the first period of their progress, are not to be found, and therefore they may infer that they never existed. This inference would be

most inaccurate. It would be as much in opposition to fact as it is contradicted by all analogy.

Why should a principle, which is fully admitted in all the other operations of the animal or vegetable world, be pertinaciously resisted, when it is applied to the diseases of man? But most of the pathological doctrines of the day would lead us to believe, that many disorganizations are actually formed or deposited in the condition in which we see them after death; no account being taken of their primary condition and of their subsequent progress. Writers who have not fallen into this error, have adopted another, possibly, not much less unfavourable to the progress of science. They invert the order of nature, and place that first which ought to be last, and that last, which ought to be first. Thus an attempt has been made to account for the formation of cysts, by an operation almost impossible; these bodies being supposed to be thrown round the previously effused fluid by some peculiar action of the vessels of the part where they are generated.

Doubtless such misconceptions have arisen, on the one hand, from the adoption of hasty and ill-

grounded hypotheses, and on the other, from the remoteness of the last appearance of disorganizations, from their simple and elementary state. Another prevailing error has stood in the way of the knowledge of those diseases, of which we are now treating. It is very generally believed, that the part where the morbid change takes place, altogether modifies and determines its nature. Thus we are told that there is one tubercle for the lung, another for the liver, and another for the membranes, &c. This opinion, though it has some colour of truth, is exceedingly fallacious. The symptoms and course of the tuberculous disease are certainly "materially influenced" by the part, where it occurs, but the origin of the tubercle itself is regulated by general laws, connected with the essential and fundamental properties of animated beings.

I have deemed it of great consequence to attempt to obtain just views of these points. To have exposed an error is one step towards the attainment of truth, and if the evidence which I have collected should not be sufficient to satisfy every one that the opinions, which I advocate, are cor-

rect, it may at least convince him that they are not rashly or incautiously adopted.

In further confirmation of what has just been advanced, I present the two following cases. The first shews the progress of tubercles in the liver, the next in the mesentery and serous membranes. An accident, which put a period to the life of the first patient, enables me to give an account of his disease more illustrative of the subject in hand, than if he had died in the ordinary course of his malady.

On opening the abdomen, a considerable quantity of serum was found within it. The omentum was thickened and was full of tubercles. On examining the liver, its surface was found to be very uneven, in consequence of the existence of tubercles, which pervaded its whole substance. Different sections of this substance afforded different appearances. Some of the tubercles were exceedingly minute and could not be distinctly seen without the aid of a glass. Others varied in size from that point up to the magnitude of a walnut. Some were solid and of a bright yellow colour; some contained a brownish looking matter; others a thick pulpy mass like broken down brain; and

others retained, in a very evident manner, the decided remains of the hydatical character. Another portion of the disease exemplified what I have already described, as sometimes occurring in the lungs, in consequence of the peculiar arrangement of the tubercles. They had united together and formed a substance approaching in density to that of scirrhus. Their boundaries in some places could not be distinctly traced, but in others, instead of presenting the original circular form of the tubercle, they were marked by angles and straight lines. Plate V. gives a very faithful representation of these appearances. One large hydatid is seen slit open. The coats were thickened, and of a whitish shining appearance, the contents of a brownish colour and thin consistence. Adjoining it, were seen tubercles with various substances in them; next the gradual obliteration of the tuberculous character; and, lastly, the partial remains of that character as shewn by rectilinear divisions. On cutting through another portion of this morbid structure, the tubercles, which had occasioned the last mentioned appearance, were seen in a less advanced state. They were in vast numbers, distinct and not con-

solidated. They contained a bright yellow purulent-looking fluid, which exuded from the cut surface, exactly as matter exudes from similar tubercles in the lungs.

Wm. M——, eight years of age, was attacked with measles about eight months ago; soon afterwards he began to complain of distress and uneasiness about the abdomen, which was observed to become tumid and enlarged. It increased in size, and when I saw him, it had attained a very considerable magnitude. It was tense, but not very hard; around the umbilicus the skin was of a brown reddish colour; under that part too the tumour felt softer, and communicated sensations to the finger, very similar to those described as occurring in the case of Henry Higgins, which I have published in my Enquiry. I expected, as in his case, that the integuments at the navel would have given way, and discharged the fluid which I thought I perceived underneath. I had no doubt, from the whole character of the disease, that tubercles and accretions had taken place, save in the part alluded to. Although therefore no external opening was formed, as in the case of Higgins, yet

it was ascertained, as in the last mentioned case, that a communication must have been established between the abdominal cavity and some portion of the alimentary canal; as large quantities of a yellow purulent-looking matter were often discharged *per anum*; and these discharges were followed by a manifest diminution of the abdominal tumour. These discharges doubtless relieved the sufferer from much distress, for he did not experience many of the most afflicting symptoms which attend similar disorganizations, when no such sources of relief occur.

He lingered till his body was emaciated to the last degree, and for several days before death took place, the front of the chest and abdomen were covered with petechiæ. I examined the diseased parts this day, and the dissection was carefully performed in the presence of Mr. John Fosbroke. On opening the abdomen, the peritonæum which lines the cavity was found universally thickened, and studded with yellow tubercles of various magnitudes. Under the umbilicus, and extending downwards to the hypogastric region, a cavity was formed by the accretion of the intestinal and ab-

dominal peritonæum to each other. Both surfaces had been highly tuberculated, many of the tubercles had passed into a state of ulceration, and the matter which was formed in this diseased cavity had ultimately destroyed the texture of a portion of the intestine, and found its way into the canal, and was voided in the manner already described. On proceeding to examine the rest of the abdomen, tubercles were found in different stages, and of different magnitudes, and in different combinations with each other, attached to the viscera or embedded in them.

I shall select a portion of the mesocolon to illustrate what I mean by these statements. There I found many tubercles in a semi-transparent state, having little advanced beyond their original vesicular or hydatical character. They seemed to be connected with the lymphatics, many of which were much enlarged. In contact with the tubercles last described were found some, completely transformed into solid bodies, while others again had contents of a curdy or cheesy consistence, which could be easily squeezed out from the cyst. In those where the consolidation was most per-

fect, the appearance of the cyst became less conspicuous, and the amalgamation both of the contained and containing parts was complete. Another variety of appearance was occasioned by the manner in which the tubercles happened to grow together. Contiguous to those already described, I observed an irregular oblong yellow mass. This was one of those appearances, which it has been customary to account for, by ascribing its origin to what has been denominated the deposition of scrofulous or tuberculous matter. I have often seen large sheets of this substance, spread over the intestines or lining the abdominal cavity. And I have no hesitation in saying, that the account in question is decidedly erroneous. I had opportunities this day of completely ascertaining this point, and I feel myself fully authorised in repeating my former assertion, that such appearances are produced, not by any process that we can understand by the word "deposition," but by the aggregation and coalition of tubercles, which happening to be generated near to each other, and having increased in size, ultimately unite, and seem, on a superficial exami-

nation, to have lost all their original and distinctive qualities.

I formerly applied the inferences to be deduced from facts of this kind to explain the growth and appearances of tumours; and in so doing I stated what is in perfect conformity with the phenomena.

The irregular shaped mass then, which we found in the mesocolon, owed its appearances to the peculiar arrangement of tubercles. As they increase in size, and begin to press on each other, their original well-defined circular form is lost; the boundaries too, which at first separated each from the other, may in time disappear altogether, and when we examine disorganizations of this class, all signs of an original tuberculous origin may be obliterated; and we may find scirrhus or cartilaginous substances intersected by septa, or divisions of various kinds, which may be considered as the only remaining traces of the elementary character of the disease.

It has been supposed that such statements were speculative, but from the preparations this moment lying before me, the most complete demonstrations of the facts may be exhibited.

Luckily we are not confined to what was found in the peritonæum, or in the mesocolon; most satisfactory elucidations of the same phenomena being presented in corresponding disorganizations of the liver. There, some tubercles, perfectly distinct and circular, stand out from the surrounding texture in a prominent form. Some not larger than the head of a pin, others as large as a horse bean. Some with their cysts as described in those in the peritonæum; some entirely consolidated. Towards the middle of the mass which contains the tubercles just described, may be seen an irregular shaped tumour; one large portion of it near the centre, is of an oval shape, whilst around it, and separated from each other by rectilinear divisions, are seen bodies of cartilaginous density; while others, more remote from this compound disorganization, and which had not come actually into contact with each other, retain their original tuberculous character in the most perfect manner; and amid them was found the remnant of a delicate cyst, which there is the strongest reason to believe, was an hydatid, accidentally cut in separating the parts.

On the surface of the spleen were a great number of minute hydatids. A few were as large as millet seeds, and of a yellowish colour, but the majority were much smaller, and when seen through a glass, were of a bright sparkling appearance. The lymphatic vessels on the surface of this organ were so much enlarged as to be visible to the naked eye intersecting each other, and it was chiefly at the points of intersection that these vesicular transparent and globular bodies were seen.

The disease (as is rather unusual in cases of this kind) had only in a slight degree attacked the thoracic cavity. A few tubercles, about the size of millet seed, were found in the right lobes of the lungs, and from the middle lobe of the left side, at the point where it extends over the pericardium, there was a solid tubercle of an oblong shape, about the size of a grain of wheat. To other portions of the same membrane were attached tubercles of the same description, some of which had attained the magnitude of hazel nuts. There was no effusion into the cavity of the thorax, nor any sign of inflammatory action near the tubercles, nor in any other part.

The foregoing cases and observations are adduced merely to exemplify the progress of tubercles. That progress, and other accidents dependent upon their existence, will, it is hoped, be made still more manifest by the facts which are about to be detailed.

CHAPTER II.

TUBERCULOUS DISEASES IN THE INFERIOR ANIMALS.

I SHALL now to proceed to elucidate the progress of tubercles as described in the preceding pages, by referring to analogous diseases in the Inferior Animals. In doing so, I shall have occasion incidentally to allude to the opinion respecting the origin of tubercles, which I maintained in my Enquiry. Hitherto I have kept that opinion out of sight, because I was particularly desirous that any degree of doubt which may still attach to it, should not be permitted to obscure what may be demonstrated to occur in the *progress of tubercles*. I wished, moreover, to avoid further discussion on that opinion, till enabled to record the results of experiments, which I have instituted, and by which, possibly, it may either be refuted or confirmed.

The cases to which I now beg the reader's attention, are decidedly of a tuberculous nature. I have selected the principal part of the descriptions from the work of M. Dupuy, an eminent French veterinary surgeon. His statements of morbid appearances correspond entirely with those which were delivered in my Enquiry, on the authority of Dr. Jenner, and which he enabled me to verify by my own observation. When the Enquiry was published, I was not aware that M. Dupuy's work existed. His testimony is the more valuable, because it is in a manner extorted from him by irresistible evidence, which compels him, in more places than one, to adopt sentiments in direct opposition to the main tenor of his work.

The *glanders*, as it is vulgarly called, is, strictly speaking, a tuberculous disease, attacking the lungs of the horse, and it bears the closest analogy to the pulmonary consumption in the human subject. *Farcy* is likewise a disease of the same genus, affecting another part of the animal. The connexion between these two diseases will be very satisfactorily established in the sequel, and afford us many

interesting facts highly applicable to the pathology of man.

As my purpose is to give accurate details of the appearances of the tuberculous diseases in the inferior animals, I shall extract from among Monsieur Dupuy's cases, such as are most illustrative. They will not only confirm what has been already advanced, but enable me likewise to point out the errors and misconceptions respecting the development of tubercles, which have been maintained both by that gentleman and other writers.

I beg, however, that it may be remembered, that, while I admit the accuracy of the facts, which I may quote from M. Dupuy, I by no means sanction the opinions by which they are accompanied. The reader must bear this in mind, otherwise he may imbibe prejudices which it may be difficult to remove. These will be more fully pointed out when we come to discuss the theory of the author, as well as that which is entertained by M. Laennec. In the mean time, pledging myself for a faithful representation of facts, I will either take the liberty of divesting them of their suppositious adjuncts, or of pointing them out to the reader, if this can

not be done without injuring the fidelity of the statements.

It is necessary to observe, likewise, that in glanders the morbid appearances are by no means confined to the lungs. The nasal cavities and the lymphatic glands, in many parts of the body, are generally also diseased. Tubercles and ulcerations are very often found in the former, and the same bodies, in different conditions, may generally likewise be discovered in other textures.

“An English mare, affected with glanders, was put to death on the 20th November, 1815.”

“The pituitary membrane on the division of the left nostril, presented some miliary tubercles which were not disorganized. The mucous membrane, which lines the maxillary, frontal and sphenoidal sinus of the left side, had become fibrous, thick, and like to the tissue of schirrus. The free surface was covered with white, hard, and fibrous “*vegetations*.”* Different degrees were observed in that transformation, from a consistence resembling the jelly of meat, to that of a schirrous or

* Some objection to the use of such words as this will be found in another place.

carcinomatous texture. The frontal sphenoidal and great sub-maxillary bones were thickened, spongy, and pervaded by a reddish fluid, and were easily cut. The internal table of the bones, which corresponds with the sinus, was covered with light porous spongy exostoses. The parenchyma of the lungs contained numerous pisiform tubercles, and white hard miliary tubercles were found in the liver;* the lymphatic sublingual ganglions also contained tubercles of the same kind."

"On examining the lungs of a mare, which was killed on the 29th of February, 1816, a great number of tubercles were found in the parenchyma of the lungs. These encysted productions were chiefly situated on the dorsal part. The internal surface of the cysts was reddish, such as one observes on the mucous membranes in consequence of catarrhs. The matter contained in these cysts was whitish, curdy, and perfectly similar to that which constitutes the tubercles of cows attacked with the *pommeliere*.†"

* De l'Affection Tuberculeuse, par M. Dupuy, p. 154.

† *Ib.* p. 141.

On reading this case, it may be supposed, that the cyst and its contents were different productions. This is an error, which is very commonly entertained, and has led to great misapprehensions as to the nature of tubercles. What is here called the cyst is a most essential part of the tubercle. It is the part which is the first formed, and without it no tubercle can probably exist. It will be proper to contrast the case just given with another, taken from the fourth division of the work, "on the tuberculous affection of domestic animals, compared with that of the horse."

"A cow, six years of age, was killed on the 2d of February, 1819. The body was examined immediately, and the following appearances were found. The pulmonary tissue was very much altered. It contained many cysts, enclosing hydatids of different magnitudes, from the size of a pea up to that of a goose's egg. Other cysts, of which the coats were of the consistence of cartilage, and even osseous, were filled with a substance analogous to that of bone; those which enclosed the hydatids were smooth and had the appearance of a mucous membrane. We thus found in these lungs hydatids

and tuberculous matter, which would seem to prove, that these bodies, though very different in their physical qualities and their organization, have many affinities in common, in regard to the causes which determine their formation, and the manner in which these bodies alter the pulmonary tissue.*

I do not give any more of the author's reflections on the last mentioned coincidence, because we shall have other opportunities to refer to that point. I cannot forbear remarking, however, that the very terms, in which he has conveyed his account of the morbid appearances, involve a contradiction to the truth, which he here, and in other places, almost admits, respecting the transition of hydatid into tubercle. The first part of the statement would lead us to suspect that the cysts and hydatids had no connection with each other, and that the tuberculous and hydatrical formations were regulated by perfectly different laws.

Let us examine the next case of a cow affected with the tuberculous phthisis. "The most remarkable lesions, which were observed, were a mass *platreuse*, hard, without odour, weighing a ki-

* De l'Affection Tuberculeuse, par M. Dupuy, p. 269.

logramme, situated between the layers of the posterior mediastinum."

" In the pulmonary tissue, there were also deposits of this *matiere platreuse*, of a yellow colour. This tuberculous matter formed rounded tumours of different sizes, with unequal surfaces, irregular, and rough. Some were uniform, others were of the size of an apple, from which doubtless arises the name of *pommeliere*, which the vulgar give to this malady. These tumours were throughout covered by the pleura, which was not sensibly altered. The lymphatic glands, situated at the division of the bronchia, contained also in their tissue tuberculous matter; their primitive organization had disappeared, and these ganglions had undergone a tuberculous disorganization. Another object, which merits the attention of observers, is the presence of a great number of hydatids of different sizes in the parenchyma of the lungs. These hydatids, regarded and described by zoologists as organized and living bodies, might throw some light on the origin and formation of tubercles, or at least prove, that these bodies, which disorganize the lungs in the same manner, *develope themselves*

under similar circumstances. This connection may become of much value in the etiology of those diseases, which deeply change the organization, and which are so common and so fatal.*

This is another example of the struggle in the author's mind between truth and error. He cannot throw aside his preconceived notions respecting the formation of tubercles, even though it is made manifest, to his own senses, that these notions are incompatible with the facts which he details. He cannot renounce his allegiance to his tuberculous matter, and in endeavouring to maintain the imagined distinction between hydatical and tuberculous affections, he causes much contradiction in his own statements, and great confusion to his readers. The words employed to describe the last case illustrate what I mean. Some of them tell us that the tuberculous matter is, in the first instance, deposited, and that it afterwards forms the tumours of different sizes, which are found in the lungs; while the concluding portion of the quotation is in direct opposition to this statement. This is an example of a very faulty method of tradition,

* De l'Affection Tuberculeuse, par M. Dupuy, p. 271.

which has wrought much mischief in our art. An unfounded hypothesis is tacked to a statement of morbid appearances, and it very often happens that the former has more influence than the latter. Wherever our knowledge is not so complete, as to enable us to describe faithfully the actual succession in the phenomena of nature, we ought to be very cautious not to mingle our accounts of what we see with any suppositions that may have arisen in our own minds on the occasion. To do so, is to poison knowledge at the fountain head, and to render our observations useless if not pernicious.

In the passage which called forth these remarks, it is implied, that the tuberculous matter is first deposited, and subsequently that the tumours are formed. Now, all this is mere conjecture; it is contrary to all analogy; and, according to the author's own statement, was much more likely to be inaccurate than otherwise. He was not required to give any opinion respecting the morbid appearances which he witnessed; at all events, that opinion, whatever it was, ought to have been disjoined from the account itself. I am quite aware

how difficult it is to avoid falling into this error; but it must be avoided by all who are really anxious to divest our art of some of its imperfections, and to prevent it from accumulating more.

The following case is worthy of attention. A bull, without horns, died of the tuberculous disease, at the age of four years. The "parenchyma of the liver contained encysted tubercles; they were so numerous that the viscus was transformed into a matter like pulverized bone. These tuberculous masses weighed ten *kilogrammes*. The lymphatic ganglions of the mesentery, of which the largest weighed four *hectogrammes*, were almost all tuberculous. And we found in their tissue the yellow matter similar to that contained in the cysts of the liver. On the mucous membrane of the *intestinum cæcum*, we found an ulcerated surface of the size of a five-franc piece, and there also recognized yellow matter, which was embedded in little cysts. Ulceration extended through the mucous membrane of the colon, as well as the rectum, and the softened tuberculous matter exuded on squeezing these parts. The extremity of the right lobe of the lungs was transformed into nu-

merous cysts, which contained the tuberculous matter. They weighed about six *kilogrammes*. A very considerable mass was attached to the diaphragm, and situated in the posterior mediastinum, and finally we found these tubercles in the coronary scissure of the heart.”*

“On examining another bull, with all the symptoms of the tuberculous diseases, the pleura costalis was found covered with flattened tubercles, of the size of a five-franc piece, which were chiefly attached to the second, third, fourth, fifth, and sixth sternal articulations. The pleura pulmonalis and diaphragm presented also a great number of tubercles of different sizes; they were found also on the pericardium, and between the laminae of the superior mediastinum. The pulmonary tissue contained so great a number of vomicae and tubercles, that there did not remain more than the fourth part of that organ, which had not undergone the tuberculous transformation. Here and there were remarked tubercles on the mucous membranes of the trachea, the bronchiæ, and upon the left ventricle and auricle of the heart. Almost all

* De l’Affection Tuberculeuse, par M. Dupuy, p. 280.

the lymphatic ganglions in the mesentery were large, hard, and tuberculous. There were also miliary tubercles and ulcerations of the mucous membrane of the small intestine, which was much contracted. The liver contained hydatids of the size of a hazel-nut, the testicles were yellow, as if they had undergone a fatty (*graisseuse*) transformation, and in this respect they had the same appearance as if they had been twisted (*bistourner*.)”*

It would only fatigue the reader to present to him more examples of this kind. The book itself abounds with them, as well as with many judicious remarks. But the author had unfortunately imbibed sentiments, concerning the origin and nature of tubercles, which forced themselves into all his reasonings, and obscured his descriptions in the manner, that we have already seen. There is a passage in the Introduction to his work, which shews, in an instructive manner, his near approximation to truth. He had almost seized the thread which would have guided him through the labyrinth, had he not permitted his understanding

* “To twist twice a horse’s genitals, so as to render him unfit for generation.”

Ibid. p. 273.

to be blinded by the prevailing errors of a faulty pathology.

“ Nous avons cru devoir comparer entr' elles ces deux affections (tuberculeuse et hydatideuse,) premièrement parceque nous les avons trouvées réunies dans les mêmes sujets, et souvent dans le même viscère ; secondement, parceque dans le kyste qui renfermait des hydatides nous avons trouvé des commencemens de dépôt de matiere tuberculeuse ; ce qui donnerait à penser que l'une peut venir après l'autre.”

After reading such a statement as the foregoing, I was little prepared to meet with the following hardy assertions at the distance of a few pages. The author there says, “ The organised production which we have called tubercle is the effect of a cause, which appears to me unknown.” So far this may be true, but in a few lines we meet with statements that seem not quite so correct. “ The tubercle at its commencement,” we are told, “ presents itself as a small, firm, greyish, hard body. This tubercle is often enclosed in a little cyst, which appears to be formed by the tissue in which it is developed.” Now here are statements in di-

rect opposition to the author's own declaration. He had clearly admitted that tubercles might be connected with hydatids, that the one might follow the other; nay further, he declares it to be his object to describe the different changes, which these hydatids undergo in the tissue of the lungs, the liver, or the mesentery, and to point out the different lesions which are occasioned "par les tubercles ou les hydatides."*

With such a conviction in his mind, it is very remarkable that he should have authoritatively stated that tubercles, at their commencement, are hard firm bodies. I am particularly desirous that the reader should fully comprehend the necessity of having clear and just views of this point. Without them, all subsequent endeavours to reconcile the difficulties, which must surround him at every stage of his progress, must be fruitless. I would therefore entreat him to pay no regard to the *dicta* of any man, but in as far as they are consistent with facts.

No one ever saw a hard tubercle deposited; and if we regard the analogy of nature, whether in the

* *Vide* Avertissement, p. 9.

vegetable or the animal kingdom, we should not perhaps err much in saying, that such an event is impossible. On the other hand, we know for certain, that some tubercles, at least, have a soft delicate texture, anterior to that of hardness, that they do advance from this transparent vesicular state to one of opacity and consolidation. Seeing therefore that the whole weight both of direct and analogical evidence lies exclusively on one side of the question, shall we be permitted to reject it, and affirm, that that takes place, which never was seen, and never can be seen, by any man?

The reader is already apprized of the opinions entertained by M. Dupuy, respecting the development of tubercles. He will therefore receive the following piece of evidence, in favour of what I have advanced in the preceding chapter, with all the consideration that it justly merits.

“A cow, which was suffocated in a few hours by a violent “*meteorization*,” had hydatids and tubercles in the interior of the lungs. We remarked between the internal surface of the cyst and the hydatid, a small quantity of a yellow substance like bone which had been reduced to powder. We

have seen in other cows, affected with tuberculous disease, that the quantity of bony matter was greatest when the internal surface was unequal, and especially when the cyst contained decayed bluish and decomposed hydatids. *This circumstance would make one believe, that the disease is of ancient date when tubercles are formed. In that case, these different transformations are to be considered as the successive states of the same malady.* We have observed another alteration of the pulmonary tissue, which we should consider as the first step in these disorganizations. *The parenchyma of the lungs presents little globular ‘crepitantes,’ circumscribed masses more elevated than the surrounding parts; the cells of the lungs are swelled and distended in these little tumours by an elastic fluid.* We have seen these remarkable alterations on the surface of the lungs of the foetus of a sheep, which had hydatids and tubercles in the interior of this important viscus. The foetus of the cow, of which we have already spoken, presented also, on the surface of the lungs, these little globular agglomerated eminences.”*

* Une vache qui a été suffoquée en peu d’heures par une

I will not at this time trouble the reader with any remarks on the inaccuracy of various expressions in the preceding quotation. It contains, when reduced to its true meaning, statements sufficiently clear to enable me to appeal with additional confidence to the understanding of the reader. It proves what has all along been contended

météorisation très-forte, avait des hydatides et des tubercules dans l'intérieur des poumons ; nous avons remarqué entre la face interne du kyste et l'hydatide, une petite quantité d'une substance jaunâtre, semblable à des os qu'on aurait réduit en poudre. Nous avons vu sur d'autres vaches affectées de la pommelière, que cette quantité de la matière des os était plus grande lorsque la surface interne était inégale, surtout lorsque les kystes renfermaient des hydatides flétries, bleuâtres et décomposées. Cette circonstance ferait penser que la maladie est ancienne lorsque les tubercules sont formés. Ces transformations diverses seraient alors des états successifs de la même maladie. Nous avons observé une autre altération du tissu des poumons, que nous regarderions comme le premier degré du dérangement que nous décrivons. Le parenchyme pulmonaire offre de petites masses globuleuses, crépitantes, circonscrites, plus élevées que les parties voisines ; les cellules des poumons sont gonflées, distendues dans ces petites tumeurs par un fluide élastique. Nous avons observé ces altérations remarquables à la surface des poumons de fœtus de brebis qui avaient des hydatides et des tubercules dans l'intérieur de ce viscère important. Le fœtus de la vache dont nous avons parlé précédemment a présenté aussi sur ses poumons ces petites éminences globuleuses agglomérées.

De l'Affection Tuberculeuse, p. 402.

for, that tubercles are the result of an anterior process of disorganization; and that the first step in that process is the formation of small vesicular bodies with fluid contents, and that the different transmutations which they undergo, are but the successive states of the same disease.

In estimating the value of this testimony, it is of consequence to observe, that it is drawn from a gentleman who had the most extensive opportunities of carrying on his investigations; and, after having, in the early part of his work, delivered opinions utterly irreconcilable with those which are given above.

Among domestic animals, none are more frequently affected with tuberculous diseases than sheep. From these animals, many of the descriptions which were given in the Enquiry were taken. M. Dupuy has presented an interesting tabular view of the number of diseases of this kind, which occurred in a given time in the flock of the veterinary school at Alfort. I mean to select a few examples, to illustrate still more strongly the connexion between hydatid and tubercle. "In one case, there were hydatids in the peritonæum and the

pericardium, and miliary tubercles in the lungs. In another there were hydatids, and tubercles in a state of suppuration in the lungs; in another, which died of a gangrenous affection of the mammæ, the mammary glands contained miliary tubercles, and the same were found in the lungs; in another, hydatids were found in the brain, the lungs, the omentum, and the mesentery; ascarides in the larger intestines, and miliary tubercles in the lungs. In another, there were found miliary tubercles in the lymphatic ganglions of the mesentery, and hydatids in the liver, the omentum, and the lungs. In another, an hydatid was found in the left ventricle of the brain, and many others on the mesentery; in another, the liver and lungs contained hydatids and miliary tubercles. The bronchiæ were filled with ‘*strongles filaires (crinons;)*’ and in another, there were hydatids in the brain and in the lungs. The lungs contained besides miliary tubercles.”* The reader will find many other examples of the same kind in the work above noticed.

In the early part of this chapter, I alluded to the important information which might be derived

* De l’Affection Tuberculeuse, p. 396.

from the consideration of the nature of farcy, as well as of glanders. Farcy is unquestionably connected with that condition of the system which gives rise to the formation of tubercles, whether in the lungs or elsewhere, and, with great justice, it has been supposed to bear the same relation to glanders, which is the tuberculous phthisis of the horse, that scrofulous affections of the extremities bear to the same disease in the human subject.

Farcy seems clearly to be generated by disease of the lymphatics of the limb. The buds, as they are called, appear at first in the shape of small tubercles or buttons, which have the same organization, and go through the same changes that mark the progress of tubercles in the lungs. This disease seldom confines itself to the extremities. It generally advances towards the chest, attacks the membranes of the nose and lips, the glands under the jaw and in the neck, and finally develops itself in the shape of tubercles in the lungs, and then it constitutes the glanders. This, however, is not to be considered as the usual progress of the last-named disease, for it often begins in the lungs, and runs its course without any appearance of farcy whatever.

I do not mean at present to push the inferences, which may obviously be deduced from the last-mentioned disorder, to the extent that they might very safely carry us. I hope, however, at some future time, to draw, from sources of this kind, evidences which possibly may silence all doubts touching the origin of tuberculous diseases, wherever they may happen to be found. Rather, however, than weaken the impression of what has been advanced as to the progress of these bodies, I am willing to leave the question, as to their origin, to a period when it may be more completely discussed,

CHAPTER III.

OPINIONS OF SOME OF THE OLDER WRITERS.

IN a question purely of an anatomical nature, it would be wrong to appeal to the authority of the ancients; but their observations touching morbid appearances may certainly be received with some degree of confidence, especially when they correspond with those of more modern authors. Whatever acquaintance Hippocrates may have obtained respecting the structure of man, there is reason to infer that it must have been greater than many are disposed to acknowledge, and from the frequency of his appeals to comparative pathology, there can be no doubt that he considered it as a powerful auxiliary in illustrating the progress of human diseases.

The whole of what he has said respecting the diseases of the chest, bears wonderful testimony to the extent and accuracy of his observations, and it

probably may demand all the knowledge that modern enquiries have unfolded, to enable us to do justice to his labours. Doubtless there will be found some things that are conjectural and inaccurate, but there are also unequivocal proofs that he well understood the character of tuberculous diseases of the lungs; that he had the most distinct views of the symptoms, which they produce, their progress, and their terminations; and that he was familiar likewise with methods of exploring such diseases, which are thought to be the result of modern discoveries.

In a very important passage, wherein he discusses the necessity of accurate chirurgical knowledge in detecting the existence of different diseases, and passes a severe censure upon all who are ignorant of the best means of forming their diagnosis in the cases which he enumerates; among others he particularly specifies the method of discovering when pus exists within the thorax by means of percussion: his words are, *εμπυον εοντα, διασειοντα γινωσκειν.*

Περί νοσων I. Sect. v. p. 448. 43. Ed. Genev. 1657.

Again, when treating of the terminations of inflammation of the lungs, and accurately describing

the difference in that disease, when it terminates by expectoration, or by the formation of an abscess within the chest, he proceeds to give the following directions for finding out the latter occurrence :— the patient is to be fixed upon a firm seat ; *ετερος μεν τας χειρας εχετω : συ δε τουτον ωμον σειων, ακροαζεσθαι ες οκοτερον αν των πλευρων το παθος φοβει.* He goes on to observe, that this method of exploring is sometimes defeated by the state of the parts ; *ην δε σοι υπο τῃ παχεις και τῃ πληθεος μη φοβει, ωτε καταμαθειν αυτο, ποιειει γαρ τουτο ενιοτε,* &c.* *Id.* II. Sect v. p. 476. 47. et seq.

In another place, his testimony with regard to auscultation, in cases of water in the chest, is no less decisive : *ην πολλον χρονον προσεχων το ους ακαζη προς τα πλευρα.* This is evidently with reference to the sound emitted during the act of respiration, which puts the fluid in motion.

But there are many other passages in which the same mode of enquiry is recommended. Indeed they are altogether so numerous, as to put it beyond doubt that it was quite familiar to Hippocrates.

* In other places, his instructions are still more minute and precise ; thus on one occasion he recommends the examination to be made after the patient had been long immersed in a hot bath, and when the stomach is empty.

Take the two following examples, *Και ην μεν ενδον αποσημνη, δυσπετες γυνωαι· ουτε γαρ διασεισαντα εσιν ειδεναι.*—

Ibid. p. 453. 20.

Again, the same thing is enforced at p. 496, III. Sect. v. *καθισας επι εδρης ακινητου, ετερος δε των ωμων αναλαβετω· αυτος δε σειε τουτον, το ους παραβαλλων ες τας πλευρας, ιν' ειδης οκοτερωθεν αποσημαινει.*

He was also perfectly aware of the difference subsisting between diseases that have an inflammatory and a tuberculous source. The author's theory respecting the origin of tubercles from bile or pituita, has nothing to do with this part of the subject. It is quite sufficient to prove that he was aware of the existence of such bodies, and of the consequences they produce. Thus, after enumerating causes which may occasion a collection of pus in the lungs, he distinctly specifies tubercles as one of them, in the following passage: *Οκοσοι εμπυοι γινοντο τον πλευμονα, η την ανω η την κατω κοιλην, ην φυματα ισχουσιν, ειτε εν τη ανω κοιλη, ειτε εν τη κατω, η εν τω πλευμονι, η ελκεα ενδοθεν, &c.* And immediately afterwards, he specifies peripneumony as another: *ην περιπλευμονην ληφθεις μη καθαυθη, &c. εμπυος γινεται.* *Ibid.* p. 450. 23. 24. 25. 32. 34.

He also describes their terminations, and the manner in which a person affected with tubercles may recover. Οκοταν φλεγμα η χολη συσραφη, σηπεται· και εως μεν αν επι ωμοτερον εη, οδυνην τε παρεχει λεπτην, και βηχα ξηρην· οκοταν δε πεπαινηται, οδυνη γινεται και προσθεν και οπισθεν οξειη· και θερμαι λαμβανασι και βηξ ισχυρη· και ην μεν οτι ταχιστα πεπανθη, και ραγη και ανω τραπηται το πυον και αναπλυσθη παν, και κοιλη εν η το πλυον προσπεσθηται και αναξηρανθη, υγιης γινεται παντελως· ην δε ραγη μεν ολι ταχιστα και πεπανθη και ανακαθαιρηται, αποξηρανθη και δε πανταπασι μη δυναται, αλλ' αυλο αφ' εωυτε το φυμα αναδιδει το πυον· ολεθριον τστο. Ibid. pp. 453, 49, 454. And afterwards : προιονιος δε τε χρονε, ην μεν αναπλυση παν, και η κοιλη εν η το πυος ενη, προσπεση τε και αναξηρανθη, υγιης γινεται. Ibid. 24.

With the existence of hydatids, as a cause of watery effusion into the lungs or thorax, he was familiar ; but he goes farther, and in one part of the ensuing passage, clearly points to the identity of tubercles and hydatids, as well as to the latter becoming purulent.

“ Water between the skin (Υδερως) arises, he says, if tubercles are formed (or generated) upon the lungs, and have become full of water and burst into the chest (λα σηθεα.) But that the Υδερως arises from tubercles, this is my proof ; that

they are found in the ox, the dog, and the swine. For tubercles on the lung, which contain water, mostly exist (or are formed) in these quadrupeds. For you may presently know it on dissection, for the water will flow out. And such things seem to be formed in man much more than in cattle, by the how much we the rather employ a diet which brings on disease ; and many, tubercles having arisen in them, have become empused, *εμπυοι*—have suppurated.”

Γινέται δὲ καὶ ἡν φυματὰ ἐν τῷ πνεύμονι ἐμφυή, καὶ πλησθὴ ὑδατός, καὶ ραγὴ ἐς τὰ στήθεα ὥς δὲ γινέται καὶ ἀπὸ φυμῶν ὑδρὸς, ἴσδε μοι μαρτύριον, καὶ ἐν βοί, καὶ ἐν κυνί, καὶ ἐν υἱ· μαλίστα γὰρ τῶνδε τετραπόδων ἐν ἰουτοῖσι γινέται φυματὰ ἐν τῷ πνεύμονι ἀπερ ἔχουσιν ὑδῶρ· διαταμὼν γὰρ, γνοίης ταχίστα· ρευστὲται γὰρ ὑδῶρ· δοκεῖ δὲ καὶ ἐν ἀνθρώπῳ, γινεσθαι τοιαῦτα πολλῷ μᾶλλον ἢ ἐν προβάτοισιν, οὐκοῦν καὶ τῇ διαίτῃ ἐπινούσῳ χρεομέθεα μᾶλλον· ἐγένοντο δὲ πολλοὶ καὶ ἐμπυοὶ φυμῶν ἐγγενόμενων.

Hippocrates, Περὶ τῶν ἐνὶ στήθεσσι παθῶν. Sect. v. p. 544. 35.

It is a remarkable thing, that so few of the older writers should have mentioned these observations of Hippocrates. They are obscurely referred to by Galen, but they are not at all noticed by Celsus. Both Sennertus and Fernelius dwell particularly on

what is said about tubercles ; and our countryman, Morton, in his work *De Phthisi*, in the 3d chapter of the second book, very clearly shews that he was well acquainted with the genuine characters of tubercular consumption, as he states distinctly that the “*tussis phthisica a tumore glanduloso, seu a tuberculo ipsorum pulmonum oritur.*” There is no reason to believe that Sydenham had any accurate knowledge on this subject, nor are tubercles mentioned at all by Boerhaave, in his *Aphorisms*.* They are noticed by Hoffman, and by Willis ; but subsequently to their time, they seem to have been very much overlooked by the writers in this country ; for in the *Pathology of Consumption*, which is given by Fothergill, the tuberculous disorganization is not even mentioned.† Soon after

* But in his correspondence with Ruysch, which I quoted in my *Enquiry*, he alludes to the doctrines which I have attempted to unfold, in language so strong, so decided, that it is surprising it should have commanded so little attention. “*Atque ita quidem harum nos rerum contemplatio ad hydatidas sensum speculatione hac deduxit. Qui spherici tumores liquida primo turgent lymphæ, sensim degenerante, juxta varios in colore et crassitie mutata modos.*” &c.

Epistola ad F. Ruysch, p. 73.

† See Fothergill's *Papers in the Medical Observations and Enquiries*.

this period, the observations of Dr. Stark were published in the Medical Communications, by Dr. Carmichael Smith. Dr. Stark was cut off at an early age, before he had prosecuted his enquiry so far as he might have done. But we are certainly indebted to him for recalling the attention of medical men to a subject, which had, in a very unaccountable manner, been permitted to fall into neglect.

He was not aware of the primary condition of tubercles, for he describes them always as firm rounded bodies of different sizes; and he was of opinion that the tubercles of a small size were always solid. He has, however, described some of the other appearances of tubercles very accurately; and had he been permitted to extend his observations, there can be little doubt, that such parts of his information as were defective would have been corrected, and some inconsistencies in his statements obviated. He has, moreover, the merit of seeing very clearly the connexion between vomica and the tuberculous formations, though he was not at all aware of the affinity that exists between these diseases, and other changes of structure in different organs of the body.

In my Enquiry, I paid less regard to what Morgagni has said on this subject, than I ought to have done. This arose, in part, from finding very contradictory opinions in his work. There are, nevertheless, statements to be found in it of an instructive nature; and in one or two places he seems to have been so well apprized of the conversion of hydatids into tubercles, that one cannot but feel astonished that he did not apply that fact to its proper purpose. He has given several examples of tuberculous disease, both of the peritonæum and pleura. In a case mentioned in Letter 16, Art. 30, it is said, "The peritonæum also, where it invests the diaphragm, was rough with some kind of globules of various magnitudes and forms." The pleura, likewise, contiguous to the pericardium, exhibited the same appearance. Another case occurs in Letter 22, Art. 18, "The abdomen contained a little quantity of limpid water. The omentum was connected, by a small kind of ligaments, with the mesentery and peritonæum. These three parts, and the surface of the intestines, uterus, gall bladder, and bladder of urine, were unequal, with protuberating bodies, lying at a distance

from each other, up and down, and of different figures and magnitudes; yet so disposed, that in the upper part of the omentum they were very small, and in the lower part very large, and besides this very thickly sown and contiguous to each other. In the left lobe of the lungs, there was not only an ulcer, which contained a sanious ichor, but beside this, such a kind of bodies as were found in the mesentery, and other parts of the belly. Some of them contained a pus, some a matter almost like a poultice in its consistence, and others were still more solid, as if they resembled the nature of conglobated glands."

To me it is very interesting to observe, that this case, as well as the one before alluded to, were connected together, in the mind of Morgagni, with the changes which hydatids undergo. I discover this by some observations in the 38th Letter, Art. 35 and 36, which have a direct reference to the cases I have quoted. "Finally," he observes, "read over again what I have formerly written to you, of hard granules or tubercles being prominent on the internal surface of the peritonæum or pleura; as water was even then extravasated in the great ca-

vities, which those membranes surround, you will certainly find the series of successive changes, which I have described. It happened some years ago in a woman, that had been taken off by ascites; the external coat of the intestines was found to be distinguished with very frequent tubercles. Part of the small intestines was brought to me, that I might judge what these tubercles were. When I first examined them, they resembled small turgid lenticular glands; but they were without an orifice and solid, and seemed to be made up neither of a glandular nor of a fleshy substance, but to be of a middle nature, as it were betwixt both. I judged that I could determine on nothing more probable with regard to them, than to suppose that they were the remains of ruptured hydatids contracted into themselves, but not to so great a degree at present as to be dry and hard."

The changes in hydatids, referred to in the above extract, are as follows: He "believes that the membranous laminæ of hydatids, or of the coats in which they are formed, after they have, by rupture, poured out the fluid that they contained, first contract themselves and their vessels into the form

of a caruncle ; and, unless a fresh fluid continue to flow thither, are finally so indurated and dried up, as to present those white and hard tubercles of a roundish figure, some larger in their size and some less, as the hydatids had been, with which the internal surface of the peritonæum, in the virgin described, and the production of it through the external surface of the spleen and intestines, were beset.”*

Here it is plainly conceded, that hydatids have something to do in the formation of tubercles. So far I am thankful for the admission, coming as it does from such an authority. Morgagni does not often indulge in conjecture ; it is therefore rather surprising, that he should have fallen upon one so unfounded and gratuitous, as that which is quoted above, respecting the rupture and contraction of hydatids. He must have seen numberless instances in opposition to that conjecture ; and had he not entertained it, it is scarcely possible that he should not have applied the doctrine which is involved in the conversion of hydatids to the many disorganizations, which bear testimony to its truth.

* Lett. xxxviii. Art. 35.

In the 36th Article of the Letter, from which the foregoing extracts are taken, is the following remarkable expression: "You see therefore that the parts in the virgin, whose history I have given, which were rough with tubercles, have been, in other dropsical bodies, beset very thickly with hydatids; as the intestines, the spleen, and the peritonæum."

Had the author confined himself to the simple fact announced above, viz. that hydatids do become tubercles, and left his speculation as to the manner of their becoming so, out of the question, we should not, as we have since his time, been bewildered by such various and contradictory opinions respecting the origin of these bodies; and the abundant evidence which he himself supplies, as to their agency in the productions of tumours and other morbid growths in different parts of the bodies, could not have failed to have cleared away the many errors which encumber this branch of pathology. I trust also, that those who are inclined to pay that deference to names, which they sometimes refuse to well-supported arguments, will now admit that I have adduced authority of both kinds, and that they will receive it with such respect as it may

deserve. Let us take an example or two from the same writer, of the conversion of larger hydatids. "A woman, not yet advanced in age, and of a good complexion, had a tumour of the whole belly. She said that before this happened to her, a kind of tumour might have been felt on the right side of the navel, unequal in its surface, and of such a magnitude that it equalled almost the breadth of a hand when laid upon it. When she was supposed by some to have an ascites, for this very reason, which was doubted of by others on account of the natural colour of her face, she died. The transverse muscles of the abdomen being cut into, a great quantity of very stinking water passed forth, which was separated from the cavity of the belly by the peritonæum. This being exhausted, the tumour of which the woman had spoken, came into view, having been generated in the peritonæum, and consisting of two or three large bladders as it were, the parietes of which were so thick, that upon drawing out the water they contained, they did not at all subside or collapse."* To the same purpose is the following observation, quoted from Duverney, re-

* Lett. xxxviii, Art. 51.

specting a " woman thirty years of age, who had begun to have a tumour on her belly seven years before. On opening the abdomen after death, a large sac was found therein, containing many cells, not at all communicating one with the other, each of which was filled with a peculiar matter quite differing from the rest : which agrees very well with his account, that a serum of a different nature had been drawn off at different times."*

Notwithstanding the evidence afforded by these and many other cases, which the author has collected, he cannot divest himself of the false notion respecting the manner in which the changes in hydatids take place. Instead of following the course which is so obviously pointed out to him, and admitting that hydatids themselves, by being transformed in their coats and containing parts, occasion the various tumours which he describes, he is constantly recurring to his original supposition, that they burst and form cicatrices, and then coalesce, in consequence of new substances growing up around them. The formation of these cicatrices (but what they are, I really cannot com-

* Lett. xxxviii. Art. 68.

prehend,) seems to be the chief office that he assigns to hydatids. He gives numerous facts to correct his own judgment on this point; among the rest is the following, from Alexander Camerarius, “who found a considerable number of hydatids, containing a lymphic water, and comprehended in a membranous sac, wherein the steatoma of a man’s liver was at the same time included.*”

It appears to me quite superfluous to multiply examples of this kind. If the reader wishes for more information, he may consult either the Letter already referred to, or the Works of Bonetus, De Hàen, Bidloo, Wharton, &c.; and for an account of similar diseases in the inferior animals, I would refer to my Enquiry, and to the Work of M. Dupuy, the French veterinary surgeon, which I have frequently quoted in a preceding chapter.

On the whole, from the most attentive consideration of all that has been adduced now, as well as in my Enquiry, respecting peritonæal tubercles, or those which may be found in any other part, different though they may be in size and appearance, there can, I think, be no doubt as to

* Letter xxxviii. Art. 44.

the identity of their origin. All the general remarks which were formerly delivered, when we were treating of pulmonary tubercle, are applicable to kindred disorganizations, when they occur in the abdomen. A small number spread over the peritonæum, or in the mesentery, may exist without doing much injury. A greater number may produce universal accretion of the viscera, the extinction of the nutritive process, and a complete impediment to the functions of the alimentary canal. Should one or more of them grow from the ovaries or the uterus, or any other portion of the abdominal surface, they may increase till they attain a very great size, and then we may either have what has been denominated encysted dropsies, or ovarian dropsies, or tumours with different contents, and of different shapes, according to the transmutations which the hydatids may have undergone, or their collocation in the diseased structure. Again, should these bodies take their growth in any of the viscera, a series of the same changes may be detected, but the symptoms and the appearances will, of course, be modified by the part where they may happen to be generated.

The French writers claim for Bichat the merit of having first observed tubercles in the peritonæum. This is a mistake. In my Enquiry, I have shewn that the disease is noticed by De Haen, and others; and that Bichat's brief statements are neither consistent with each other, nor with the phenomena to which they refer. In pointing out this discordancy, the author has no intention of speaking disrespectfully of that eminent physiologist; he most sincerely venerates his genius, and most gratefully acknowledges himself his debtor for the instruction which he has derived from some of his works. Had all his researches been conducted with equal wisdom and caution, posterity would have hailed him as one of the most successful cultivators of medical science.

The extracts given in this chapter from Morgagni, prove that he too knew the tuberculated disease of the peritonæum; and I have, moreover, now to state, what escaped my notice on a former occasion, that one of the plates given by Dr. Baillie, in his Morbid Anatomy, likewise represents the same affection.

CHAPTER IV.

OPINIONS OF MODERN WRITERS.

IN the preceding pages, I have given a faithful description of the progress of tubercles. I have endeavoured to divest it of all extraneous matter, and to admit no expression, which has not a well-understood and definite meaning. My account has been drawn from minute attention to individual cases; and the conclusions have been established by finding the various morbid appearances existing together in the same viscus. Were it not that I had opportunities of thus watching the disorganizations *in transitu*, I might, perhaps, have felt myself disposed to multiply distinctions unnecessarily, and to establish varieties, where none in truth actually exist. I have enforced my statements by an appeal to the testimony of some of my predecessors. But I have not confined my illustrations to the human body; I have drawn addi

tional proofs from comparative pathology, and I hope hereafter to obtain, from the same channel, demonstrations of some opinions advanced in my Enquiry, which have been deemed of a doubtful nature.

In the prosecution of my design, I have now to say something of the writers of the present day; and first with regard to M. Bayle. His "*Recherches Sur la Phthisie Pulmonaire*" have been much applauded by succeeding writers, and by no one more than by M. Laennec. There is a seeming inconsistency in this, for the two authors differ much from each other, and the last-named gentleman delivers doctrines, which by no means accord with those of M. Bayle. It is very necessary to point out these discordances; for both these writers are appealed to, as if they coincided with each other, and as if the observations of the one elucidated and enforced the statements of the other.

There is some reason to believe that M. Bayle has fallen into an error, which he seemed anxious to avoid; that he has confounded the symptoms and appearances which arise from tuberculous disorganizations, with those which may depend upon

other diseases, which attack the thoracic viscera. His definition of phthisis pulmonalis clearly leads us to this conclusion. It is so comprehensive as to include almost every disorganization of the lungs, while it nowhere specifies that disorganization, which forms the basis of almost all the varieties of phthisis, which he enumerates. It seems, therefore, faulty both by stating too much and too little. It embraces diseases very dissimilar in character from pulmonary consumption, and it excludes what gives to the disease itself all its peculiarities. It is the more remarkable, that he should have committed such mistakes, as when he wrote the definition in question, he was giving what he conceived to be the essential character of the disease, while the very circumstance, that gives it that essential character, is kept out of sight altogether. "Voici," says he, "quel est, d'après le resultat de mes recherches, le caractére essentiel de la phthisie."

"On doit nommer phthisie pulmonaire *toute lésion du poumon qui, livrée à-elle-même, produit une disorganization progressive de ce viscère, à la suite*

de laquelle surviennent son ulceration, et enfin la mort."*

Doubtless we shall be told, as an apology for this definition, that it was meant to include other varieties of pulmonary disease, besides those which depend upon tubercles. But it is on this very ground, that the definition is objectionable. There may be other varieties, but they are so dissimilar from the tuberculous disease, as not to be, with any degree of propriety, included under the same general denomination. M. Laennec, notwithstanding the praise which he bestows on M. Bayle, seems quite aware of this; for his essential character of phthisis pulmonalis accords more with the opinions, which we have been expressing. "*L'existence,*" says he, "*des tubercules dans le poumon est la cause, et constitue le caractère anatomique propre de la phthisie pulmonaire.*"†

It is indispensably necessary to obtain just views on this subject. Wherever tubercles do exist, it is certain that a previous disorganizing process must have been going on, and there is no neces-

* Ch. I. p. 5.

† Vol. I. p. 10.

sary affinity between the diseases where they exist, and those where they are not to be found.

Such, however, were not the views of M. Bayle, as he gives six species of phthisis, one of which, at least, in its simple state, (the "*phthisie ulcéreuse*,") is affirmed to be totally independent of tubercles. This, I take to be an exceedingly questionable position, and I am glad to perceive that the same opinion of it is entertained by M. Laennec. He observes, "Quant aux especés décrites par M. Bayle sous les noms de *phthisie granuleuse*, *phthisie avec mélanose*, *phthisie ulcéreuse*, *phthisie calculeuse*, et *phthisie cancèreuse*, la première n'est, comme nous le verrons tout-à-l'heure, qu'une variété de la phthisie tuberculeuse; les quatre autres, au contraire, sont des affections qui n'ont de commun avec la phthisie tuberculeuse que d'exister dans le même organe, et qui rarement produisent l'effet dont cette maladie tire son nom, c'est-à-dire, la consommation."*

It is not easy to discover on what foundation the arrangement of Monsieur Bayle rests. His different species are not, as might have been expected, characterized by exclusive and specific qualities.

* *Id.* vol. i. p. 20.

On the contrary, they are so mingled together, as clearly to prove, that the names, which the author has assigned to what he conceives to be the various sorts of tubercles, as well as to the diseases which each variety is supposed to generate, are altogether arbitrary ; thus, in the first species, which is denominated the “ phthisie tuberculeuse,” we are told, that “ le poumon présente alors des tubercules enkystés, ou non enkystés. Ces tubercules sont formées par une substance, homogène toujours opaque, de couleur blanche ou d’un blanc sale, tantôt jaunâtre, tantôt grisâtre.”* Again we are told, that the size of the tubercles varies from that of a grain of millet to that of a chesnut, and that the miliary tubercles are, for the most part, excessively numerous. In the very next paragraph it is said, that these tubercles, which are formed of a homogeneous opaque substance, may exist in three different states ; that they are at first firm ; next, that they become soft in the centre ; and, lastly, that they are altogether destroyed by suppuration.

The doctrine announced in these words will be examined hereafter. All that I wish the reader

* Recherches sur la Phthisie, p. 21.

to notice at present is, that we have in this first species of phthisis *encysted tubercles*, and *non encysted tubercles*, and *miliary tubercles*; and moreover that these various tubercles, which are expressly said to be formed in one specified manner, may exist in three different states. But this is not all. The first species, we are told, is almost always complicated with the second; so that to the three varieties already given, we may add the "*granulations miliares*" and all their accompaniments. Thus, therefore, we find that all the varieties of tubercles, which the author has enumerated, may be found existing together in the same species of disease. It is not to controvert this statement that we offer it to the consideration of the reader, but to appeal to it in confirmation of the sentiments, which are invariably advocated in these pages, viz. that diversity of appearance in tubercles does not imply diversity of origin, but merely varieties in the transmutations of bodies, which were originally and fundamentally the same. Various opportunities will occur for elucidating this statement, when we come to examine the opinions of M. Laennec.

Before proceeding to do so, it may be as well to remove a misapprehension, which has arisen from a supposed resemblance in the opinions which M. Laennec and myself have respectively advanced. That resemblance has been conceived to be so great, as to induce some to suspect, that my descriptions must have been written with the knowledge of the doctrines which have been published by that author. This notion could only have been entertained by those who have imperfectly examined our several writings, and totally disregarded the dates of their publication. By a date inserted at page 53 of my Enquiry, it will appear that a considerable portion of it was printed before the 12th of March, 1818. In consequence of some unavoidable delays, the publication did not take place till about the same month in the ensuing year, which was at least six months before the work of M. Laennec came out at Paris.*

It seemed proper to make this statement; although the criticisms, which I now feel myself called upon to deliver, respecting that writer's opinions, will at once remove any suspicion, that the

* The date of his Dedication is 12^o kalendas Sextiles, 1819.

fancied coincidence may have occasioned. Possibly, I may be accused of arrogance in questioning the inferences of a gentleman, whose opportunities of making observations have been so very numerous. This I do with much diffidence and reluctance ; but I trust it will appear that I follow this course on grounds, which fully vindicate the conduct that I am about to pursue.

I am happy in being able to avail myself of the authority of M. Laennec in support of many of the positions which I have advanced : but there are opinions, which he maintains, that I mean briefly to shew to be not quite consistent either with each other, or with the phenomena of disease. The obscurity, which arises from such contrarieties, is considerably augmented by an intricate and involved arrangement of his varieties of tubercles, as well as by the employment of speculative expressions, to which no definite meaning can be attached. The statements of appearances, given by M. Laennec, taken separately, are, with some exception, I believe, faithful : that is to say, he has truly described what he has seen, or thinks he has seen. I do not mean by this to assert that he is not an ac-

curate observer ; but, that he has drawn inferences from some appearances, which do not coincide with the real progress of the disease. I think it will, moreover, appear, that he has not been able to disengage his mind from the trammels of preconceived opinions ; and these have, most unfortunately, obscured the subject on which he was engaged. Had I more slender authority than I have, I should feel exceedingly unwilling to make these remarks ; but, when I draw my proofs from the author's own writings, and shew that his conclusions are inconsistent and contradictory, it is for the interest of science that they should be pointed out. And I am the more moved to this, because, from the very mistakes and misapprehensions which exist, I am satisfied that the strongest illustrations of the true state of the case may be deduced.

Let us now, therefore, proceed to examine the opinions of M. Laennec respecting what he denominates the developement of tubercles in the lungs. I do not pretend to be able to comprehend exactly the meaning of many of his expressions ; but in order to prevent the possibility of misrepresentation, it is my intention to lay before the reader the various

passages in his work, which are peculiarly expressive of the sentiments of the writer. In vol. i. p. 21. he gives this statement: “ *Les tubercules se développent sous la forme de petits grains demi-transparens, gris, quelquefois même diaphanes et presque incolores*; leur grosseur varie depuis celle d’un grain de millet jusqu’à celle d’un grain de chenevis: en cet état, on peut les nommer *tubercules miliaires*. Ces grains grossissent, deviennent jaunâtres et opaques, d’abord au centre, et successivement, dans tout leur étendue. Les plus voisins se réunissent en se développant, et forment alors des masses plus ou moins volumineuses, d’un jaune pâle, opaques, et d’une densité analogue à celle des fromages les plus fermes: on les nomme alors *tubercules crus*.”

Excepting what is contained in the first part of the above quotation, the statement corresponds pretty closely with the description which I have already given. But as the difference which I have just remarked is of considerable consequence, it is very necessary in a discussion of this kind, that it should not be lost sight of.

It is not of much consequence whether tubercles be denominated *miliary* or not ; but these bodies, instead of being, at their first developement, demi-transparent, grey, and *sometimes* only pellucid, are, as I have already affirmed, so soft as not to be cognisable by the touch ; and so colourless and transparent, as to require nice observation to detect them. When they become demi-transparent, grey, or yellowish, it is a proof that they are in progress to a state different from that, in which they originally existed.

We have already seen that the *miliary* tubercles, according to the author, form the first step towards the production of the *tubercules crus*. But he has scarcely made this affirmation, before we have another mode of developement ascribed to these *tubercules crus* ; as may be seen in the concluding portion of the next section. “ Quelquefois même des masses tuberculeuses d’un grand volume se forment par suite d’une semblable imprégnation ou infiltration, et sans *développement préalable de tubercules miliaires*. Le tissu pulmonaire ainsi engorgé est dense, humide, tout-à-fait imperméable à l’air ; et lorsqu’on le coupe, les incisions présentent

une surface lisse et polie. A mesure que ces indurations passent a l'état de tubercules crus, on y voit se développer une multitude de très-petits points jaunes et opaques, qui, en se multipliant et en grossissant finissent par envahir la totalité de la portion endurcie."

From these passages it appears, that the same bodies may be developed in *two* or *more* ways; and from the words next in order, to which I beg the reader's attention, it appears, that there may be other methods, which the author has not distinctly specified.

"*De quelque manière que les tubercules crus se soient formés*, ils finissent, au bout d'un temps plus ou moins long et dont la durée paraît très variable, par se ramollir et se liquéfier. Ce ramollissement commence vers le centre de chaque masse, qui, de jour en jour, devient plus molle et plus humide, jusqu'à ce que le ramollissement eut gagné la circonférence et soit devenu complet."

It is with the observation contained in the first portion of this paragraph, that I am at present chiefly concerned. When viewed in conjunction with the author's other assertions, it indicates a

want of precision, and an inaccuracy, which ill becomes the work of a scientific enquirer. The notion which he discloses in the concluding part of this quotation, I strongly suspect, rests upon fallacious views of the phenomena which he describes. The delineation which I have already given of the progress of tubercles will explain what I mean by this remark. And I shall, before I conclude, advance other proofs in opposition to his representation.

Before we quit this part of the subject, it is necessary to advert to another instance of indistinctness and confusion in the author's method of delivering his sentiments. The 35th and 36th paragraphs at pp. 25, 26, 27, refer chiefly to what the writer calls false membranes, which are formed upon the coats of the tubercles,* after they have discharged their contents; and which membranes pass on to a cartilaginous degree of firmness. Now I do not altogether controvert the truth of such a representation, but the manner in which the discussion is carried on is eminently calculated to introduce confusion into the mind of the reader, and, of course, to prevent a due perception of what ac-

* Les parois se revêtent d'une sorte de fausse membrane.

tually occurs. If the reader will call to mind what has been delivered in the preceding chapters, he will perceive that these false membranes and cartilages, with all their author's cumbrous and unintelligible adjuncts, are nothing more than the coats of the tubercles in the different stages of their transmutation. Indeed, he appears himself at last to have arrived nearly at the same conclusion; for after having talked p. 28, No. 38, "de la membrane cartilagineuse des excavations pulmonaires, et sur quelques productions analogues qui se développent quelquefois dans le même cas;" he adds, "je n'ai jamais vu *ces kystes*, soit primitifs, soit consécutifs, passer, &c.;" and so truly and intimately are these *kystes* a part of the tuberculous formation that they constitute the "tubercules enkystés de M. Bayle."

The reader will be surprised, after all the intricate and involved discussion, which is to be found in the different paragraphs, on which we have been commenting, that this part of the subject should be summed up in p. 29, No. 40, by the following sentences: "Le mode de développement que nous venons de décrire est le plus commun, mais il

n'est pas l'unique. Il en existe deux autres, qui ne sont probablement que des variétés dans la marche de l'affection tuberculeuse, mais qui présentent cependant des différences assez marquées pour mériter d'être notées."

I cannot but confess that I read these words with very considerable astonishment ; and I am sure, every competent judge, who gives due attention to the subject, will join with me in expressing regret that such inconsistencies should be discoverable in such a work. Be it remembered, therefore, that in describing the most common mode of development of tubercles the author has already presented us with the following particulars : first, that they are small, demi-transparent grains, which pass through various stages till they acquire the density of the firmest cheese ; and are then denominated *tubercules crus*. By this, of course, we are led to understand, that the demi-transparent, or miliary stage is a necessary antecedent to the farther transmutations of tubercles. But it is not so : for the doctrine in question has scarcely been propounded, before it is asserted that these same *tubercules crus* may be formed by some other obscure

process, "*et sans développement préalable des tubercules miliaires.*" Now, here are at least two modes of developement; and, as has been already remarked, we are informed by the commencement of No. 31, p. 22. that there may be more. "*De quelque maniere que les tubercules crus se soient formés,*" &c.

Again, with regard to these tubercles themselves, we have a process described, from demi-transparency and softness, to density and firmness; and, lastly, this process is reversed: the centre of the tubercle begins to soften, and a progress from density to softness and fluidity is said to take place.

It may be that such things do occur; but all the analogies that may be derived from contemplating the functions of animals, either in their healthy or diseased states, teach us to believe, that the phenomena in question are accomplished by steps less intricate, by principles more simple and consistent. If we are to follow the author's method of ratiocination, a different name, and a different method of formation must be ascribed to every variety of appearance, which may be discovered in the body. Each peculiarity must be considered as an individual; and all the advantages

which man derives from a systematic arrangement, whether constructed upon artificial or natural principles, must be in a great degree lost. As well might we go about to describe every peculiarity of every leaf or every fruit which may grow on the same tree, as attempt to find essential and specific characters in morbid appearances, which merely indicate different conditions of parts which are fundamentally and originally the same.

Should it ultimately be proved that the views, which I have presumed to offer to the public, are erroneous, it cannot, I trust, be denied that there is at least simplicity and consistency in them. There seems also a striking adaptation to the phenomena of disease, and an accordance with the changes which we witness, that comes home to the mind with a power of demonstration that is seldom to be met with in medical pursuits.

I ventured, when I first offered my Enquiry to the public, to express some sentiments of this kind. Every succeeding day, and every succeeding observation, has brought to my mind the fullest conviction of the truth of the principle which I have espoused. I would seek no other evidence

than that which I could find in the volumes of M. Laennec, to substantiate that principle: but the same facts, when seen through one medium, may lead to a clear, explicit, intelligible, and consistent whole, which, when viewed through a channel of a different kind, yield nothing but varying, unsatisfactory, and contradictory details.

The different methods of developement ascribed to tubercles, and the opposing steps in their progress, which we have noticed above, amply justify these remarks. The softening of tubercles, as has been already hinted at, is the natural offspring of that doctrine, which maintains that they may be deposited in a solid state, without having undergone any preliminary changes. It is apparent that this doctrine rests upon assumptions, which do not admit of any direct proof. It is an inference drawn from appearances, which, when rightly understood, seem to lead to an opposite conclusion. I have already endeavoured to shew, that all the phenomena receive the most simple and satisfactory explanation, without having recourse to the intricate and incompatible processes above enumerated. We have only to apply that prin-

ciple which is admitted, as directing the progress of some tubercles, in a rational and philosophical manner, to explain all the deviations and peculiarities which have given rise to the notions we are controverting. When this is done, these apparent deviations and peculiarities will cease to perplex us, and will be seen rather as characteristic gradations in the series of changes which tubercles undergo.

In a preceding chapter, my opinions on this subject were stated very explicitly. As the topic is interesting, a brief recapitulation may not be improper here. At their commencement, then, we have seen that tubercles are minute, transparent, vesicular bodies. As they advance, they become changed, both in their contained and containing parts, observing a progress from transparency and fluidity to various degrees of density and firmness. We find, accordingly, on dissection, some tubercles perfectly condensed, others half condensed, and others, perhaps, but little changed from their primary state. What are we to infer from these things? Shall we say, that those which are not completely consolidated, are so, because they had

been previously hardened, and were afterwards, by a retrograde movement, brought into a softened state? Or shall we affirm that the reverse is the case; that the bodies in question, instead of undergoing this very unaccountable and improbable series of changes, are found in a semi-fluid or soft state, merely because the condensing process has not been completed. If we do not admit this, there is no reason why we should not proceed a step further, and suppose that tubercles, in their transparent state, instead of being what they really are, the first step in the series of morbid changes, are the last, and that they too are formed out of tubercles whose first condition was that of solidity or hardness. According to the one way of viewing the question, we have what appears to be a complicated, unnatural, and most unsatisfactory explanation afforded. According to the other, we have a series of phenomena, perfectly consistent with each other, and supported, as far as it is possible to support it, by the most direct and conclusive evidence. We actually see hydatids passing through all the changes which have been described; we find in the same viscus every link in the chain;

and with such evidence before us, it appears to me highly unphilosophical to seek for another and opposite mode of transformation, to account for appearances, which are, without one single exception, explicable without its aid.

Should these observations prove to be founded in truth, it is plain that a great source of error will be removed; for it is impossible, with such prejudices as I have now alluded to, standing in the way, ever to comprehend the principle, which it has been my object to establish, as applicable to the origin and progress of tuberculous diseases.

In a former part of this chapter, I alluded briefly to what M. Laennec has said respecting what he denominates false membranes, cartilaginous bodies, pulmonary excavations, and fistulæ; and proved, by the author's own statements, that all these various denominations, widely differing, as they seem to do, from each other, were, in truth, all applicable to one and the same portion of the disease, *viz.* the cyst of the tubercle.

We have already had to lament the misconceptions which have arisen from the misapplication of the word cyst. It is employed, as if it denoted

something different from the tubercle itself, when, in fact, it is essential to its existence, and without it, it would be divested of all the peculiarities which characterize this species of disorganization. Referring to the descriptions already given, of the progress of pulmonary tubercle, nothing is more easily explained than all these varieties of appearances, which seem to have excited such confusion in the author's mind, and caused him to employ the multifarious phraseology which we have quoted. In some cases, we have seen that the coat or cyst of the tubercle is the part that is first transformed; that it loses its transparency, increases in density or firmness, and becomes almost cartilaginous,* while the contents remain soft or curdy. Again, the transmutation goes on in a different manner, the consolidation beginning in the interior, and ultimately converting the whole into a solid tubercle of uniform texture, the amalgamation between the contained and containing parts being so complete, as to leave no traces of their original divisions. It is, I apprehend, from not duly considering these facts, that some tubercles have

* *Vide Enquiry, p. 93.*

been said to be encysted, while others are not. In the one instance, the distinct appearance of the cyst is lost in the uniformity of the solid tubercle; in the other, it is rendered more apparent than it was in its original state, by its increased thickness and firmness, and by the contrast which it exhibits to its half-transformed contents.

I have already expressed it as my opinion, that tubercles in the first-mentioned state, are not prone to fall into a state of ulceration, and that it chiefly occurs in those which are last described. When, by this means, such bodies have discharged their contents, the thickened coat or cyst is left behind, and presents to us, on dissection, those firm cartilaginous excavations, concerning which M. Laennec has spoken so variously, and to which, I apprehend, he has assigned an office, somewhat different from the true one. He looks upon them as the indications of a salutary process, by which a cure of tuberculous consumption may be effected. It certainly happens that individuals, who have expectorated the contents of tubercles, recover. If an individual, in whom such an event had taken place, should be examined after he had died of ano-

ther disease, the cartilaginous remnants of the broken down tubercles may be found. But the inference, which M. Laennec has drawn from such an occurrence, seems to be inaccurate. Let it be remembered, that the body in question is not the indication of a process, by which a cure has been effected. It is but the sign and the proof of previously existing disorganization: a disorganization which did not prove fatal; not because it was arrested in its progress by this barrier, but because the number of tubercles that fell into a state of ulceration, was small, and left sound lung enough for the functions of life, instead of carrying the patient to his grave, (as is usual where greater numbers exist,) by their successive decompositions, or by the continuance of a morbid action in the cyst which originally contained the tuberculous matter. This statement corresponds exactly with the opinion of Hippocrates, which has been given above. He required, as has been seen, for the recovery of persons affected with tubercles in the lungs, that when they fell into a state of ulceration, the matter in the tubercle should be wholly expectorated, and that the cyst (*κοιλίη*) which contained it should likewise be obliterated or healed up.

This simple and intelligible statement cannot be rendered more satisfactory or more consistent with truth, than it already is, by all the elaborate descriptions of the present day. When I wrote what I have said respecting Hippocrates, in the preceding chapter, I was not aware that some of the passages, which I have pointed out in reference to auscultation, had been noticed by M. Laennec; and, of course, I could not be conscious that some of my inferences were at variance with those of that gentleman. But, least of all could I have imagined, that he, while searching for the passages in question, should have omitted to notice the accurate information, concerning tuberculous disorganizations, which we have shewn that Hippocrates possessed.

Wherever tubercles have most prevailed, and have fallen into a state of ulceration, there we, of course, find the remains, just alluded to, most numerous. The unseemly clefts and fissures, which are seen in what is vulgarly denominated an abscess of the lungs, are occasioned by the successive decompositions of tubercles; and the size, and shape, and general character of such excavations, are de-

terminated by the magnitude, relative position, and number of these tubercles. What I have said, will be best understood by referring to the Plates. There may be seen single well defined cavities, occasioned by the decay of single tubercles, and, at no great distance, a large, uneven, irregular-shaped excavation, which has been the result of the dissolution of a greater number of contiguous tubercles.

Independent of the different methods of development assigned to tubercles by M. Laennec, on which we have already commented, he tells us that there exist two others, which, however, he admits, may be probably only "varieties in the march of the tuberculous affection, but which are, nevertheless, sufficiently marked to deserve to be noticed."*

I am not disposed to dwell at any length upon these anomalies, partly because I can attach no precise ideas to them, and partly because they are inconsistent with the previous statements. It is, moreover, curious to observe, that the author himself, in the course of a very few pages, again returns to the first proposition which we quoted, and thereby virtually destroys the whole of the reasoning, which

* Vol. i. p. 29.

he has built upon his capricious and incomprehensible divisions.

“Les granulations tuberculeuses, que l'on observe a la surface de la plevre et du peritoine, sont quelquefois incolores et tout-à-fait diaphanes, d'autres fois grises et seulement demi-transparentes. Dans l'un et l'autre etat, elles presentent souvent un point jaune et opaque au centre ; and quelquefois enfin, on les trouve converties en matiere tuberculeuse plus ou moins ramollie. Il n'est pas rare de voir tous ces divers degres de developpement sur la même membrane.”*

It is in vain to attempt to reconcile the explicit declarations of this passage with the involved and contradictory assertions, which we have been called upon to notice. It is utterly impossible that both can be true; and it may be hoped, that the attentive reader is by this time prepared to say on which side the evidence preponderates. The passage, which has been copied above, contains an account of the progress of tubercles precisely similar to that which was given in my Enquiry; and I appeal to it with peculiar satisfaction, both because

* Vol. i. p. 38.

it sustains the accuracy of that account, and fully justifies the observations, which other parts of M. Laennec's work have called forth.

I pass over the remaining chapters of the first part of this work, and proceed to make a few remarks on some of the author's opinions, as delivered in the second part. And, first, with regard to what he denominates "*Accidental Productions, developed in the Lungs.*"* There is something very perverse in this arrangement. Tubercles, according to the author's own declaration, are accidental productions, as much as those which are treated of in the present chapter. We can discover, therefore, no good reason for so much disjoining the one from the other. The attempt that is thus made to separate disorganizations which have the greatest affinity to each other, may possibly prove injurious to the advancement of sound pathological knowledge. This is not a question of mere technical arrangement, which might be decided either one way or another, without materially affecting our views of structural diseases. On the contrary, a right understanding of their nature is essentially

* *Vide* Chapter iv. Part 2.

involved in its issue. The whole of the reasoning contained in these pages, is founded upon the belief, that tubercles, of whatever size and of whatever properties, have one common origin ; and although the author's manner of treating this subject leads directly to a different conclusion, I mean here, as on former occasions, to show, that such a conclusion is at variance even with the facts which he himself enumerates. Take as an example the very names of the accidental productions which he proposes to describe. They are, " 1^o. les kystes proprement dit ; 2^o. les kystes contenant des vers vésiculaires ; 3^o. les masses fibreuses, cartilagineuses, osseuses ou osséo-crétaées ; 4^o. les tubercules ; 5^o. l'espèce de cancer que j'ai désigné sous le nom d'*Encephaloïde* ou de *Matière cérébriforme* ; 6^o. celui auquel j'ai donné le nom de *Melanose*."* Again, let the reader attentively consider what the author affirms in the following sentence ; it is a general remark applied to the disorganizations enumerated above. " Ce que je viens de dire des tubercules s'applique également aux autres espèces de tumeurs qui se développent, dans le poumon. J'ai trouvé

* Vol. i. p. 263.

dans cet organe des encéphaloides du volume du poing, autour des quels le tissu pulmonaire était tout-a-fait crépitant et ne présentait aucune trace de compression.”*

The admission contained in the first part of the last extract is of value to me, because it accords with the whole tenor of my enquiries; but it can be of no service to the author, as it tends more and more to point out his inconsistencies. We have only to look at his definition of the word *cyst*, to have a proof of this. “J’entends par *kystes*, avec la plupart des anatomistes modernes, une membrane accidentelle formant une sorte de sac sans ouverture, ordinairement obrond, quelquefois cependant irrégulier et anfractueux, et contenant une matière liquide ou demiliquide, secretée par la membrane même qui forme le kyste.”†

Now, I would ask the author, whether there is any thing in this description which corresponds with what he has said respecting tubercles; and whether his accounts would not lead the reader to believe, that they were productions in all respects different from each other? That they are not so,

* Id p. 266.

† Id. p. 267.

it has been my object to prove from the beginning; but if the author's views are correct, all attempts to establish their connection with each other, or to give an accurate and consistent description of the progress of tuberculous diseases, must be abandoned.

We have already seen how M. Dupuy was compelled, in opposition to his avowed opinions, to admit the connection between hydatids and tubercles. It is not so with M. Laennec; for he persists in maintaining his preconceived notions as to the peculiar and unchanging qualities of the cysts above described, even although the very next sentence in his work discloses facts that might have made him doubt the accuracy of his decision. "Il est encore une autre espèce de kystes; ce sont ceux qui renferment des substances plus solides et étrangères à l'économie animale saine, comme la matière tuberculeuse et les diverses espèces de cancers auxquelles ils servent seulement d'enveloppe."* Here, again, we have the ancient error presented to us in a new form. The opinion that a cyst is a formation *sui generis*, exhibiting certain properties, and

* Id. p. 265.

always preserving its independent functions, is not to be abandoned, although the strongest evidence be afforded that it is founded in a misapprehension. We have seen it already prevail to such an extent, as to introduce inextricable confusion into the description of tubercles ; and it meets us here, under circumstances which render its inaccuracy still more conspicuous.

The cysts, the definition of which was given above, *are hydatids*. The author is evidently very unwilling to admit this ; but I am enabled to prove that I am correct, by his own words. “ Les kystes sont, des toutes les productions accidentelles, celles qui se développent le plus rarement dans le poumon de l’homme : Morgagni n’en donne qu’ un seul exemple. Mais il n’est pas rare d’en trouver dans celui *des bœufs et des moutons*. Ces derniers sont ordinairement sérieux, contiennent un liquide ténu et tres-limpide, et sont formés par une membrane très-mince. Chez l’homme, au contraire, je n’ai jamais trouvé dans le poumon que des kystes composés de l’espèce de ceux que j’ai décrits ci-dessus, et j’en ai rencontré tout au plus trois ou quatre.”*

* Id. pp. 268, 269.

The passage just quoted, in the first place *identifies* cysts with the bodies which are commonly called *hydatids*; for I presume it will not be contended, that those described in the preceding extract as found in the lungs of cows and sheep are of a different description, even though what the author subsequently says about "*vers vesiculaires*," would teach us to believe that they are. In the next place, it is proved that these cysts contain sometimes a liquid or demi-liquid substance, or a tuberculous matter, or a cancerous matter; in short, that they become solid. But this simple statement does not suit the author's views. He must introduce specific differences, where none ought to exist, and thereby obscures an otherwise simple question. In consequence of this fondness for minute distinctions, we have two species of cysts set in opposition to each other: the first being a membranous sac, containing a liquid matter secreted by the membrane itself. Another species contains solid substances. But although, in the first-mentioned species, it is said that the contained parts are formed by the containing, in the last-mentioned, it seems to be inferred, that

this relation does not exist, in so far as they are merely considered as *envelopes* to the solid substances which they may enclose.

Surely this cannot be correct; for if we find such changes, as are here admitted to have taken place, in bodies of the same general character, it is not quite philosophical to contend, that what is admitted to be true of the one, is destitute of foundation when applied to the other. This is a question, in which technical objections and nosological arrangements can have no place. It matters not, whether *cysts* be called *hydatids* or *hydatids cysts*. I contend, that here, as in innumerable other instances, the transmutation of such bodies is clearly made out; and this I do with a full knowledge of what the author has said on this subject; for, so far from admitting the identity of *hydatids* and *cysts*, he avers the contrary, and tells us, that they have been a long time confounded with each other.* It is for him to reconcile the contradiction between this statement, with the declarations which we have extracted from his preceding article. This objection

* Id. p. 270.

cannot be removed by any opinions which the author may entertain respecting the species of hydatids. He may affirm, that the simple vesicle containing a limpid fluid, as it is found in the lungs of cows or sheep, is a cyst, but not an hydatid, while the disorganizations which form the subject of the third article are hydatids, but not cysts. This, in truth, if we rightly analyze his meaning, is the very expedient that he has recourse to. But it avails him nothing; for in the account of a case of *hydatids*, which were found in the lungs and in the liver, we find the following expression: “La partie du *kyste hydatique* qui était placée hors du foie,” &c. &c. Now, in this expression, it cannot be denied that the cyst and the hydatid are identified. In two other tumours, which were found in the thorax, there seems to be a degree of ambiguity on this point; but the first sentence of the author’s commentary on the case, at once shews the incorrectness of his own distinctions, and puts the matter beyond all dispute. “La description *des rapports des kystes* n’est pas assez détaillée dans cette observation,” &c.*

* Id. p. 276.

The next article in this chapter treats of cartilaginous, osseous, calculous, and cretaceous productions of the lungs. From the remarks which have been just delivered, the connection between cysts and hydatids has been established by the testimony of M. Laennec himself, in a manner not to be rebutted; and all the particulars included under the article which we are now examining, bear additional and convincing testimony to the truth of the other propositions which we are illustrating. It seems an invidious, truly it is a painful task, to detect so many examples of loose and ill-considered statements, which are delivered by the author; but as his influence is great, it is the more needful that it should not prevail, except when his authority is founded on accurate knowledge.

As we are at present merely considering tuberculous diseases, we shall not notice any disorganizations which may not be proved to be clearly connected with them. It is certain, that hydatids, among other changes which they undergo, sometimes show signs of ossification in their coats, or in their cavities, or become converted into cartilaginous, osseous, calculous, or cretaceous substances.

Descriptions of occurrences of this kind, as they have been met with in the lower animals as well as in man, were given in the Enquiry. Additional illustrations have since been presented to the reader, and more may be obtained from the article in M. Laennec's work, which we are now contemplating. Without, therefore, denying that bony matter, or cretaceous matter, may be deposited where there are no traces of the tuberculous formation, it is quite sufficient for my purpose, that they are at times connected with that formation, and that any, or all the varieties of the substances above alluded to, may be found within the same tubercle, or within the same cyst. It is by occurrences of this kind, that we are enabled to trace the connexion amid all the varied appearances of this class of diseases. In conformity with this statement, we are told by M. Laennec, that the imperfect ossifications are either encysted, or not encysted. The cysts themselves are sometimes imperfectly ossified: cretaceous matter likewise is generally found in cartilaginous cysts.* These statements are perfectly correct. They correspond

* *Vide* pp. 281 and 282.

with those which I formerly delivered; and they fully justify the inference which I have drawn from them; but that inference does not suit the author. Instead of considering them as varieties in the tuberculous formation, he returns to his theory about the cicatrization of pulmonary excavations, (which excavations, be it remembered, however much they may be disguised by diversity of names, are neither more nor less than the remnants of broken down tubercles;) and then writes as follows:—
“ D’après ces faits, je suis porté à croire que dans le plus grand nombre des cas, les concrétions osseuses et crétacées du poumon se sont développées à la suite d’une affection tuberculeuse guérie, et sont le produit des efforts de la nature, qui, cherchant à cicatriser les excavations pulmonaires, à développé avec trop d’exuberance le phosphate calcaire nécessaire à la formation des cartilages accidentels, qui constituent le plus souvent les fistules et les cicatrices pulmonaires.”*

The reader may determine how far this piece of pathology is consistent with the descriptions, given by the author, of “ encysted ossifications, and of

* Id. p. 287.

ossified cysts, and of cartilaginous cysts, containing cretaceous matter!"

On the subject of melanosis, I have little to say in addition to what was formerly stated. It seems generally to be associated with tubercles in some form or other; but in what relation the diseases stand to each other, I am not yet prepared to say. The next article in this chapter treats of the "*Encephaloïdes du Poumon*." These are the productions, which, in this country, have, with sufficient inaccuracy of expression, been generally included under the denomination of fungus hæmatodes. Many interesting examples of that kind were given by Mr. Langstaff, in his valuable paper, which was published in the Ninth Volume of the *Medico-Chirurgical Transactions*. The observations, which I took the liberty of making on these cases in my *Enquiry*, are quite at variance with those delivered by M. Laennec on the same subject. He tells us, that the cerebriiform matter may exist in three distinct forms; and that it also presents three distinct periods in its developements, viz. that of its formation, or of crudity; that of its complete developement, in which the resemblance

to the cerebral tissue is most complete; and that of its softening.

The reader, I trust, has not forgotten the remarks which were made, when a process analogous to this was affirmed to be discernible in the development of pulmonary tubercles. He has not, moreover, I trust, lost sight of the facts which were presented to him from the works of M. Dupuy, in a preceding chapter. If, to the evidence above referred to, he would add that which is to be found in various parts of my Enquiry, he will be prepared to judge, whether such a statement as is given by M. Laennec be compatible with the ordinary progress of disease. Here, as on former occasions, he seems entirely to have inverted the order of events, and to have assumed that to be the last stage in the morbid changes, which, there is the strongest reason to believe, was among the first. I need not repeat, that it is impossible to prove any of his statements, no man ever having seen a firm tumour in the lungs become a soft one; while the transition from softness to solidity is proved by numberless examples.

Without, however, dwelling upon this point, let us return to what may be considered as the specific character of tuberculous diseases, viz. their showing, in some shape or other, the traces of a formation originally encysted; and these we have most distinctly in the disorganizations which we are considering; and I must refer to what has been said in my Enquiry, as well as in the preceding pages, for an account of the manner in which the relative position, and magnitude, and contents, of different tubercles, when congregated together in any particular part, affect the sensible qualities of any disease which is the result of such a combination.

How far many of the disorganizations, which M. Laennec, in different places, accounts for by the word "*infiltration*," may be better explained, by a proper application of the facts above referred to, I am not prepared to say; because "*infiltration*," when applied to a living body, is a word to which I can attach no precise idea whatever. It is one of those general terms, which we have had occasion to disapprove of in another part of these illustrations; and we earnestly wish, that it had

not been found where it stands. We have seen, that the encysted cerebriform masses exist in three states, according to the author, the last being that of softness or liquidity, in which the matter is like that of thick pus; or, in other words, we have cysts containing solid substances, and others fluids of different degrees of consistency. These are the tumours and vomicæ which we have formerly alluded to, in describing the hydatical transformations. But it was not to point out this fact alone, that we have made these remarks at present. They bear upon another question, and guide us now, as they have done before, to a solution of many of the difficulties which are the result of the author's involved and contradictory statements.

I have ventured to call the third variety of his encysted "encephaloid" a vomica, because it corresponds exactly with the descriptions of that disease, as given by the best nosological writers, and with the account, likewise, which I presumed to deliver on a former occasion. The reader will be surprised to find, that I can adduce the authority of the author himself also, in support of this statement; that is to say, the descriptions of vomica,

and of the third variety of encysted encephaloid, so perfectly correspond, as to leave no manner of doubt that they apply to the same disease. “ Je regarde les vomiques, telles que les connaissent les praticiens, et que je viens de les décrire, comme le produit du ramollissement* d’une masse tuberculeuse d’un grand volume.”†

Compare the above with what follows: “ La matiere cérébriforme ne reste pas long-tems dans l’état que je viens de décrire ; elle tend sans cesse à se ramollir, et bientôt sa consistance égale à peine celle d’une bouillie un peu épaisse. Alors commence la troisième période : bientôt les progrès du ramollissement deviennent plus prompts, et la matiere cérébriforme arrive peu à peu à un état de liquidité semblable à celle d’un pus épais ; mais elle

* It is to be remembered, that the notion of a great mass of tuberculous matter being softened so as to produce a vomica, receives no countenance from me, for it is utterly at variance with all the best descriptions of the disease. The author asserts, that Hippocrates considers vomicae as true abscesses of the lungs, and the result of suppuration. Unless I have grievously misinterpreted his meaning, this statement is inaccurate. He labours with peculiar earnestness and minuteness, to establish the difference between inflammatory and tuberculous diseases. I have given an example at p. 66, and many more might be pointed out.

† Id. p. 116.

conserve toujours sa teinte blanchâtre ou d'un blanc rosé."*

Now, if we substitute the word tuberculous for cerebriiform, (which, knowing the diversity in the appearance of tubercles, we may do with perfect propriety,) there is nothing said of vomica which is not applicable to a disease, which is described under a totally different name in a remote part of the work. I have already controverted the author's opinion as to the softening process; but whether that opinion be true or not, is nothing to the present question. The inconsistency of the author stands conspicuous, whichever side of the argument we espouse.

There is another inconsistency subsidiary to the last, which it may be worth while to notice. I take it for granted that vomica, and the third variety of "encephaloid," are identical, and, of course, that the same accidents may happen to both: but the author, as if to maintain a distinction which has no solid foundation, tells us that it is not so; that, in the first-mentioned disease, the contents of the body which is formed by the *softening* of the tuberculous matter are evacuated,

* Id. p. 315.

while those of the encysted encephaloid remain within the cyst; or that the evacuation, or non-evacuation of the contents of the vomica, and the encysted encephaloid, constitutes the only point of difference between them; so that the same substance is a vomica, when its cyst is broken, and an encephaloid, when it remains entire."*

I now proceed to make a few remarks on what the author denominates the accidental productions of the pleura, and which may occasion an effusion of fluid into its cavity. The affections of the pleura, which occasion such occurrences, we are told, are chiefly cancerous productions and tubercles, developed on the surface of that membrane. The disorganizations described here, correspond with those of which I gave an account in the second part of my Enquiry; but the author's desire to multiply distinctions, is as manifest here as in other parts of his work; for, besides the cancerous productions, as they are called, there are two species of tubercles which are said to attack the membrane in question. In the last species, which accords with that which Bichat has slightly alluded

* Id. p. 319.

te, and which, I believe, in no respect differs from the others, except in size, we are told that hydrothorax is *always* an accompaniment.* Here we have another hardy assertion, uttered with much confidence; but it is unfortunately not consistent with fact. I have given several examples of this very disease, where, instead of effusion into the cavity, there was such perfect accretion of its sides, as to render effusion utterly impracticable. As far, therefore, as my experience goes, I should say, that accretion is a much more frequent termination than effusion; and the same remark is equally true, when applied to the peritonæum.

It was one great object of my Enquiry to prove, that the tubercles which attacked the serous membranes, were similar in their origin and progress to those which are generated in the lungs; and that the differences which they exhibit, were altogether to be ascribed to the varieties in the structure and functions of the parts, where they happen to exist. Enough, I would hope, has now been said to show that this statement is true. I have already proved that the account which was given of the progress

* Id. p. 417.

of tubercles, is, in a great degree, supported by several of M. Laennec's observations. We have an additional testimony in the following words:—
“ Quelquefois ces tubercules sont au premier degré, c'est-à-dire demi-transparentes, grisâtres ou presque incolores ; d'autre fois, au contraire, ils sont au second degré, c'est-à-dire jaunes et opaques. Je ne les ai jamais observés dans l'état de ramollissement.”*

This statement, so far as it goes, is correct ; but I have already shewn, that there is nearly as great a diversity of appearance in membranous tubercles, as there is in pulmonary. They almost always exist together ; and it is therefore but reasonable to conclude, that the peculiarities in the course of pulmonary tubercles, arise from the circumstances in which they are placed. The author admits, that in the membranes, tubercles do not undergo the softening process. I have endeavoured to show, that it does not take place in those in the lungs ; and this admission of the author adds strength to the reasoning which I have already employed. In the lungs, from being incessantly acted upon by the

* Id. p. 416.

external air, and exposed to the influence of the circulation of a vast volume of blood around them, and again interfering, as they necessarily must, with the due performance, both of respiration and circulation, we can easily comprehend how their appearance should be less uniform, and how they should be more prone to fall into a state of ulceration, than when they occur in other parts of the body. In the circumstances just alluded to, we have an easy solution of the peculiarities of pulmonary tubercle, without having recourse to the suppositions on which we have been animadverting in the preceding pages.

There are many other statements in M. Laennec's work which require to be examined; but having dwelt for the present, at sufficient length, on such parts of it as refer to tuberculous diseases, there is less occasion to consider what is contained in the other portions of his book. Before concluding, however, it is necessary to obviate the effects of what may be esteemed as rather an unfair criticism. The author tells us in his preface, that the arrangement of his materials has been rendered altogether subsidiary to the elucidation of the me-

chanical means, which he wishes to recommend for detecting and distinguishing the different diseases of the thorax. We are quite aware, that he considers the method of percussion, as introduced by Avenbrugger, as one of the most precious discoveries with which medicine was ever enriched.* With such a feeling, it is not to be wondered at that the author should render all other considera-

* It is not easy to find out on what principle this *great* discovery is ascribed to M. Avenbrugger. There is something very peculiar in M. Laennec's method of awarding his praise; and it is quite surprising how many imperfections are attached to one of the most precious inventions, "*dont la médecine se soit jamais enrichie.*" "*On ne peut nier cependant que cette méthode d'exploration ne laisse encore beaucoup à désirer. Elle n'indique souvent rien dans la phthisie pulmonaire, et dans aucun cas elle ne peut faire distinguer cette maladie de la péripneumonie chronique. Dans la péripneumonie même, elle est d'un faible secours quand l'engorgement inflammatoire occupe seulement le centre du pòumon, ou quand les deux pòumons sont affectés à-la-fois d'une manière légère ou à-peu-près égale; elle ne donne aucun signe propre à faire distinguer cette maladie de la pleurésie, de l'hydrothorax, ou de tout autre épanchement dans la plèvre. Elle ne fournit aucun moyen de reconnaître le pneumo-thorax, ou pleutôt elle devient une cause presque nécessaire d'erreur dans ce cas. Elle ne fait reconnaître les maladies du cœur que lorsque cet organe est devenu très-volumineux; et le plus souvent la mort arrive avant que la maladie soit parvenue à ce degré. Elle n'a jamais donné d'indices sur l'existence*

tions subordinate to such an invention, more especially as he bestowed much labour and care in devising aids for rendering this invention more extensive in its application, and more accurate in the information which it may communicate. Possibly there may be some who entertain less exalted notions of this discovery, and who, though they be willing to take every advantage of the assistance that it may afford, in finding out internal disorganizations, are nevertheless not quite satisfied of the propriety of breaking down the natural connexion which subsists between diseases of the same genus, merely for the sake of explaining the powers of a mechanical invention, concerning the utility of which great doubts may be entertained. Sure we are, that all the intelligence which such a contrivance can convey, in its best state, and in the most

de l'anévrysme des gros vaisseaux, que dans des cas où l'application de la main et la vue même en fournissaient de plus sûrs encore."

The reader may determine how far the more general *estimate* of the invention in question is compatible with this detailed enumeration of its demerits. I alluded to this matter before in a note at p. 21. I go further now, and say, that, with the exception of the cylinder, it would appear that Hippocrates knew and practised every method of exploring diseases of the thorax.

practised hands, never can compensate for the injury which is done to the cause of science, by disconnecting things which are allied together in every essential particular, and treating of them as if they belonged to different genera.

In various parts of these illustrations, earnest appeals are made against the "ill use" of words. It seems to be a fault which is carried to a most pernicious extent in many of the foreign schools; and there is too much reason to fear, that it is poisoning whatever is valuable among ourselves. It will not be thought that I am too zealous on this point, but by those who are ignorant of the extent and nature of the evil that I deprecate, and who are not aware that it has been pronounced by the highest authority* to be a subject of doubt, whether language, as it has been employed, has contributed more to the improvement or hindrance of knowledge among mankind.

It were easy to select very many objectionable expressions; but there are scarcely any that have struck us more forcibly than the word "apoplexy," applied to a disease of the lungs, and the word

* Locke.

“vegetation,” to an affection of the heart. On every account it is required of us, to make a stand against innovations of this kind. We have difficulties enough in our path; the road to precise and accurate knowledge, either respecting the healthy functions or morbid conditions of our frames, is sufficiently intricate, without having it obstructed and entangled by such unseemly contrivances.

I am quite aware, that the merit of giving the word “*vegetation*,” an application to an *animal* function, is not altogether due to M. Laennec. By employing it, however, he lends his authority to this, its mis-use; and so far he is answerable at the bar of reason for any injury it may occasion. Those who are not aware that any species of vegetation could go on within them, may be surprized to find that there is not only one, but two sorts. “*Deux espèces tres-distinctes de végétations peuvent se développer dans les cavités du cœur.*”*

A reference to an example of the first species is given, as it is described in the *Sepulchretum*, by Riviere. On looking to the case that is specified, no mention of vegetation, or any thing akin to it,

* Id. p. 334.

can be found. There was a displacement of the heart, and an ossification in the middle of it, and sandy matter in the cavity.* This is an observation of Platerus, not of Riviere, as is stated by M. Laennec. The one immediately preceding, which is taken from Garnerius, possibly may be the one that is meant, which describes a large fleshy excrescence that was found in the heart, and weighed one ounce and six drachms.

The other species of vegetations are called "vegetations globuleuses." The case connected with this species is a valuable one; and I make use of it, not certainly because it confirms the author's opinion with respect to the nature of the disorganization, but because it confirms the statements which are delivered both in the Enquiry and in these Illustrations. Putting aside, therefore, every notion connected with the word vegetation, let us attend to the facts. The lungs were filled with tubercles of different sizes and forms; some were hard, others softened, like soft cheese. The cavity of the right ventricle of the heart, presented, in different parts of its surface, vesicles, little larger

* Lib. ii. Sect. 8. Obs. 24.

than a pea. They were attached by pedicles to the columnæ carneæ. One of these vesicles, of the magnitude of a little cherry, occupied the point of this ventricle. These vesicles contained different matters; in several it was demi-liquid, and presented the appearance and colour of the lees of wine; in others, it was of a yellowish white colour, and of the consistence of pap; and in some others, on the contrary, there was nothing but a clot of blood, mixed with a small quantity of fibrine.*

It will require greater authority than that of Baron Corvisart, or M. Laennec, to justify the use of the word "vegetation," in such a case as is given above; and possibly we shall not be considered as presumptuous, if we say that no name, however distinguished in the regions of science, ought to command respect for such a statement as the following: "*Les vegetations globuleuses sont évidemment une nouvelle preuve de la possibilité de la concretion du sang sous l'influence de la vie.*"† It needs must be injurious to the cause of knowledge, to write after this manner. We are un-

* Vol. ii. p. 349.

† Id. p. 351.

willing to characterize it as it ought to be characterized; but we earnestly hope that such faults as it discloses may be avoided; for unless they be, it will be vain to talk of our progression in medicine.

The case itself is an interesting and instructive example of tuberculous or hydatical disease; and I very confidently refer to it for further demonstration of the manner in which tubercles are formed. The vesicles in the heart had not been completely transmuted: some of those in the lungs had been so; and if the word vegetation be applicable to the one, it is not less so to the other, and to all the accidental productions which M. Laennec has described, whether in the lungs or elsewhere. It is of no use to affirm, that the existence of pedicles, or the situation where the disease is developed, makes any difference in this question. Hydatids very often have pedicles, when they are generated within cavities. The very same formation may sometimes be traced, when they are lying upon the surface of the omentum or peritonæum. In such cases they are not pendulous, but they may be seen clustering together, and mutually connected by distinct filaments; in this respect exactly resem

bling the foot-stalks of a plant which unite the leaves to the branch. In the Enquiry there is one case recorded, where small tuberculated fringes hung into the cavities of the heart, from the mitral and semilunar valves; and in the ventricle they were likewise attached to the columnæ carneæ. In this case, there were no tubercles in the lungs; but in other respects it coincides with that of M. Laennec, and both doubtless are to be referred to the same general class of diseases.

Is it wise, let me ask, to give a different name, and to ascribe a different origin to every one of the bodies that have been mentioned in this chapter, because they may happen to differ in some of their sensible properties? Shall we not rather proceed on another principle, and, instead of attempting to establish an endless variety of specific differences, seek to find out the properties, or the qualities, which such bodies may have in common, and assign no more causes than are sufficient to explain the phenomena. The last method, it will not be denied, is at least most consistent with the principles of an enlightened philosophy; and, in the present

instance, I apprehend it will also appear that it is most consonant with fact.

Let us take, therefore, such a case as has been often described. Suppose, that, in the same viscus, we have hydatids ; the same bodies undergoing a change, either in their contents or in their coats, some of them completely changed, some into solid bodies, and others with a great variety in their consistence and texture. With such appearances presented to us, shall we be permitted to say, that hydatids are always to be considered as hydatids, though they were detected, half transformed, wholly transformed, and beginning to be transformed, at the same time and in the same viscus. Would it be right, in spite of such evidence, to affirm, as has been done, that hydatids never form cysts or tumours, or tubercles ; or, in short, that they can ever be any thing but hydatids ?

Formerly, I had occasion to lament the dominion which words exercise over the mind ; and I selected the word *hydatid* as an illustration of that evil influence. I wish I could say, that any thing which I had advanced had overthrown its power. But finding, as I do, that all the authority which

has been already brought forward, has been insufficient to accomplish this very desirable end, I feel it incumbent upon me to renew my efforts, in hopes that a reiteration of arguments may accomplish what the first statement could not effect.

It is evident, that when tubercles attain a considerable magnitude, whether in the lungs or in any other viscus, they must at first have been generated at considerable distances from each other. Were it not for this circumstance, if their growth happened to be simultaneous and progressive, it must have been speedily arrested by their approximation. Examples of this, we constantly see in the common pulmonary tubercle, where the number effectually interferes with the magnitude. But even in that disease, we sometimes find one or more bodies much larger than all the rest,

In consideration, therefore, of all these facts, I am again disposed to affirm, that all the bodies in question, however different they may be in size or in structure, were at their commencement similar to each other; and that, to subsequent changes which they undergo, both in respect to size and

structure, are to be ascribed all the varieties of appearances which tuberculous diseases present.

Neither cysts, nor tumours, nor vomicae, are to be exempted from this statement. Great as is the magnitude which the latter sometimes acquire, there is every reason to believe, that at their origin they had a beginning as minute as the smallest tubercle. The rate of the growth of such bodies in the human subject, it is almost impossible to ascertain; and it cannot even be very accurately found out in the lower animals. But there is great reason to believe, that at times it may be very rapid, though, generally speaking, it is slow and progressive. In the experiments of Dr. Jenner, I have seen the healthy rabbit transformed into one with a tuberculous liver in a very few weeks; and this fact, as far as it goes, gives us some slender analogy to guide us in this matter. But it is certain that cases have occurred, where both vomica and tumour have existed in the same lung, without affording any well-marked proofs that they were there. The growth of both being gradual, respiration and circulation become accommodated to the diminution of the capacity of the lungs; and no-

thing but experience could convince us of the vast extent to which that accommodation, in some instances, may take place.*

* For illustration of the foregoing facts, I would refer to the cases of Messrs. Langstaff, Howship, Dr. Lettsom, and the several others which I have delivered in different parts of my Enquiry.

CHAPTER V.

OPINIONS OF MODERN WRITERS.

THE doctrines which I espouse, have, I hope, been elucidated by a reference to the opinions of other pathological writers. Without doing so, I could not hope to establish my own ; nor could the reader be sufficiently aware of the beneficial effects of a candid examination of conflicting sentiments, in enabling us to establish the truth. In pursuit, therefore, of this object, it is necessary to say a few words concerning the work of another French writer of, I believe, considerable authority on the Continent. At all events, he puts forth very confident pretensions to public respect ; and if it should ultimately appear that his assertions are correct, I shall be compelled to acknowledge that all my reasonings are fallacious, and that all the evidence on which they rest, has been utterly misapplied. It is my wish to treat M. Broussais with every becom-

ing delicacy ; but holding at nought, as he does, all the knowledge and experience of his predecessors, and claiming to himself the merit of being the first to shed a bright and steady light over the whole range of chronic affections, it is the more needful to endeavour to ascertain whether he really is entitled to the merit which he claims. Judging from what he says in his preface to his *Examen de la Doctrine Medicale*, it would appear that some of his cotemporaries are not altogether disposed to bow with submission to his authority. Of any thing that is merely personal, we need not take heed ; but when an individual claims to himself a right to lead the judgment of his fellow-creatures in a matter of science, it is an object of some importance to find out whether he is endowed with such qualities as may make him a safe guide. When this question has been disposed of, other matters may be more easily adjusted.

If we were to judge by the author's professed regard to patient and minute enquiry, as well as to a rigid system of logical deductions, we might perhaps feel disposed to believe that his lofty pretensions were not without foundation. But on find-

ing, in the first page of the introduction,* a sweeping charge of ignorance and confusion against all the most illustrious of his predecessors, who had treated of chronic diseases; and perceiving likewise, in his preface, some assumptions not quite consistent with the caution and reserve of a modest reasoner, we have been induced to look at some of his statements a little more narrowly than we might otherwise have done.

The whole of his pathology of chronic diseases, may be summed up in the following sentence. "In fact, the very great majority of the unfortunate persons that I found consumed by a chronic malady, were simply victims of an inflammation which could not be cured in its acute stage."† This is rather a startling proposition. We are disposed most readily to admit, that many irremediable maladies are produced by unsubdued acute diseases. But, accustomed to look at such disor-

* *Histoire Des Phlegmasies Chroniques.*

† "En effet, la tres grande majorité des infortunés que je trouvai consumés par une maladie chronique, etaient tout simplement victimes d'une inflammation qui n'avait pu etre guerrie dans sa période d'acute."—Vide *Broussais Histoire de Phlegmasies.* Paris, 1816. *Pref. p. 14.*

ganizations as are described in this volume, as the product of something very different from that which we have ever understood to be the result of inflammation, in any of its kinds or degrees; we felt anxious to ascertain how the author would adapt this process to the very numerous, and, as we thought, contradictory offices which he assigns to it. Our difficulties began to be removed, when we arrived at his observations on inflammation in general. The first paragraph contains something like a *petitio principii*. He says, "If we run through the immortal work of Morgagni, we find, at every step, unequivocal traces of inflammation." This is taking for granted the very thing to be proved; in as far as men of no inconsiderable authority in our profession have thought, that such disorganizations indicated the agency of a principle very different from inflammation.

In order to accommodate matters, he rejects the ordinary definition of inflammation. He gives sundry modifications of inflammation, according to the difference of the tissues, and the vital properties of the part affected; and, after detailing the influence of these modifications upon the func-

tions in general, lays down the following definition as the result: "Toute exaltation locale des mouvemens organiques, assez considerables pour troubler l'harmonie des fonctions, et pour désorganiser le tissu ou elle est fixée, doit être considérée comme une inflammation."* And this simple inflammation, which effects all the disorganizations in man, has no less than sixteen modifications, besides peculiar influences upon the system in general; the whole hypothesis being constructed upon so comprehensive a scheme as to include, not only all the disorganizations which by possibility may occur, but "*likewise all the local exaltations of organic movements, which may trouble the harmony of the functions!*"

Formerly we entertained the notion, that definitions were intended to limit, and confine, and to mark, specific differences. Our author's method of proceeding, evidently rests upon other views. It is not his wish to circumscribe, but to enlarge, the meaning of his words; to make it so wide and comprehensive as to include any thing, and every thing, that he may choose to range under it; and

* Id. p. 54.

we could not help calling to mind the following expression: "Quod vero hujus remedium est (definitiones scilicet) in plurimis huic malo mederi nequit, quoniam et ipsæ definitiones ex verbis constant, et verba gignant verba."*

This engendering quality of words is happily illustrated by the author in every page. In his mind, they would seem to be associated with some distinct ideas, though they certainly fail in conveying precise information to the understanding of the reader. Were we to judge, indeed, by the unhesitating and oracular manner with which he describes the progress of disease, we would imagine that he had been introduced to a degree of familiarity with the healthy functions and morbid actions of all the parts and textures of the animal machine, which was quite unknown to all former enquirers. He knows to a tittle the offices destined to be performed by the sanguineous capillaries, and the white capillaries, not in one texture only, but in every texture; and he speaks with as much confidence of these offices, as if he had seen them all going on under circumstances which could admit

* Bacon de Augmentis Scientiarum, p. 274.

of no ambiguity ; as if he had acquired unlimited controul over all the functions of the body, and by some peculiar analytical process, had reduced them to their elements, and assigned to each its appropriate province.

Knowing the characters that are most prone to indulge in such loose assertions, it was no matter of surprise to us to find, in many places, signs of weakness and failure in the author's system. Independent of the varieties of inflammation already enumerated, he tells us that there are *innumerable* others.* His views on this subject would almost lead one to suspect, that he had been schooled in that vain philosophy chiefly conversant about primary forms and occult qualities, which used to beguile the human mind, and to divert it from more solid enquiry. He seems to have forgotten the evil influence of indefinite words upon the understanding ; and while he injudiciously admires and extols its powers, he much misapplies them in the discovery of truth.

These remarks have arisen from a due consideration of the tenor of the author's writings. He

* Id. p. 57, Tom. 1.

appears to have possessed many valuable opportunities for observation, and to have devoted himself zealously to the cultivation of his art. It is much to be regretted, therefore, that his illogical and dogmatical mode of pursuing his enquiries should have rendered them less fruitful than they might otherwise have been. Any one may discover this by merely reading the practical details, where we find so much that is conjectural and speculative, with what ought to be mere matter of fact, that it is impossible to separate the one from the other ; and the value of his experience, therefore, is much deteriorated. It is peculiarly to be lamented, that he should have fallen into errors of this kind, when treating of structural diseases. Regardless of all the distinctions, which are clearly to be discerned between them, and those which arise where no previous disorganization had existed, he unhesitatingly applies the same notion to account for the whole. Many facts, which might have proved somewhat refractory, in spite of the accommodating qualities of his hypothesis, he has been obliged to pass over without any notice. We have instances of this in his descriptions of the tuberculous disorganiza-

tions. His physiology, as he calls it, of tubercles, is entirely gratuitous ; and, so far as our knowledge extends, it is quite inconsistent with fact.

“ Je prendrai,”* says he “ acte de cette dégénération du système ganglionnaire pour faire l’observation suivante : ce sont les capillaires lymphatiques qui prédominent dans les glandes ; quand celles-ci sont long-temps irritées, même par sympathie et à raison de la phlegmasie de la membrane muqueuse voisine dont elles reçoivent les absorbans, elles sécrètent une matière caséiforme ; donc j’ai tout lieu de croire que cette matière est un produit de l’irritation de ces capillaires : je trouve le même produit en d’autres lieux où je n’aperçois pas de ganglions, tels sont le parenchyme des poumons, la plèvre, le péritoine, etc. ; mais j’y vois des faisceaux nombreux de capillaires absorbans qui s’en vont chargés des fluides qu’ils ont pompés dans les aréoles du tissu cellulaire, ou sur les surfaces sereuses du voisinage, et ces parties viennent d’éprouver une inflammation chronique, comme les muqueuses pulmonaire et bronchique l’avaient éprouvée dans les cas précédens : est-il si déraisonnable de penser que

* Examen de la Médecine, p. 304.

ces vaisseaux, quoiqu'ils ne soient pas ici disposés de manière à former ce qu'on appelle des glandes conglobées, aient pu donner un produit analogue à celui que je lesai vu sécréter dans les ganglions extérieurs, dans le mésentère et dans le médiastin?"

It ill becomes an author, who is very indignant against all who presume to use inaccurate language, to deliver his sentiments in this manner. In the first place, he affirms, that tubercle is a degeneration of the ganglionic system, arising from long continued irritation of the lymphatic capillaries, which had been excited by the inflammation of the contiguous mucous membrane; and then it is, that these lymphatic capillaries secrete a cheesy matter. Next we are told, that this ganglionic degeneration is produced in places where no ganglions are found, such as the parenchyma of the lungs, the pleura, peritonæum, &c. But to reconcile the contradiction just alluded to, we are informed, that there are many bundles of the capillary absorbents charged with fluids, which are pumped into the areolæ of the cellular tissue, or upon the neighbouring serous surfaces; and that these parts undergo a chronic inflammation, similar to what the mucous mem-

branes had previously undergone; and then he concludes, by asking, whether it be unreasonable to suppose, that though the vessels are not so arranged as to compose what are called conglobate glands, they may not afford a produce similar to what is seen secreted in the external glands, in the mesentery, and the mediastinum?

There is an overwhelming profusion of error in this statement. Without dwelling upon the first steps in the process, as laid down by the author, let us examine what he says as to the formation of tubercles, by the secretion of a cheesy matter by the absorbents. This secretion, we are expressly told, is the immediate consequence of irritated lymphatic vessels. Now, if there be any truth in the observations which have been already recorded in the preceding pages—if it be allowed that tubercles, in the first stage of their existence, contain no such cheesy matter at all—and that the existence of matter of this kind indicates only one of many changes which they undergo in their progress—what shall we say of the accuracy of an author who makes the occurrence above alluded to, at once the sign and the proof of a particular series of morbid

changes, even though tubercles may exist, without exhibiting that appearance which he holds to be characteristic of the genus?

The connexion between tubercles and the lymphatic system, is not at present the point at issue; and our opinions on that subject ought not to be confounded with the progress of tubercles themselves. Our author, as has been seen, affirms that these bodies are the result of a *caseous secretion* from inflamed lymphatics; and he is obliged to find *areolæ* for the deposit of this matter, in situations where one never heard of them before, and to which no precise notion can be attached. It is sufficient for our present purpose, to contend, that the author has not truly described the phenomena. He has assumed things as proved, which are very questionable: he has called to his aid agents, of whose existence there is no proof whatever; and he has totally disregarded the observations of all former pathologists. It materially concerned me, to dwell upon this point; for if it should be found that his views are correct, my labour has been in vain: and all the attempts that have been made to trace the progress of tuberculous diseases, would,

in that case, appear to have been futile and unproductive.

Another example of the author's abuse of general terms, may be found in the word "irritation." After using it very freely, he observes, "c'est le mot qui doit rallier tous les médecins à la vraie theorie, non seulement des phthisies pulmonaires, mais encore de la très-grande majorité des affections pathologiques."* We cannot better express our sentiments respecting such a term, than in the following words, used by a great author in reference to this very error: "At alterum genus perplexum est, et alte hærens; quod ex mala et imperita abstractione excitatur. Exempli gratia, accipiatur aliquod verbum ("*irritation*" si placet) et videamus quomodo sibi constant, quæ per hoc verbum significantur; et invenietur verbum istud, nihil aliud, quam nota confusa diversarum actionum, quæ nullam constantiam aut reductionem patiuntur."†

I now proceed to make some observations on Dr. Abercrombie's pathology of consumptive diseases.

* Id. p. 32.

† Novum Organum, pp. 15, 16. Sect. lx. Lib. i.

After the ample detail respecting the progress of tubercles in the lungs, which may be found in the foregoing pages ; and after having endeavoured to point out some of the inaccuracies and inconsistencies of the doctrine of M. Laennec, we shall have little difficulty, I trust, in making what is now to be said clear and intelligible.

If the statements which I have presumed to offer, be founded in truth, they cannot be less effective in shewing wherein Dr. Abercrombie has erred, than they were, in manifesting what appeared to me to be the misapprehensions of the French pathologists. I have more than once expressed my unwillingness to dwell upon points of this kind ; and nothing but a very earnest desire to forward the cause of knowledge, could induce me to undertake a task which cannot be executed without exposing myself, in some degree, to the imputation of indulging a controversial spirit ; than which, nothing is farther from my intention.

I shall first compare some of Dr. Abercrombie's opinions with each other, and then attempt to ascertain how far they are supported by facts. He divides the " morbid conditions" of the lungs, which

give rise to true consumption, under the following heads: First, the *White Tubercle*; second, the *Semi-transparent Tubercle*; third, the *Fleshy Tubercle*; fourth, the *Black Degeneration of the Lungs*, the *Melanosis* of the French writers; fifth, the *simple Hepatized Induration of the Lungs*.

It will not be needful, after what has been already said, to go minutely through all these divisions. Indeed, to do so, would, I fear, be an unprofitable labour; for after bestowing every attention on the author's remarks, I am obliged to confess, that I have been quite unable to extract from them any thing like a precise meaning. I observe, too, that what is intended for a simple, practical detail, is often made the vehicle of an immature hypothesis; and a history of disorganizations is thereby presented to the reader, which little corresponds with nature. We have had repeated causes to lament inaccuracies of this kind, in the course of the discussions into which we have entered; and it is peculiarly to be regretted, that they are to be found in the pages of such a writer as Dr. Abercrombie.

From the author's division of the morbid conditions of the lungs, already given, it appears, that there are three sorts of tubercles, and two other varieties of diseased texture. If the principle which he has adopted in his arrangement were of any value, the varieties might have been very much multiplied; for any of the diversities in the sensible qualities of tubercles, which have been already enumerated, would have formed as sufficient grounds for distinctions as those which he has adopted. I cannot suppose that the remarks, which have been presented on this point in former chapters, can have been forgotten. They are in every respect applicable to Dr. Abercrombie's arrangement. It is considerably more involved even than that of M. Laennec. The latter gentleman, we have seen, has in some very essential points confirmed what I have asserted to be the progress of pulmonary tubercle; but instead of abiding by the simple and direct course, he is led away by pre-conceived opinions.

Dr. Abercrombie seems to imagine, that his white tubercle and his fleshy tubercle, like M. Laennec's *tubercule crus*, are, by some unaccount-

able process, deposited in their solid state in the lungs. The reasoning which was applied to the examination of this opinion, on a former occasion, is in all respects suitable to the present; for by Dr. Abercrombie's second species of tubercle, it appears that he too admits that these bodies do exist in a soft and semi-transparent state. He even says, that there is reason to believe that, at a certain period of their progress, they become opaque.* This reluctant admission, as far as it goes, gives countenance to my assertions respecting the transmutations of these bodies. Seeing, therefore, that a progression, from semi-transparency and softness, to opacity, is admitted, according to the natural order of arrangement, one would have expected to find the second species in the situation of the first; but that would have been fatal to the author's notions, respecting the primary state of tuberculous disorganizations.

There is no reason to infer that melanosis has any *necessary* connexion with tubercles; and there can be no doubt, that this observation is more evidently true, in regard to what is called the hepa-

* Edinburgh Medical and Surgical Journal, Vol. xviii. p. 4.

tized induration of the lung. Dr. Abercrombie himself admits this ; for he says, that it may exist “ in a great extent, without the slightest appearance of tubercles.”* But this opinion is scarcely broached, before we find statements which tend to a directly opposite conclusion, as this very induration, which certainly is often found without the slightest appearance of tubercles, it is said, “ appears to form the basis of the pathology of consumption.”†

Unless it is meant to affirm, that tubercles have nothing to do with the pathology of consumption, the foregoing statements are incompatible with each other. I have endeavoured to shew how tubercles may occasion the induration of the surrounding lung ; but they certainly often exist, and cause death, without producing such effects, while the indurated or hepatized state, as it is called, may be induced without giving rise to any thing which bears the slightest analogy to a tubercle, or to pulmonary consumption.

* *Vide* Edinburgh Medical and Surgical Journal, Vol. xviii. p. 6.

† *Id.* p. 6.

Throughout the whole of this, as well as his former papers, the author seems entirely to have overlooked the different effects which arise from inflammation, when it attacks a part which had been previously the seat of a morbid growth, and when it falls upon parts not previously disorganized. This omission seems to be the cause of a good deal of the confusion which may be detected in his writings. Another thing worthy of remark at this time, is, the unhesitating manner in which he at one period talks of the nature of diseases of the lungs, while, at another, he writes in such a style as to show, that he himself had little or no confidence in the arrangement which he proposes. An example of this vacillation has been given in what has been said respecting the hepatized induration of the lungs. On one occasion, it is said, that it may exist without the slightest appearance of tubercles; on another, it is associated with tubercles, and is supposed to form the *basis* of the *pathology of consumption*. And again, it is disjoined from tubercle, the latter and the hepatized induration forming the opposite extremes of two distinct structures. Such, at least, is my inter-

pretation of the passage which is to be found, p. 21. vol. xviii. of the Edinburgh Medical Journal.* But the same inference is obviously to be deduced, from all that is delivered respecting the progress of the different diseases; and if what has been said in the preceding pages has not escaped the reader's memory, he can find no difficulty in explaining the contrarieties just alluded to.

I do not wish to affirm, with too much confidence or certainty, what I am now about to state; but from the best consideration that I have been able to give the subject, it appears to me, that

* “ The preceding remarks refer chiefly to the proper tubercular disease of the lungs. Some of them may be considered as in a great measure conjectural, and much observation is certainly required to enable us to arrive at any certain conclusions. But, in the present state of our knowledge, there is reason to believe, that *the induration of the substance of the lungs*, which lays the foundation for consumption, varies considerably in its characters in different cases, and in some of them consists of a mixture of various structures, differing considerably in their nature, and in the morbid changes which they undergo, and thus giving rise to considerable varieties in the characters of the disease. In the minute investigation of these varieties, little has hitherto been done; but I think we are fully warranted in establishing a distinction betwixt the two structures which seem to form the extremes—the white tubercle, and the simple hepatization.”

the condensed and liver-looking appearance of the lungs is not often an idiopathic disease; and that, in the majority of cases, it is the result of a previous disorganization in some portion of the thoracic viscera. I have already endeavoured to explain how tubercles give rise to such an appearance. They act as mechanical obstructions to the circulation of the blood; and it is very natural to suppose, that the lung which surrounds such bodies should, on some occasions, fall into the state in question. The same thing sometimes happens from effusions into the cavity of the chest, or the pericardium, or from any obstruction arising from disease of the heart, or any of the large vessels. An example of the last description is afforded by the following very interesting and instructive case of arteritis, accompanied with disease of the valves of the aorta and pulmonary artery. The subject of it was an athletic, robust young gentleman, of about twenty-three years of age. He was given to active, and, at times, to very violent exertion. For a considerable time before his death, he suffered much from difficult respiration, which at times was so much increased

by quick motion, as to threaten almost suffocation. On such occasions, the face and lips became almost livid; and a most violent cough, without any expectoration, was often likewise induced. The distress, too, was often much augmented, either by an empty state of the stomach, or by acidity and indigestion. Another symptom which gave him inexpressible distress, and which increased in violence, and became more frequent as the disease advanced, was what he called a rising of the stomach, but which in fact was a violent convulsive action of the heart itself and diaphragm, occasioned by an obstruction to the transmission of the blood.

For a long time, I felt at a loss to account for these symptoms, though the indications of impeded circulation were so manifest; yet, except during the convulsive actions, there was no irregularity of pulse; and though the cough was often violent and frequent, there was no expectoration. I once was inclined to attribute the symptoms to such an obliteration of the pulmonary texture, as happens when tubercles are consolidated and united together; of which I have given many examples. The symp-

toms, however, became of so urgent and marked a nature, that I was compelled to look to the heart and arterial system, for an explanation of many of them. Among these, were pains felt chiefly in the course of the large arterial trunks, tenderness of the joints, and a decided increase of pain when pressure was made upon these parts. Observations of this kind were chiefly made by pressing upon the humeral and popliteal arteries, and upon the wrists and knees. About this time, too, the nails became of an adunque form, and the hands and feet often became cold.

During the early part of the disease, this gentleman very often continued to use violent exercise. Possibly this may have interfered, in some degree, with the benefit that such remedies as were prescribed, might have afforded. But the disease was never in any effectual degree subdued by any thing that was done. Towards the last, the difficulty of breathing was constant, and at times more distressing than any thing I ever beheld. The pulse, too, was uniformly accelerated, being seldom under 120, but not irregular, except at the times already mentioned. During the last

few weeks of his life, bleeding, which had been occasionally used before, was very often repeated. In the early periods, the blood did not shew signs of inflammation : in the latter, it did so very strongly. The blood drawn, for a season mitigated the severity of the symptoms; but it had no effect in removing the disease, which proved fatal on the 23d of July, 1821.

On examining the body, the pleura pulmonalis was found adhering to the sternum; the pericardium was seen slightly inflamed on its outer surface; but there was no other mark of disease about the rest of the membranes. The lungs on both sides were of a very firm, condensed texture, of the deepest purple colour, and full of dark blood, which was squeezed out on pressure; but they *contained* neither tubercles nor any other disorganization. The heart itself did not appear unhealthy, except on its internal surfaces, and these appeared of a florid scarlet colour, as if they were fully injected. This appearance was especially remarkable about the semilunar valves, and along the inside of the aorta; and across these valves was observed a delicate, but strong and diaphanous membrane, placed

so as to occasion the greatest obstruction to the transmission of blood. Something of the same kind was observable, but in a less degree, about the mitral valves. Circumstances rendered it impossible to examine the whole of the arterial system; but the humeral artery was laid bare, and its internal coat examined, which was found of as bright a colour as the aorta itself. The abdominal viscera were very carefully examined, but no mark of disease was any where found.

I cannot help suspecting, that the view which I have given of this liver-coloured induration of the lungs, is very much supported by the examples of the disease which are brought forward by Dr. Abercrombie. None of them were of a pure or unmixed nature; for, with one doubtful exception, they were all accompanied, either with tubercles, or what the author denominates abscesses.

In my Enquiry, I endeavoured to show, that many sacs or cysts, which are found either in the lungs or elsewhere, and contain matter of a purulent appearance, are not, strictly speaking, abscesses. In this statement, I was supported by all the best pathological writers; and I in an especial manner

proved, that the disease which is denominated vomica, and which is still reasoned upon as if it were the produce of inflammation, proceeds from a very different cause. On this point, I gave a quotation from Sauvage, which is not unworthy of the reader's attention.

Dr. Abercrombie seems to have adopted some of the opinions of Laennec, which I have already endeavoured to prove erroneous. Alluding to the manner in which it is supposed that pulmonary consumption may be cured, he says, "The other state of tubercular disease," in which such an event may take place, "is when the tubercles are in circumscribed nodules of small extent, inclosed in distinct sacs, and surrounded by pulmonary substances in a healthy state. In this case, it is probable that the tubercular matter may become softened and suppurated, and may be entirely discharged by expectoration, leaving the sac empty; that the sac may then continue in a state of unhealthy ulceration, discharging matter; or that it may pass into a healthy state, presenting the appearance of a firm membranous cyst, with one or more bronchial tubes opening into it, the symptoms

ceasing with the entire discharge of the tubercular matter.”*

All that is accurate here, accords with the opinion of Hippocrates, as given in a former chapter. My principal reason for noticing the subject again, is to point out what appears to me to be an inaccuracy of some moment, in the preceding part of the extract. From it, an inexperienced reader would immediately conclude, that he was presented with another species of tubercle, which had not been mentioned in the previous enumeration, viz. a tubercle which is found in the shape of a circumscribed nodule, inclosed in a distinct sac. Would not any one infer from this, that this tuberculous nodule was independent of the sac, and altogether a separate formation ; that, in short, a sac or cyst of a particular kind was somehow or other superadded to the tubercle? Now, all this is highly inaccurate, and eminently calculated to mislead. The sac which Dr. Abercrombie thus disjoins from the tubercle, is a most essential part of that body ; for if the evidence which has been presented respecting the origin and progress of tubercles, is of

* Edinburgh Medical and Surgical Journal, Vol. xviii. p. 19.

any weight, it unequivocally proves that this sac, as it is called, is the part of the tubercle which is first evolved, and that the consequences which arise from these bodies, in a great measure depend upon the manner in which the transitions in this part of the tubercle are effected.

I am unwilling to fatigue the reader with needless repetitions; but as the subject is one of great interest, I am disposed to add a few more words, in the hope of rendering the last-mentioned point, if possible, still more explicit.

Tubercles in their incipient or vesicular state, as well as when they are transformed into solid bodies, exist in the lungs, without affording any of the pathognomonic signs of pulmonary consumption. In both there may be cough, or difficulty of breathing; but it is manifest that the expectoration, which tubercles in other states afford, cannot take place. But should any of these bodies, in passing through their changes, be brought into a state of active disease, a very different series of phenomena are presented. We have not only the symptoms which foreign bodies in the lungs occasion, but we have those also which arise from such bodies being broken down

in their texture, and yielding the various substances which they may have contained. This constitutes the genuine tuberculous phthisis. When, therefore, tubercles exist in the lungs, the greatest danger arises from their falling into this condition. Under favourable circumstances, there is reason to believe that they may be absorbed or preserved in a quiescent state, and that they may proceed towards consolidation, and ultimately cease to afford distress to the patient. Every solid tubercle that is found in the lungs of any individual who may have perished from any other malady, has passed through this process; and if they should happen to have been neither numerous nor large, they may have produced little or no inconvenience to the individual. Unfortunately, tubercles are generally produced in great numbers; and after they have advanced through some of their gradations, they are too liable to fall into a state of ulceration, before they reach that point at which such an event is less likely to occur. The appearance of the broken down coats of tubercles, which have thus discharged their contents, amid others that are perfectly consolidated, and perhaps, also, with others still re-

taining their transparent and original character, demonstrates to my mind these positions in such a satisfactory manner, that I cannot help being surprised that so many contradictory hypotheses should have been devised, to explain the phenomena.

From the whole, I think, it must be conceded, that tuberculous diseases, wherever they are situated, have an origin and a progress peculiar to themselves; and that, in disorganizing parts, wherever they may happen to be found, they advance in a very different manner from those other diseases which dissolve or change the healthy textures of the body, without any previous alteration of structure having taken place. The tuberculous disease, therefore, of the lungs, ought in no respect to be confounded with other diseases of that viscus, where no tubercles exist. Here we have one fixed point, from which our enquiries may proceed, and to which, likewise, we may look as a beacon, to guide our steps, when we are likely to be bewildered by the multiplicity of appearances which diseases exhibit in their last stages. By pursuing this method, we shall cease to confound

primary with secondary affections, and not identify diseases, which are dissimilar, with each other.

Whatever opinions may be entertained as to the doctrines which I have endeavoured to unfold, both in the Enquiry and preceding pages, I hold it to be established by incontrovertible evidence, that induration of the lungs, so far from being, as it has been supposed, the basis of other changes of structure, is itself a result of advanced pulmonary disease; that it may arise from various causes, and that there are many fatal instances of tubercular phthisis where it does not, strictly speaking, exist at all. It certainly is sometimes occasioned by the tuberculous disease, and it is also produced by other causes. But it is not a necessary concomitant of the former; and so little does it stand towards it, in the relation of cause, that we shall be much nearer the truth by reversing the proposition, and affirming, that induration of the pulmonary tissue is so far from causing tubercles, that it is rather caused by them, in as much as these bodies exist, in their early state, without one single trace of the induration alluded to. It is, moreover, necessary to remark, that *induration of the lung*

is an expression unfit to be used, as including the various diseases of that organ. The induration which proceeds from tubercles, when they are united together, cannot, with propriety, be called an induration of the *lung*; for the tuberculous disease has taken place of the pulmonary texture, and left in its stead a structure differing in all respects from the one whose situation it occupied. It is manifest, therefore, that there is a wide and most essential difference between induration of the *pulmonic texture*, which may have been occasioned either by tubercles, or independent of tubercles, and that other sort which is the result of hardened tubercles themselves.

Tubercles, as has been already stated, may be surrounded by the dark-coloured indurated lung. When acute inflammation attacks the lung in this state, the progress of the disease is generally rapid and fatal, and those dark-coloured gangrenous excavations, as they have been called, are produced, which have been described by different authors, the peculiar state of the surrounding lung modifying, in such cases, the progress and appearance of the tuberculous disease.

The same line of demarcation, which we have endeavoured to draw between tuberculous affections and the induration of which we have been speaking, must be maintained, while we examine other diseases, whether of the lungs, or of other parts. By doing so, our researches will be better defined, our results will be more simple, our information more precise; and we shall possibly avoid many of the contradictions which must necessarily continue to perplex us, so long as we conduct our investigations without sufficient regard to the essential differences of things.

Seeing that I have certain doctrines to support, it may be imagined that the evidence which I might adduce is not so completely exempted from all colour of prejudice and partiality, as to gain from the reader a willing assent. I am too well acquainted with the tendency of the human mind to represent every thing in that light which is most congenial to its own feelings, not to see that a certain degree of hesitation and reluctance, in the admission of every new opinion, forms a most essential element in the character of a candid enquirer. At the same time, it must be admitted,

that the caution and diligence which is here implied, is less frequently to be found than could be desired; and that the laudable resistance to novel doctrines is sometimes also extended to the evidence by which they are supported; and both are, without any examination, consigned to unmerited neglect, while there are numberless instances of a hasty and indiscriminate adoption of unsound and unsatisfactory speculations and hypotheses, which rest on no solid proof whatever.

In medicine, we have been so little accustomed to accurate and precise reasoning regarding the nature of diseases, and the action of remedies, that a more than usual degree of scepticism may perhaps be tolerated in our art. But, doubtless, the evil habits arising from faulty and defective methods of investigation, as well as the difficulty and obscurity of the subjects to which our minds are directed, has contributed to render medicine subject to all the variations and revolutions which have marked the progress of other branches of knowledge, without hitherto causing it to participate, in a proportionate degree, of their advancement. From this we might almost infer,

that the "latitude and longitude" of our field of enquiry have not yet been ascertained; that we are not perfectly acquainted with its boundaries, and the relation they bear to other contiguous sciences.

These few remarks seem not inapplicable to the subject which we are now discussing. Should the opinions which I have ventured again to urge upon the consideration of my professional brethren, be found to be correct, they cannot fail materially to elucidate the nature of many diseases, and consequently ultimately to influence the extent and application of the resources which we may possess for their removal. I originally approached the subject with much diffidence; and if longer study and deeper acquaintance with it has made me more confident, and more unhesitating in stating what I believe to be true, I trust that it will be found that, in doing so, I have neither indulged in self-conceit nor arrogance.

I am willing to claim assent to the propositions which I have advanced, not on proofs collected by myself, or by those who may feel an interest in supporting such propositions; but at once to place

the issue of the matter in question on the testimony of those, who either entertain opinions hostile to my own, or who have no opinions at all on the subject. All that I seek for, is a fair and correct statement of such appearances as may be found in tuberculous diseases in their different stages, by competent observers.

I have more than once affirmed, that the doctrine touching the origin and growth of tubercles is not to be confined to one viscus, or to one structure; but that it is, *mutatis mutandis*, applicable to all such bodies wherever they are found. In the preceding chapters, we were chiefly occupied in considering them as they exist in the lungs; let us now see what is to be collected from briefly tracing them in some other parts.

For this purpose, I mean to make a few observations on some passages of Dr. Abercrombie's paper on the organic affections of the brain. It would have been a great advantage, had his statements of morbid appearances been given without any opinions respecting their origin. From this not having been done, the very thing to be proved is sometimes assumed; and, on that account, there is difficulty in

separating what is conjectural from what is certain.

This error has probably arisen from the limited nature of the pathological views which Dr. Abercrombie has adopted. "I have formerly,"* says he, "proposed an opinion, that the diseases which come under this head, (*i. e.* "morbid changes in the substance of the brain and cerebellum,") are the result of chronic inflammation, affecting a small part of the brain; and I have endeavoured to trace its progress, from the state of simple inflammation to that of induration and final suppuration, in which it often terminates. In its early stage, I believe it is a disease which may be cured, and that it may continue a considerable time in this state; that after a certain time it passes into induration; and that, probably, there is in some cases formed around the indurated portion a sac of coagulable lymph. In this state it may continue for a long time, producing urgent symptoms, and may at last be fatal, by passing into suppuration, or without having suppurated. According to the stage of its progress, therefore, in which death takes place, we

* Edinburgh Medical and Surgical Journal, Vol. xv. p. 491.

may find a portion of the brain in a state of recent inflammation, or indurated, and of a red or yellowish colour ; or we may find a part of it hard, and a part suppurated ; or we may find the diseased part entirely suppurated. The simple induration we find chiefly when the patient dies of some other disease. When the disease itself is fatal, it is generally by a fresh inflammatory action ; and thus the indurated part often passes into suppuration. We find, accordingly, in such cases, other marks of the inflammatory state, as deposition of coagulable lymph, and serous effusion. The symptoms attending this form of the disease, both in the brain and cerebellum, will be seen from the cases. The striking distinction in the appearances, is between simple induration of the cerebral matter, and tumours imbedded in the substance of the brain. The term tumour seems to have been by many applied to both ; so that there is a great want of precision on this part of the subject. That which is properly called a tumour in the substance of the brain, perhaps should have a sac, by which it is separated from the cerebral matter ; and it remains to be investigated, whether such a tumour is

a portion of brain in a diseased state, and around which a sac of coagulable lymph has been formed by an inflammatory process ; or whether a distinct tumour may be formed in the substance of the brain, of the same kind as those which are formed on the surface."

Now, let us examine this quotation a little more closely. I feel great unwillingness to differ from any respectable author, and still more in saying any thing that by possibility could be construed into a mark of disrespect. As, however, a due regard to truth and consistency has made it necessary, that inaccurate or unsatisfactory reasoning should be exposed, wherever it is found, I am constrained to wave all other considerations, and to proceed at once to a painful duty.

First, therefore, it is supposed, that morbid changes in the brain, or the cerebellum, are produced by *chronic inflammation*. The different steps in the formation of these disorganizations are, if we may judge from the context,* first, recent depositions of a soft "gelatinous" matter ; next, induration, and the formation of a sac around the in-

* *Vide* p. 489.

durated portion, by means of coagulable lymph; and, finally, suppuration of the previously indurated mass. But the last portion of the paragraph by no means accords with this statement. It involves a question which it is necessary to solve in the first instance, before we can decide upon the opinions of the author. Indeed, it is a remarkable thing, that the question should have occurred to his mind at all, without inducing him to hesitate a little more respecting the pathology which he embraces. If the one supposition be admitted, the other must be rejected; for both cannot be true. The question, then, is no other than, whether these morbid changes arise merely from alterations in the *texture* and *consistency* of the *brain itself*, by reason of inflammatory action, or whether some of the most important of them are not to be ascribed to an agency of a perfectly different description?

Till this distinction is clearly understood, it is in vain to enter upon this investigation, with any hope of a satisfactory result. Without further preamble, therefore, I hope I may be permitted to repeat, that there is a most material difference,

both in the origin and progress of those diseases, which depend upon the growth of parts originally foreign to the body in its healthy state, and those which arise from dissolution or alteration of the original texture of our viscera, or of any other parts of our body, by the agency of inflammatory action, or of any other cause that may be supposed adequate to produce the effect.

It is matter of considerable surprise and regret to me, that this very necessary distinction should have been so much overlooked on the present occasion; and that an undue attachment should still be exhibited to obviously defective modes of reasoning, in spite of the observations which have been made by an eminent man, whose opinions, in general, have in this country been received with much confidence and respect.

I took the liberty, in my Enquiry, of referring to Mr. Hunter's sentiments on this point. Much as he did to elucidate the doctrines of inflammation, and much as he is looked up to as the parent of the fashionable pathology of the day, it is clear, from his own writings, that, with an unaccountable perversity of intellect, his most devoted followers

have applied his doctrines of inflammation, to account for the origin and progress of disorganizations, which he absolutely and unconditionally excluded from that part of his system.

Dr. Abercrombie must forgive me, when I say, that he is manifestly not to be exempted from this mistake. There are abundance of other passages in his writings, which very clearly shew how deeply it had taken hold of his mind; but there is evidence enough in the passage already quoted, without seeking for any others. Having thus endeavoured to clear the way a little in this intricate investigation, and again asserting, with the great author* already referred to, that the diseases in question are "neither preceded by inflammation, nor a consequence of it, and that they depend on a principle very different from inflammation," let us return to the passage which called forth these remarks.

There is the more reason to be surprised at such a misapprehension on the part of Dr. Abercrombie, as we find, in the second paragraph of the same paper from which we have made the fore-

* See Hunter on the Blood, p. 391.

going quotation, that some sentiments are avowed which seem in opposition to the theory he attempts to enforce, respecting organic changes in the brain and cerebellum. The paragraph just referred to, contains some intelligent observations; and after reading them, I felt no small degree of surprise in discovering the whole of them virtually contradicted in the course of a very few pages. "In tracing," he observes, "the symptoms of organic disease of the brain,* it is necessary to distinguish betwixt those symptoms which are properly connected with the disease itself, and those which immediately precede the fatal termination; and it is necessary to keep in view the changes which take place in the morbid appearances in connection with these fatal symptoms. It is chiefly when the patient dies of some other disease, without any change in the disease of the brain, that we find the morbid condition of that organ, in what may be called its original state. When the disease is fatal, it is generally by an attack of chronic inflammation; and in connection with this, important changes take place, both in the seat of the original disease, and in other

* Id. No. lxi. pp. 482, 483.

parts of the brain." "In these cases, besides the original organic disease, we find various morbid appearances connected with the fatal attack: such as effusion, suppuration, or a softened state of part of the brain; and in some of them, there is reason to believe that the organic disease itself, or part of it, has passed into suppuration. In some cases, again, an attack of this kind may occur, and may be carried off by the ordinary treatment, the original disease then going on, until another attack of inflammatory character is fatal." "Another remarkable variety occurs, in which the organic disease seems to produce no urgent symptoms, until the symptoms of chronic inflammation appear."

I hope the reader will indulge me, by comparing carefully the last-mentioned passages with that which I formerly quoted; and possibly he may discover, what he little expected to find, statements so various and contradictory, and so utterly irreconcilable with each other, as entirely to destroy the author's own hypothesis. We have, first, organic diseases of the brain represented as the result of chronic inflammation. Next, we find these same diseases advancing without any symptoms of chro-

nic inflammation; and, again, we are told, that chronic inflammation, which is assumed as the cause of these diseases, does not take place till they are proceeding to a fatal termination, and when it is excited by that very cause which itself is said to have produced. In one passage, he ascribes deposition, induration, suppuration, and the formation of a sac or cyst around the morbid mass, to chronic inflammation; while, in another, he affirms that tumours formed in this manner must have existed for a long time, without any symptoms of chronic inflammation whatever; these only having commenced (in the case alluded to) "six weeks before death."*

The following is the account of the dissection, to which the author himself refers, as illustrative of the last remark. "Much fluid in the ventricles; and the substance of the brain, in several places, very soft and much broken down. A large firm white tumour adhered by its base to the middle of the falx at its lower part on the right side. It was nearly five inches in circumference, at the broadest part, and about an inch and a half in thick-

* Id. p. 483.

ness. Internally, it was uniform white and firm, like coagulated albumen. It was imbedded in the substance of the right hemisphere, where it had formed a depression for itself, but without any adhesion to the substance of the brain. Its attachment to the falx was at the very lower part, and part of the tumour descended lower than the edge of the falx. There were two smaller tumours, the size of hazel nuts, but of the same appearance, attached to the falx, one at its posterior, and the other at its anterior extremity, both at the right side; and a fourth, also small, imbedded in the anterior lobe of the left hemisphere, and attached by a slender filament to the pia mater.”*

Notwithstanding the author's fondness for chronic inflammation, as the agent in morbid changes of the brain and cerebellum, it is difficult to find any thing in the tumours just described that affords the slightest support to his opinion; and he therefore might truly say, with respect to this and a vast majority of his cases, that the original organic disease had existed long anterior to, and independent of, chronic inflammation.

* Id. p. 495.

From a due consideration of all the phenomena of this, and other kindred diseases, I contend, that an inflammatory process is not that by which they are generated. There is the most conclusive evidence that cysts and sacs are not fashioned by the effusion of coagulable lymph around a diseased mass, or that the various appearances of these cysts, whether they be thick and firm, or slender and transparent—whether they be ossified, or show no signs of such a state—whether they contain a clear and watery substance, or a thick gelatinous purulent-looking matter, or be half-fluid or wholly so, or exhibit any of the above enumerated appearances in any variety of combinations—there is, I repeat it, the most conclusive evidence that such occurrences are not the result of any process analagous to inflammation. Further, I maintain, that whether there be one such body, or more than one, whether it be large or small, whether it be attached to the membranes or be imbedded in the brain itself, that all have one origin, and that it is common to the whole class of similar disorganizations in every part of the body.

I need not surely repeat, that the difference in the situation where such bodies are generated, must have a great effect upon their growth, upon the symptoms which they produce, and possibly also upon the changes which they themselves undergo. In some situations, a very small body of this kind may excite fatal symptoms, while in another it may grow to a vast size, with little or no disturbance of the system.

I hope the foregoing observations will prevent us from confounding what is clearly to be ascribed to the growth of foreign substances themselves, and the havoc which they may occasion by exciting diseased action in the part by which they are surrounded, with that other class of maladies, which arise, where no such disorganization previously existed. There is no organ, in respect to which it is more needful to keep this remark in mind, than the brain. Its vast importance in the animal economy, and the peculiarity of its texture, may possibly pre-dispose it to be more influenced by the growth of extraneous bodies, than other parts of our system. It cannot, however, be denied, that such bodies have been many times found

imbedded in its substance, and have produced death; while the contiguous parts afforded no signs, either of inflammatory action or of other disease. Take the two following examples.

The first occurred in the practice of Dr. Shute, my colleague at the Infirmary. A boy, about eight years of age, remarkable merely for having a large head, and for being very drowsy, was suddenly cut off without any strongly marked symptoms. On examining the head, a large cyst, not less than three inches in diameter, occupied the right hemisphere, immediately under the dura mater. In some parts it was transparent, or nearly so; in others it was of a greyish colour; and at the latter places, it was also considerably thickened. It was removed entire, with the greatest facility, from the surrounding parts, which did not exhibit any mark of disease whatever. This mass, when examined, proved to be an hydatid, undergoing some steps in its transformation. I have already mentioned the changes in the appearance of its coats. The fluid which it contained was of a bluish cast, and resembled very thin starch. It inclosed, however, besides, innumerable

small hydatids, some floating in the fluid, but the majority attached to its rough and thickened internal surface. Many of them were so small as to require the aid of a magnifying glass, to show their very delicate but perfect formation. Others were as large as peas; and many of this sort were completely transformed into tubercles, which, in colour and appearance, very much resembled pearls scattered over the surface of the large cyst.*

The next case which I shall mention, was that of a woman about fifty years of age. For a considerable time, she had complained of distress and uneasiness about the head. When I saw her first, she was sitting by the side of the fire, supporting her head upon the wall. She looked heavy and torpid, and with difficulty could be roused to speak. She could, however, when urged, give very satisfactory answers to questions, and could also walk about: but this she did with a very unsteady gait, and like a person in a state of intoxication. None of the other functions of the body were at this time much affected. But, soon, all the symptoms above enumerated began to increase. There was

* See Plate 1.

greater torpor, greater difficulty of moving; the circulation through the extremities became exceedingly languid, and the sensibility to external impressions, as well as the power of the voluntary muscles, gradually decayed, till death put a period to the disease. Her feet and hands, which had become cold and purple, and approached very nearly to a gangrenous state, gave her very great pain for a considerable time before she died. This pain she chiefly expressed by moaning deeply when they were touched.

On examining the head, nothing particular was found till we got into the centre of the brain: I there discovered a cyst of about the size of a goose's egg, occupying the situation of the lateral ventricles. But its main attachment was to the pineal gland. It was fixed to that body, and forced its way upwards, into the situation which I have already described. In attempting to detach it from this body, it was ruptured, and a very considerable quantity of a dark bloody-looking fluid escaped. The blood-vessels around its attachment were more numerous, and larger than usual; but there was no other mark of disease. The cyst, in this case,

was not thickened, as in the last ; and its texture altogether was more delicate, and it did not contain any other bodies of the same nature.

The observations which I deemed it proper to make, respecting Dr. Abercrombie's account of organic affections of the brain, are in a striking manner illustrated by referring to some of the author's earlier papers.

He first divides diseases of the brain into three classes—"the inflammatory, the apoplectic, and organic. Active inflammation of the brain, is in this country so uncommon, that some have doubted whether it really exists as an idiopathic disease. For this reason, I confine my observation to chronic inflammation. I include under this term, all those affections of the brain which, beginning with symptoms of an inflammatory nature, terminate either by suppuration or effusion ; and I do not comprehend serous apoplexy, which, beginning with apoplectic symptoms, belongs to another branch of the subject. Those affections, which I include under chronic inflammation, appear under various degrees of activity. Some of them are evidently examples of the pure serofulous inflam-

mation, while others approach to the character of acute phrenitis; and on this account, there may be some objection to the term. But as they pass into one another by almost insensible gradations, and are intimately allied in their symptoms and their terminations; and as none of them exhibit all the characters laid down by systematic writers, as those of phrenitis, it appears to me, that it will simplify the subject if we consider them all under the general term of chronic inflammation. The reasons will appear in the sequel, which lead to believe, that the varied forms in which we meet this affection, are not different diseases, but different forms in which we meet with the same disease.*

I beg the reader to consider well the import of this passage, and to compare it with others already quoted, but more especially with the first which I have submitted to his attention. It was taken from the Essay on the Organic Diseases of the Brain; and gives, I conceive, a summary of the author's opinions respecting the origin and progress of such diseases; and, under this belief, my re-

* Edinburgh Medical and Surgical Journal, Vol. xiv. pp. 265, 266.

marks were made. I hesitated much, before I offered them; but I see, from other passages, that the author considers *chronic inflammation* as the main agent in the production of *organic diseases*.* How this opinion can be reconciled with the classification announced above, which allots one place to *inflammatory*, and another to *organic diseases*, is more than I can presume to explain. It were easy, indeed, to afford proofs in addition to those already presented, that the difficulties on this head are quite insurmountable.

Surely, if medicine is ever to acquire any of the precision and certainty which other branches of knowledge have attained; if we are ever to unravel the perplexing intricacies which have hitherto so much retarded our acquaintance with the nature and progress of diseases, we must not give ourselves up to such vague methods of reasoning. It were infinitely better to abstain from all attempts at arrangement, and simply to detail symptoms and morbid appearances as they are found, without any regard to their causes, than first to adopt a classification which is abandoned, ere it is

* Id. p. 300.

well announced; and then to assume an imaginary condition of the brain, as the cause of diseases opposed alike to each other, in their origin, their progress, and their terminations. If such things are permitted, if medical men can rest satisfied with such explanations of diseases, there is nothing left for us but to deplore the unfortunate condition of our art, which, with all its boasted improvements, has evidently, in a class of disorders of the most frequent and fatal nature, not advanced one single step beyond its rudest and most elementary form. If it may be assumed, that diseases, avowedly different in their symptoms, and still more so in their terminations, may be the produce of one common cause; that that same cause may be in activity, or not in activity, may yield the symptoms by which it is supposed to be characterised, or may be silently effecting the greatest disorganizations in the body, without any symptoms at all; if, I say, such be the state of medical logic, we are evidently far from the road which leads to truth; we are wandering in darkness and in ignorance, and must abandon our errors, and cease to

confound dissimilar things, ere the light of science can shed its rays upon us.

On the whole, I must repeat it as my conviction, that one grand error pervades our reasoning respecting structural diseases. We identify the changes which arise from the growth of bodies, originally foreign to the healthy structure of the animal, with those which arise from diseased actions of parts, where no previous change of structure had existed, and ascribe the change of structure itself, and the consequences which it induces, to one and the same cause. It was one great object of my Enquiry, to mark the boundaries between these two classes of diseases. It little concerns me, at present, to speak of the origin of the various adventitious bodies which alter the structure of animals; it matters not to what cause they are assigned, provided it be kept clear from those other disorganizations, which are the result of diseases, independent of any previous alteration in the healthy texture.

If this distinction be observed, we can see our way clearly and distinctly through many intricate pathological questions; if we forget it, there

is nothing but inextricable confusion and contradiction. This division does not involve any theoretical questions at all, and would not lose any degree of its importance, were all the other opinions which I have endeavoured to enforce, respecting tuberculous diseases, proved to be utterly groundless. It rests upon facts which every one may ascertain; and numerous and decisive as they are, it is wonderful that they ever should have been overlooked. This affords a melancholy proof of the tendency of the human mind to error, of its attachment to ill-founded theories, and of their influence in fostering prejudices, and in actually overthrowing the testimony of the senses. Ever since the controversy between Ruysch and Malpighi, respecting the minute ramifications of blood-vessels, inflammatory action, of some sort or kind, has been held to be the sole agent in all the deviations from the healthy structure of animals. There have been abundance of disputes as to the nature of inflammation itself; and, as we have already seen, not a few contradictory offices have been assigned to it. In these respects, we have doubtless overstepped the boundaries originally assigned to

vascular action ; for surely the great Haller, when, in the pride of victory he exclaimed, "*Causa Ruyschiana vincit !*" never could have anticipated that such a pathology as is now chiefly in vogue, could have been reared, upon the doctrine to which he lent his great authority.

Many of the cases which have been detailed with so much industry by Dr. Abercrombie, may very fairly be ascribed to the effects of inflammation ; but there are many others, which, I am prepared to show, both from his own descriptions, and from corresponding cases in man and in other animals, cannot, with any regard to accuracy of language, be ascribed to such a cause. Many of them were unquestionably produced by another disorganizing process, which gave rise to cysts and tumours, long before any symptom of inflammation made its appearance, and which, we have already seen, may produce death, although no such symptoms ever had appeared at all.

It were needless to dwell upon the other contradictions which may be detected. If the distinction which I have endeavoured to point out, be founded in fact, any reader who keeps it in mind,

will easily discover them without further assistance. But there is one of the varieties of suppuration of the brain, as given by Dr. Abercrombie, which it may be as well to notice. He says, that there are "no less than four varieties, in the form of suppuration of the brain." I am quite aware of the great latitude that writers of the present day assume in the use of terms; but in the instance before us, it has been carried further than I could have anticipated. One of the varieties is denominated the encysted abscess; that is, a "distinct abscess, confined within a soft cyst, the surrounding cerebral substance being healthy."*

I doubt not that I shall be accused of vast ignorance, when I affirm, that an "encysted abscess" is a contradiction in terms. But so it must be admitted to be, if we are not prepared to sacrifice both pathological and nosological accuracy. The authority of Sauvage, on this point, may not perhaps be disregarded. "*Apostema abscessus, abscess, apostume*; est illud quod prægressa inflammatione phlegmonodeâ partem ipsam in quâ residet, in pus convertit, instertitiis ut plurimum musculorum con-

* Edinburgh Medical and Surgical Journal, Vol. xiv. p. 294.

tentum, *minimè vero capsulâ propriâ involutum*; differt itaque à vomicâ et ab apostasi." I had occasion to refer to this distinction in my Enquiry. Now, I have no hesitation in affirming, that what vomica is to the lungs, such cysts are to the brain. The cyst, as I have more than once affirmed, is the part that is developed in the earliest period of the disease. It advances slowly in its progress, and may either retain its original character, or be gradually transmuted into a body with very different properties, which produces symptoms and consequences varying according to the structure and function of the part where it is generated.

On taking leave of this part of the subject, it may not be improper to observe, that the diagnosis of diseases is necessarily rendered imperfect by incorrect notions respecting their origin. Diseases perfectly dissimilar in their nature and progress, are classed together. The consequence is obvious. Organic changes are ascribed to causes which never produced them. At one time, symptoms are appealed to as the ground of distinctions; at another, morbid appearances afford the specific differences. So that we have morbid appearances, without the

symptoms peculiar to the species ; and symptoms, without the morbid appearances which they are said to characterize.

I have satisfaction in observing, that some of the opinions which I published respecting the formation of tumours, have been sanctioned by Mr. Charles Bell, in a paper recently printed in the 12th volume of the Medico-Chirurgical Transactions. At least I infer from his statement, that hydatids are transformed, and become tubercles ; and that it is to the aggregation and relative position and contents of these tubercles, that the species of disease which he describes as the *carcinoma mammæ hydatidis*, owes its character. " This tumour," he observes, " when cut into, does not exhibit a concentrated mass, but is distinguishable into parts or clusters of lesser tumours. When these sub-divisions are cut into, they present the most common carcinomatous appearance, being firm in texture, and having the ligamentous bands, both forming areolæ and diverging lines ; and these are distinguishable by their whiteness from the matter they embrace. In the interstices of the tubercles, some larger bags or cells are found,

of a yellowish or amber colour. These cells are of various sizes; and the larger ones contain a darker fluid, like blood or bile. It is the developement and remarkable increase of one or two of these cells, which give a prominent and square character to this species of carcinomatous mamma."*

The name assigned to this species of tumour by Mr. Bell, but, still more, the internal evidence contained in this quotation, justify me in affirming, what has been proved by numberless other examples, that hydatids, transmuted and arranged in certain forms, give to this class of disorganizations their peculiar characters. This principle is not applicable to one species of disease alone, as I have shewn elsewhere. After making use of it, to explain varieties in the structure of tumours, and attempting to illustrate my meaning by diagrams, I observed, "let the reader call to mind what has been said respecting the contents of tubercles. Next, let him suppose, that the lines in the third diagram represent bodies of that kind; that each of these bodies may have a different structure; that one may be schirrous, or cartilaginous; ano-

* *Vide Medico-Chirug. Transact. Vol. xii. p. 225.*

ther may contain a pulpy matter ; a third, a fluid like cream ; and a fourth may possess its original character of an hydatid ; or that any of the other varieties of substances or fluids, which are detected in such bodies, may be there. Again, let him suppose that each tubercle was at first separate, the one from the other ; but that as they increased in size, their distance from each other, of course, diminished, and that ultimately they came in contact, and united. What would be the result of such an event ? A tumour would be formed, divided by septa, and containing substances of various descriptions.— But suppose that the tubercles had proceeded simultaneously in their changes, and that they did not differ in their structure ; in that case, the tumour formed by their union would of course have a greater uniformity of appearance. The original divisions might in some places be visible, and the tuberculous character likewise ; but both may be obliterated ; and, instead of a structure marking the boundaries and arrangement of the elementary parts of the diseased mass, we may find the whole transmuted and condensed into a

solid substance, with little or no variety of texture."*

If the reader has any doubt respecting the accuracy of the foregoing statement, I would beg him to compare it with the extract which is given from Mr. Bell's paper. It has been supposed, that I delivered a conjectural representation, while in truth it was most rigidly deduced from facts presented to my mind; and I have no hesitation in repeating, that the peculiarities in the arrangement of the component parts of tumours of this kind, are well accounted for by the principle which I endeavoured to unfold.

Mr. Bell has fixed upon the developement and increase of one or more of the cells which he describes, as the points which give to this species of carcinomatous mamma its distinctive character. If this be the only mark by which its specific difference is to be ascertained, it is quite insufficient for such a purpose; for, except in tumours which are composed of one cyst, it may be common to all, and of course can be peculiar to none.

I have applied the principle involved in this

* Enquiry, p. 221.

statement, to explain the cause of the irregularities, both in the feel and appearance of tumours, as well as to account for the differences of texture, and lobulated forms, that we detect when we make a section of such diseased structures.*

In reference to this point, and after describing an illustrative case, where the principal part of the disorganization was in the thigh, I deemed it proper to make the following, among other remarks: "In this case, it appears that some tubercles were distinct and independent, others were coalescing, and were losing their tuberculous character, while others had coalesced, and lost it altogether. It is manifest, that an examination of this morbid structure, in its earlier stages, would have shewn its tuberculous origin in a more extensive, but not in a more convincing degree. At that time, too, it is probable that the contents of the tubercles would have been found to have been different from what they were in their more advanced stages. Analogy favours this supposition; and it receives strong support from the state of the tubercles in the lungs and in the peritonæum. Proofs and illustrations

* *Vide Enquiry, pp. 26, 27.*

of this opinion may be drawn from the works of every author, who has written on this class of diseases. They differ as much about their origin as they do about their names; but neither of these circumstances can affect this discussion. Cases of cancer, of tuberculated sarcoma, of fungus hæmatodes, and many other varieties of morbid growth, may be demonstrated, by the very descriptions which the authors themselves give, to have been formed as I have described.”*

That the principle now delivered, is applicable to all tumours of this class, cannot, I conceive, be questioned by any gentleman who is competent to examine the evidence on which it rests. The case which I have quoted from Mr. Bell, unquestionably arose from a cluster of hydatids generated in the mamma. If we give due attention to what is implied in this statement, and consider what has been demonstrated to happen to these bodies, as their transformation goes on, and at the same time bear in mind the explanations which have been given, touching the morbid appearances which arise from such transformations, we can be at no loss in

* Id. p. 228.

accounting for the diversity of appearances, either in the cysts or in the instertial matter.

In my Enquiry, I have affirmed that, amid the great variety of appearances which are found in diseases classed under the head of fungus hæmatodes, the remains of the hydatical character may be most distinctly traced. I adduced satisfactory proofs of the correctness of this opinion, from the works of various authors. Subsequent observation has enabled me to give an elucidation of the progress of this disease in the muscular textures of the body. In such situations, we should scarcely, *a priori*, expect to find the appearances which I am about to describe. Although, when we consider their nature, they perfectly harmonize with what have been detected in kindred disorganizations in other parts of the body.

A section of a muscle, when first attacked by the disease in question, presents to the eye a reticulated-looking texture, with the cavities filled up with a transparent gelatinous-looking substance; the original fibrous structure of the muscle being completely supplanted by this new formation. That is to say, a congeries of transparent vesicles

are seen, instead of red muscular fibre. In other parts of the same disease, where its progress has been further advanced, this simple and elementary form is lost; the vesicles and their contents are no longer transparent; another process is going on, which ultimately transmutes all the parts which it attacks, into those irregular *cerebri*-form-looking masses, that are supposed to be characteristic of this disease in its advanced stages.

My meaning will be best illustrated by the following case. The disease attacked the knee of a young man. It grew to a great size, and spread a considerable way up the thigh. The limb was removed by amputation. A longitudinal section of the diseased structure beautifully exemplified the facts which I have stated. Around the joint, where the disease had existed longest, and where it had acquired the greatest magnitude, all the original organic properties were destroyed; and such a mass as I have just described, appeared in their stead. As we advanced upward from the extreme point of morbid transformation, the gradations in the process became evident, in a very striking and instructive manner. First, the density and colour

changed; next, the arrangement of the parts of the diseased structure became less confused; next, the boundaries of some of the muscles could be traced; next, the vesicular and semi-transparent state of the disease shewed itself; and, lastly, this appearance became less and less manifest, till it was lost in the healthy muscular texture.

CHAPTER VI.

REMARKS ON THE TREATMENT OF TUBERCULOUS DISEASES.

IT is not my intention, at present, to enter at length upon the subject of the treatment of such disorganizations as we have been considering in the preceding chapters. There are, nevertheless, several remarks suggested by the doctrines which we have endeavoured to support, that bear so much, both upon the prevention and removal of these diseases, that it would be wrong to pass them by without notice.

In the inferior animals, the circumstances which seem chiefly to pre-dispose to the generation of tuberculous diseases, are cold, moisture, and bad food. In the human subject, it cannot be doubted that the same causes are equally influential. It is likewise to be observed, that when the animal frame has been deteriorated, from whatever cause that deterioration may have arisen, there is a disposition

to transmit to the offspring of such animal the impaired constitution of the parent. All who are conversant in the rearing and breeding of cattle, are well acquainted with the influence of this principle; and that it has a manifest connexion with the qualities of the human race, is very generally admitted.

In no class of diseases is the effect of hereditary taint more frequently observed, than in pulmonary tubercles. The knowledge of this fact, and due attention to the pre-disposing causes, supply us with information which ought not to be neglected, when we are devising such means as may tend to obviate, or to remove, one of the most fatal and frequent scourges of our race.

The information which may be collected from well-constructed experiments, regarding the propagation and removal of tuberculous diseases among the inferior animals, is a subject which is exceedingly worthy of the attention of the medical philosopher. We are not altogether without some accurate information on this point; but it is earnestly to be desired that we were possessed of much more. It is certain, from the experiments of Doc-

tor Jenner, which were, in part, detailed in the Enquiry, that we can, by unsuitable food, soon call up a tuberculous disease in rabbits ; and it is equally well known, that a wet season and bad pasture will bring into existence the same disease, to a much greater extent, in sheep and other animals. It is, besides, ascertained, that the disease in both cases may be got rid of (provided it be not permitted to advance too far,) by a more wholesome diet, and judicious removal from the influence of the other pre-disposing causes.

The extent to which it is possible to excite the powers of the system, so as to promote the absorption of morbid growths, is, probably, much greater than is generally supposed. Several interesting cases illustrative of this position will be given in the sequel. Without, therefore, insisting much upon it at present, it shall be my object to impress upon the reader's mind the leading principle, to which our attention ought invariably to be directed in the management of this class of diseases. It is this : a disorganizing process is going forward, which produces certain consequences, according to the situation in which it occurs ; and our only

hope of restoring health consists in arresting that process, and in rousing into action those powers of the system, which may remove the changes that have taken place. It is manifest, that these objects can be best attained in the early period of such disorders. To that period, therefore, our attention ought to be directed with the most watchful care; for there is good reason to hope, that by such vigilance numbers, who would otherwise perish, may be preserved. Many of the signs which denote the disposition to tuberculous disease, in its most fatal and most frequent form, are recognised by every one. There are others that more experienced eyes may detect, which, when they are generally and duly considered, will suggest such measures of precaution as may often, perhaps, avert the impending evil.

Whoever has read the history of the progress of tubercles in the lungs, as given in the preceding pages, will easily comprehend the needfulness of watching the signs which are above alluded to. Unless this be done, we shall have still to lament the frequent occurrence of those cases, which have far advanced towards an irremediable condition, ere

any efficient or judicious means have been adopted to stay their progress. The very first cough of an individual who is about to fall into tubercular phthisis, little as it may have attracted his own notice, or the observation of his friends, is often a sure indication of advanced disorganization. That disorganization, in its primary condition, did not much disturb the system ; but it was laying the foundation of a disease which was to baffle all subsequent endeavours for the restoration of health.

The practical inference deducible from this statement, obviously is, that we should endeavour to effect the removal of the disorganization, while it is yet in a state to admit of its being accomplished. We are not without lights to guide us in this attempt ; nor are we altogether without means to gain our end ; and there can be little doubt that, as our knowledge of this branch of pathology advances, our sources of relief will be increased.

Since it appears that whatever enfeebles the frame, or deteriorates the constitution, pre-disposes to the diseases in question, how shall we avert this pre-disposition? The answer is apparent : we must do every thing in our power to in-

vigorate and fortify the tender frame ; to bring all its functions into a healthy state, and by all means to endeavour to keep them so. But, suppose that this cannot be effected; that the pre-disposition has already advanced to incipient disease ; that change of structure has actually commenced ; what in that case, is to be done ? We must first seek the absorption of that change of structure ; or, at all events, prevent its increase. Next, it may be asked, what agents can we command for accomplishing such desirable purposes ? Though not so numerous, nor so certain, as might be wished, there are, nevertheless, some of unquestionable efficacy ; and these we shall shortly consider.

Formerly, the most powerful deobstruents were believed to be mercury and alkaline preparations. The first-mentioned remedy is unquestionably useful in many forms of the diseases of the lymphatic system ; but, when the lungs are the seat of the disorder, its utility is more questionable. As it is to the last-mentioned variety of disease that these Illustrations chiefly apply, it may be proper to deliver the observations respecting treatment with a reference to the same complaint. When the

external parts of the body are attacked, or when the abdominal viscera are under the influence of the disorder, we can seldom be at a loss in foretelling what must be the consequence of its continuance. But, when it first develops itself in situations where we can neither see its progress, nor detect its existence by any well-marked or exclusive signs, there is not only greater need for caution, but greater difficulty in counteracting the evil.

Judging from the effects of some of the remedies that are most efficient in removing obstructions in the external parts of the frame, it is not unfair to conclude, that similar remedies may tend to cure similar diseases, when they take place in the internal parts. The due application of this principle, affords the only hope of effectually obviating the causes of pulmonary consumption. The difficulties which stand in our way in attempting to accomplish such an object, are doubtless great. But it is something gained, to be well aware of the point to which our endeavours ought to be directed. Knowing, therefore, as we do, that the bodies which give to pulmonary consumption its peculiar and fatal character, are, before they arrive at that state,

in a condition to be checked in their progress, and possibly to be altogether taken away from the constitution, we have the greater encouragement in diligently seeking for the means of doing so.

I reluctantly return to statements which have been delivered on former occasions; but, since our success must much depend upon the time of administering our remedies, I would again beg the reader to remember, that tubercles, as they advance in their progress, not only are themselves the causes of danger, but that this danger is also much augmented by the changes which they necessarily occasion in the surrounding parts. It is on that account peculiarly required of us to endeavour to meet the disease, before these irremediable disorganizations have taken place. When they are in their primary stage, and when the texture of the surrounding parts is little altered, there is reason to believe that much may be effected. Unluckily, we have seldom fair opportunities of attacking the disorder at this period; our exertions, for the most part, being required, not to meet an approaching enemy, but merely to mitigate, in some degree, the ravages which he commits in his course.

There must ever be, from the structure and functions of the organs of respiration, something peculiarly hazardous in every acute disease by which they may be attacked, as well as in every disorganization of a more chronic nature. But, after making due allowances for such peculiarities, it is perfectly consistent with the laws of the animal economy to conclude, that remedies, which may be influential in curing kindred diseases in other parts of the body, may not be powerless here. Experience justifies this conclusion. In a former chapter, the connexion between external diseases of the horse, and some of the internal disorganizations, were alluded to ; and it was affirmed, that the farcy bears the same relation to glanders, that scrofulous affections of the extremities, or outward parts, bear to pulmonary consumption in men. It was also mentioned, that the pulmonary affection in both instances, though it often succeeds the outward one, is very frequently primary. The object of repeating this remark, is merely to establish the affinity between the diseases, and to enforce and illustrate the practical conclusion which rests upon that connexion. If, therefore, we possess any

means of amending what is called a serofulous constitution, or of removing it, after it has shewn itself in that form, to which the word *scrofula* is particularly applied; we have sufficient grounds for believing, that what is efficient in regard to an external disorganization may be beneficial in counteracting an internal one: and, moreover, that if we can by any means succeed in changing the habit of a body, which is disposed to outward diseases of this kind, and bring it into a state of health and vigour; that we thereby take the best methods of counteracting similar tendencies in other parts of the frame.

I am quite aware that it may be supposed, I am dwelling with unnecessary earnestness upon a very simple question. It is, however, an interesting and important one; and, simple and apparent though it be, it cannot be denied, that it has very often escaped the attention of some of the best instructed and most ingenious physicians.

As far as my experience goes, there is no remedy which possesses such powers in promoting the absorption of morbid growths, as the hydriodate of potass. The reports of the influence of this re-

medy in curing bronchocele, as published by Doctor Coindet, of Geneva, first brought it to my notice. The nature of my own enquiries had led me anxiously to look for some agent of this kind; and having been fully convinced of the affinity between bronchocele and the diseases of which I had treated in my Enquiry, I considered it extremely probable, that a remedy which could remove the first-mentioned species of disorganization, might be very beneficial in the others. On this principle I acted; and the result of my trials has fully justified the anticipations which I had formed.

I will shortly illustrate this remark, by detailing a few cases.

The first I shall mention is one of *physconia hydatidosa*. It much resembled a case which I have described at page 95, of my Enquiry. The abdomen was as large as that of a woman in the last stage of pregnancy. The tumour had been more than once reduced in size, by the long continual use of mercury and liquor potassæ; but it never was effectually removed. More than once its bulk was very much diminished, by an event which establishes its original character, and jus-

tifies the name which I have assigned to it: I mean, the disruption of one or more of its cysts, and the discharge of the contents into the alimentary canal; such fluids as hydatids are known to contain* in various stages of their progress, having, after the events just described, been discharged both from the stomach and per anum.

This patient began the use of the hydriodate of potass in solution, on the 6th of October, 1821. She took at first eight drops twice a-day, and continued them very regularly till the 23d of March, 1822. By this time a marked effect had been produced on the size of the tumour; but in consequence of some unpleasant feelings about the stomach and head, the drops were discontinued, and not resumed till June the 22d.

From the use of this medicine, a very striking absorption of the diseased structure has taken place. Before she began it, the bulk was nearly as great as at any former period: now, it is not discernible by the eye; and it requires a pretty accurate examination by the touch to discover the remains of the substance, as she calls it, in the left iliac region.

* *Vide Enquiry, p. 94.*

I am fully entitled to ascribe these results to the remedy in question; for saving the use of laxatives, and the occasional application of leeches, none other were employed. A small tumour* has recently appeared in the left mamma. This is an occurrence which much confirms my views respecting the origin of this class of diseases, and is easily accounted for, if the principles which I have elsewhere endeavoured to unfold are correct.

The next case was a-kin to that just mentioned. But it occurred in an individual much further advanced in life. The disease had been of slow growth; but it was not much regarded till a few months ago, when it began to give great pain, and rendered the individual incapable of using almost any exertion. When I examined her, I found a tumour, about the size of a child's head, occupying chiefly the left side of the abdomen. It had a solid feel, and was very tender to the touch.

I ordered leeches and fomentations with hemlock, in order to relieve the pain and tenderness; and then directed an ointment, containing hydri-

* It is of importance to observe, that this tumour has, by the continued use of the remedy, been nearly absorbed.

date of potass, to be rubbed upon the swelling night and morning. At the same time Brandish's* caustic alkali was administered internally.

The rapidity with which the size of the tumour has been diminished, has quite surprised me. The remedies have not yet been used four weeks ; and I am informed by the gentleman in regular attendance, that it does not equal half its original bulk. The pain and tenderness are quite removed, and the patient can walk, and exert herself almost as well as ever.

Except in cases of bronchocele, when the iodine was employed, I have seen no absorption so marked as this. I do not know if any influence is to be ascribed to the caustic alkali. I am disposed to think that there is ; for it certainly is a deobstruent of very considerable power.

In another case of a very formidable aspect, the efficiency of the iodine was shewn in a surprising degree. A gentleman had a series of tumours,

* He was a surgeon at Alcester, in Warwickshire, and acquired a great reputation by the success of his treatment in scrofulous diseases. The preparation in question was invented by him, and an account of it will be found in a very sensible pamphlet which he published.

which reached from the angle of the jaw to the top of the shoulder; some of them were very large, being equal in size to a goose's egg; they extended also to the front of the neck. Various powerful medicines, such as the compound calomel pill, liquor potassæ, &c. &c. were used without any effect. The hydriodate of potass was administered internally twice a-day, in doses of ten drops. It was continued for several months; at the end of which time, the morbid growths were almost completely removed, all that remained being a very small substance, not larger than the half of a walnut.

The result of these cases gave me great encouragement to use it in others of a kindred nature; but the disorganizations were of a description which gave little hope that their removal could be effected; I mean pulmonary tubercles. The reader will be best able to judge of what there is some reason to hope may be accomplished, by attending to the following case. I have selected it, because the symptoms were very strongly marked; and the proofs of the existence of tubercles in the lungs were as complete as they possibly could be.

A young gentleman, of a delicate frame, had been long affected with frequent cough; but at first he did not expectorate at all. He lost flesh; his pulse increased in velocity; his respiration was frequently hurried; and his countenance and manner indicated most serious disease. He had been in this situation for many months; when, after a fit of coughing, more violent than usual, a small globular-shaped, but somewhat flocculent mass of tuberculous matter, partially tinged with blood, was discharged. This event fully confirmed my suspicions respecting the cause of the harsh dry cough, which had so long harassed him; and convinced me that tubercles existed in his lungs. Under circumstances of this kind, it is needless to say, that the most unfavourable prognostic was called for. I expected, of course, that in this, as in other similar cases, the patient would soon exhibit all the worst symptoms of pulmonary consumption. The expectoration of such matter as above described, having occurred a great many successive times, at considerable intervals, tended to strengthen my apprehensions. I dwell upon these particulars, because, without such proofs as they

disclose, no fair estimate could be formed of the value of the remedy on which I chiefly relied for the removal of the complaint. I consider it, therefore, as proved, that the patient in question had tubercles in the lungs; and that they were rapidly hastening to that stage when recovery becomes almost hopeless.

My plan of treatment was the following: I kept him in a regulated temperature; I stimulated the chest occasionally by blisters and tartar emetic, and confined him to a strictly vegetable diet. At the same time, anodynes were occasionally used, to abate the frequency of the cough. But knowing that all these means, unless the tubercles themselves could be got rid of, would be of little avail, I administered such remedies as appeared most likely to promote that object. I began with the use of Brandish's caustic alkali, in a little compound infusion of orange peel, twice a-day. After employing these remedies for some weeks, I resolved to give him the hydriodate of potass. He began with eight drops twice a-day; and continued it for three weeks without intermission. It was then left off for about a fortnight, and resumed; the quantity

having been increased to ten and twelve drops. The consequence of this treatment has been an almost complete removal of the cough ; an entire cessation of all expectoration ; a complete freedom of breathing ; a reduction of the pulse to its natural standard ; a healthy state of the stomach and bowels, and a decided augmentation of flesh and strength. The patient is able to take long-continued and active exercise on horseback, and has consequently been exposed to considerable alternations of temperature, without suffering inconvenience.

The time is not yet come to speak definitively of this case ; but, so far as it goes, it is perfectly satisfactory ; and affords as strong testimony as one case can give, that a most beneficial impression has been made upon the disease. Should things continue to go on favourably, I shall feel no hesitation in believing, that this was an example of tuberculous phthisis, arrested in its progress mainly, I believe, by the medicine, of the qualities of which we have been speaking.

I have ordered the remedy in a considerable number of other cases of pulmonary consumption.

The circumstances, however, in which the patients were placed, gave me little or no hope that any decided advantage could be gained from its employment. Extensive ulceration had already taken place, and all the most threatening symptoms of approaching dissolution existed. My trials, in these cases, only confirmed what was, *a priori*, to be expected; namely, that the period for affording effectual relief was past. But, on the other hand, they strengthened that conviction which I have endeavoured to impress upon the reader's mind, that there probably is a period in the most formidable of all the tuberculous diseases, when its course may be stayed, and the cause of subsequent evil eradicated from the system. Whether my expectations on this point will ever be realized to the full, I cannot say; but it is certain, that no just hopes of any great improvement in the treatment of this very fatal malady can be entertained, which do not rest upon the principle to which I have alluded.

I take this opportunity of recording another case which, in all respects, is applicable to the subject of which we are treating. The patient, a female,

came under my care about three months ago ; she was affected with almost all the symptoms which characterize tuberculated accretions of the peritonæum, in a considerably advanced stage. There was weight, tension, hardness, and in some places tenderness of the abdomen ; great oppression after taking food ; almost constant nausea, and great irregularity in the functions of the bowels. There was likewise great emaciation and languor ; a rapid, feeble pulse, and that peculiar anxious expression of the countenance, which I have elsewhere insisted upon as a strong indication of the internal disorganization mentioned above. In short, every symptom led me to believe, that that disease had actually begun to establish itself. My hopes of any essential relief, of course, were very small : but the facts already stated, clearly pointed to the sort of aid that it was necessary to attempt to procure for her.

Leeches were applied to the tender part of the abdomen ; and an ointment containing the hydriodate of potass was rubbed upon it. The action of the bowels was regulated by mild aperients ; and, latterly, the hydriodate of potass was also admi-

nistered internally. Two blisters were applied in the course of the treatment. The result of these remedies has been a restoration of the healthy feel of the abdomen; the swelling, tension, and hardness, having been altogether removed. The functions of the alimentary canal have become more natural; the pulse has diminished in frequency; the countenance has lost its expression of distress, and she has decidedly acquired flesh and strength.

Possibly some may say, that this was merely an example of what has been denominated chronic peritonitis, which was removed by bleeding and blistering. I think it of consequence to express my clear conviction, that it was not so; and that it belonged unquestionably to that family of diseases, to which tubercles give the character. For an explanation of what I mean by this statement, I must refer the reader to what I have said in my Enquiry, when treating of the difference between tuberculous and inflammatory affections of the serous membranes.

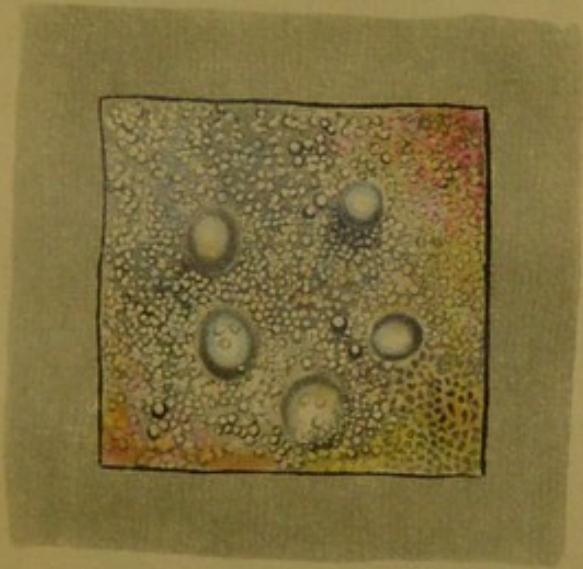
That the present case belonged to the former, I infer from the history of its progress. It commenced insidiously; advanced slowly; and had reduced her

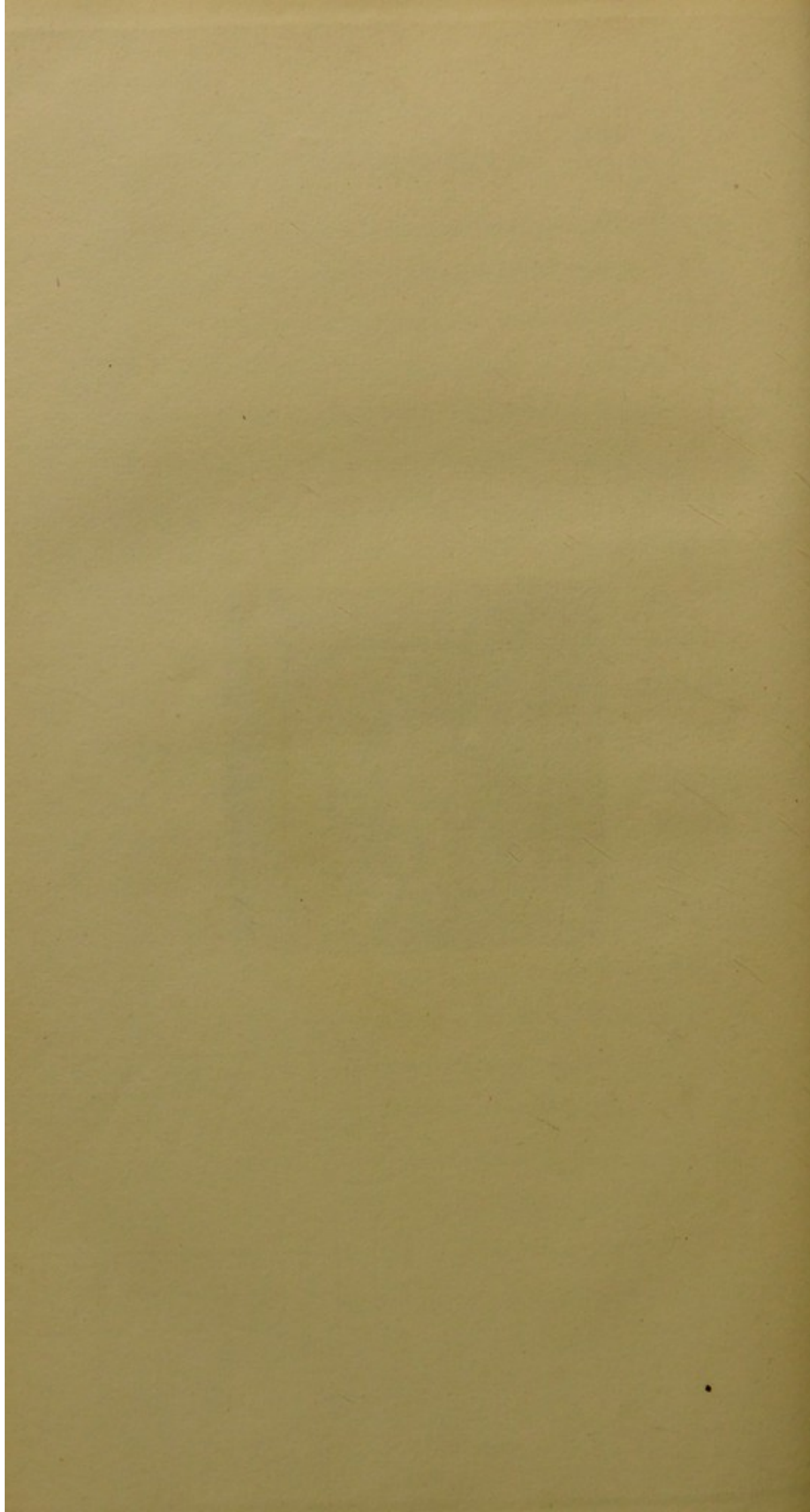
to the state which I have already described, in spite of the use of appropriate remedies, which had been judiciously employed before she came under my care. Symptoms may have been mitigated by the means employed; but the disease itself was not arrested; and it was in full progress to a worse condition, when I first saw her. This accords exactly with my experience in similar cases, as given in the Enquiry. And, if I except this single instance, I have seen no example of this form of disease on which any decidedly favourable impression has been made.

The facts detailed in this chapter touching the use of iodine, will be held, I doubt not, by my professional brethren, to confirm the reports of this medicine as given by Dr. Coindet. I agree with that gentleman, that it has a distinct and direct effect upon the lymphatic system; and that we now seem to be in possession of a remedy calculated to prevent and remove many of the most untractable and hitherto irremediable of human maladies. I have scarcely experienced any of the inconveniences which, he says, sometimes arise from its use; and I am rather inclined to think, that

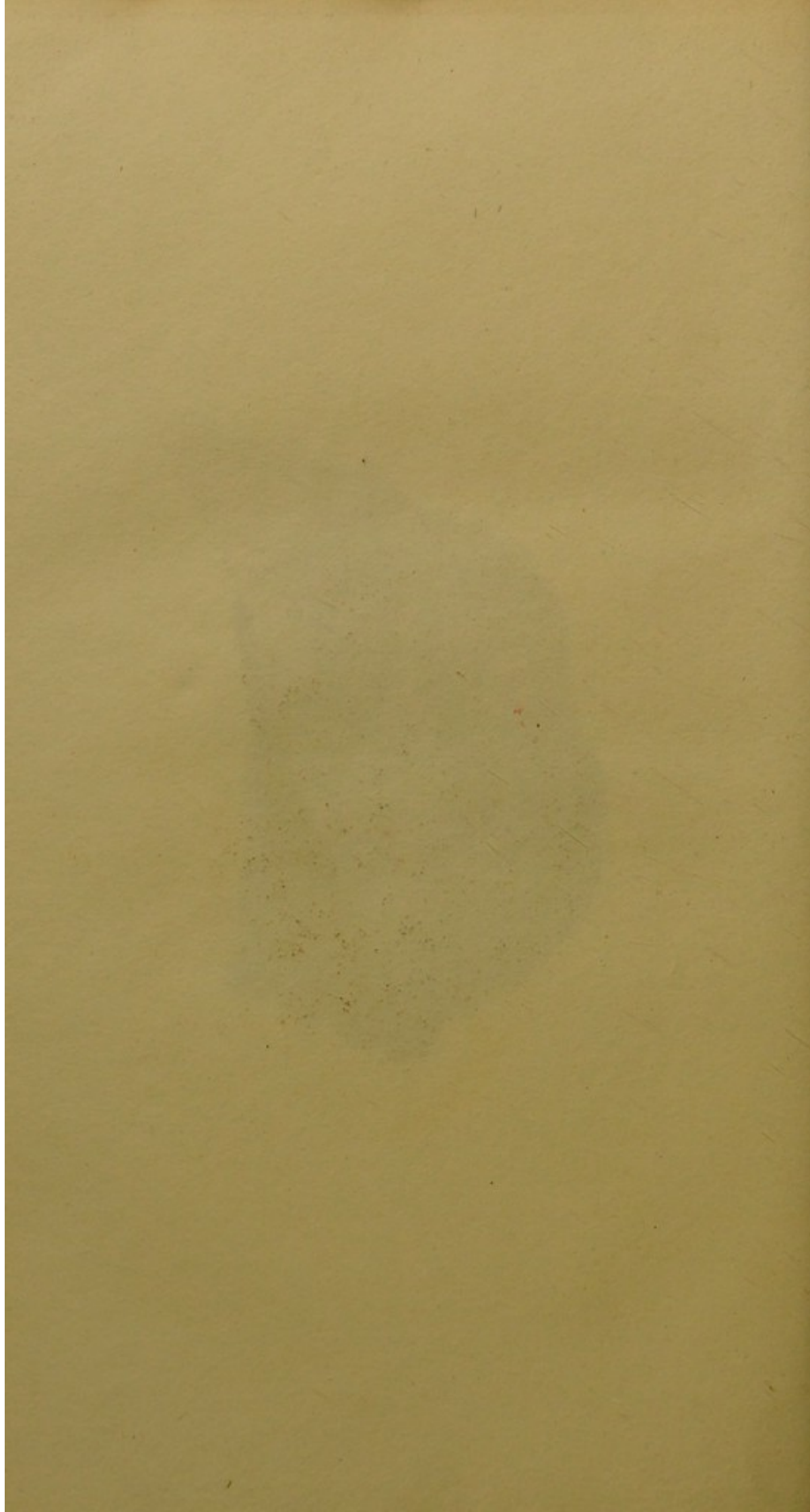
mere friction or inunction will not, in many cases, be successful, without also giving it internally.

Those who have done me the honour to attend to what I have said upon a former occasion, respecting the origin of tuberculous diseases, and their affinities with each other, cannot fail to discover how much the opinions which I then ventured to express, are illustrated and enforced by such facts as have now been given. The deductions have been established by their practical application; and the truths which have been thus obtained, have acquired consequence, not less from the satisfactory nature of the proofs by which they are sustained, than by the light which they afford, in enabling us to carry our investigations still farther into this branch of knowledge.

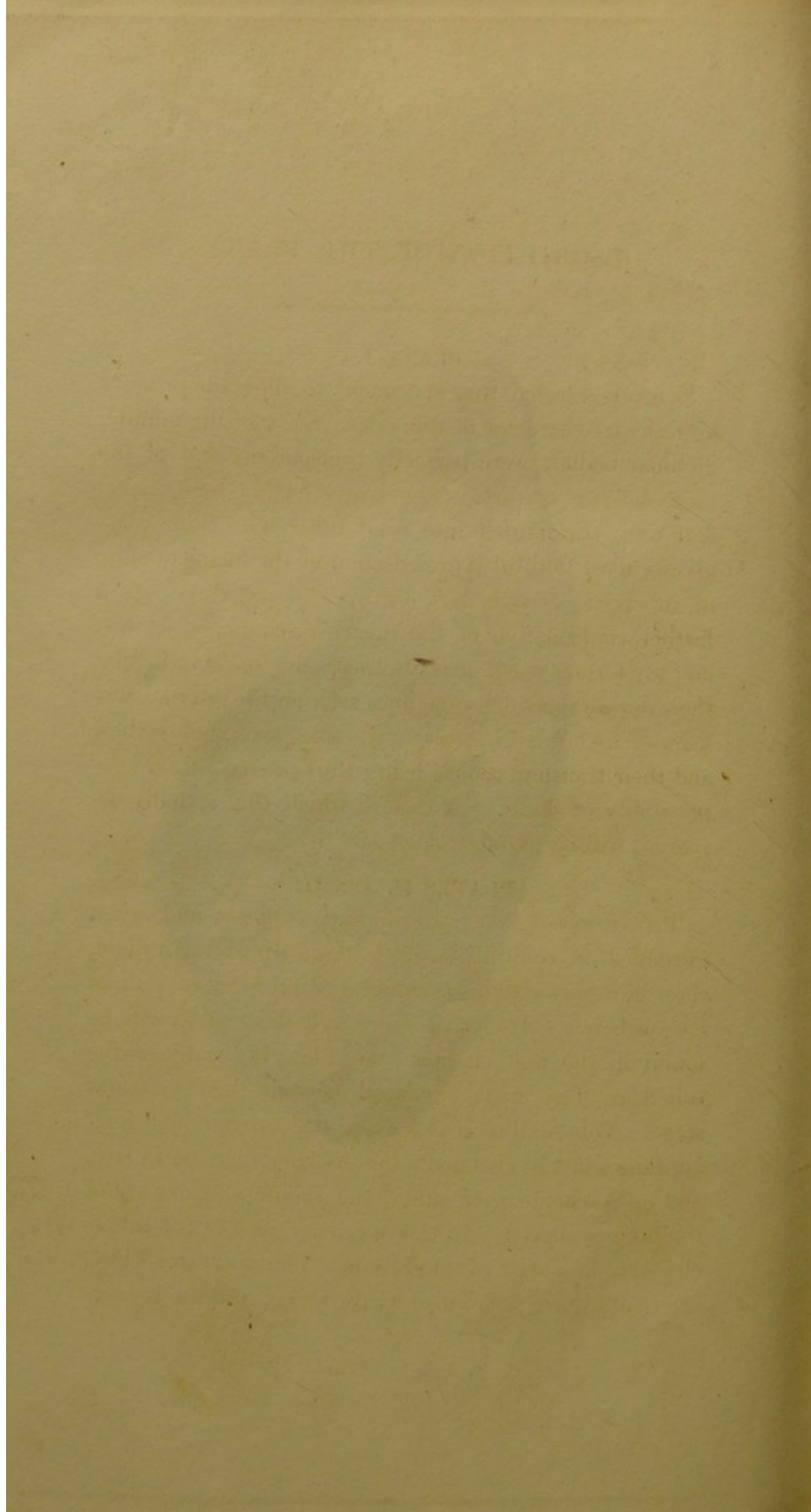












DESCRIPTION OF THE PLATES.

PLATE I.

I HAVE selected this specimen, to show the primary or vesicular character of tubercles. Some of the minute, globular bodies, were perfectly transparent, and of the most delicate texture. The larger had become opake, and were transmuted into solid tubercles. This plate gives a most faithful representation of the incipient state of tubercles, described in the case at page 22. As a farther confirmation of the doctrines contained in this, and my former work, it is also necessary to remark, that these appearances are sometimes seen on the internal surfaces of hydatids themselves; the origin of such bodies, and their transmutations, being thus proved beyond the possibility of doubt. A case in which this actually occurred, will be found at page 190.

PLATES II. AND III.

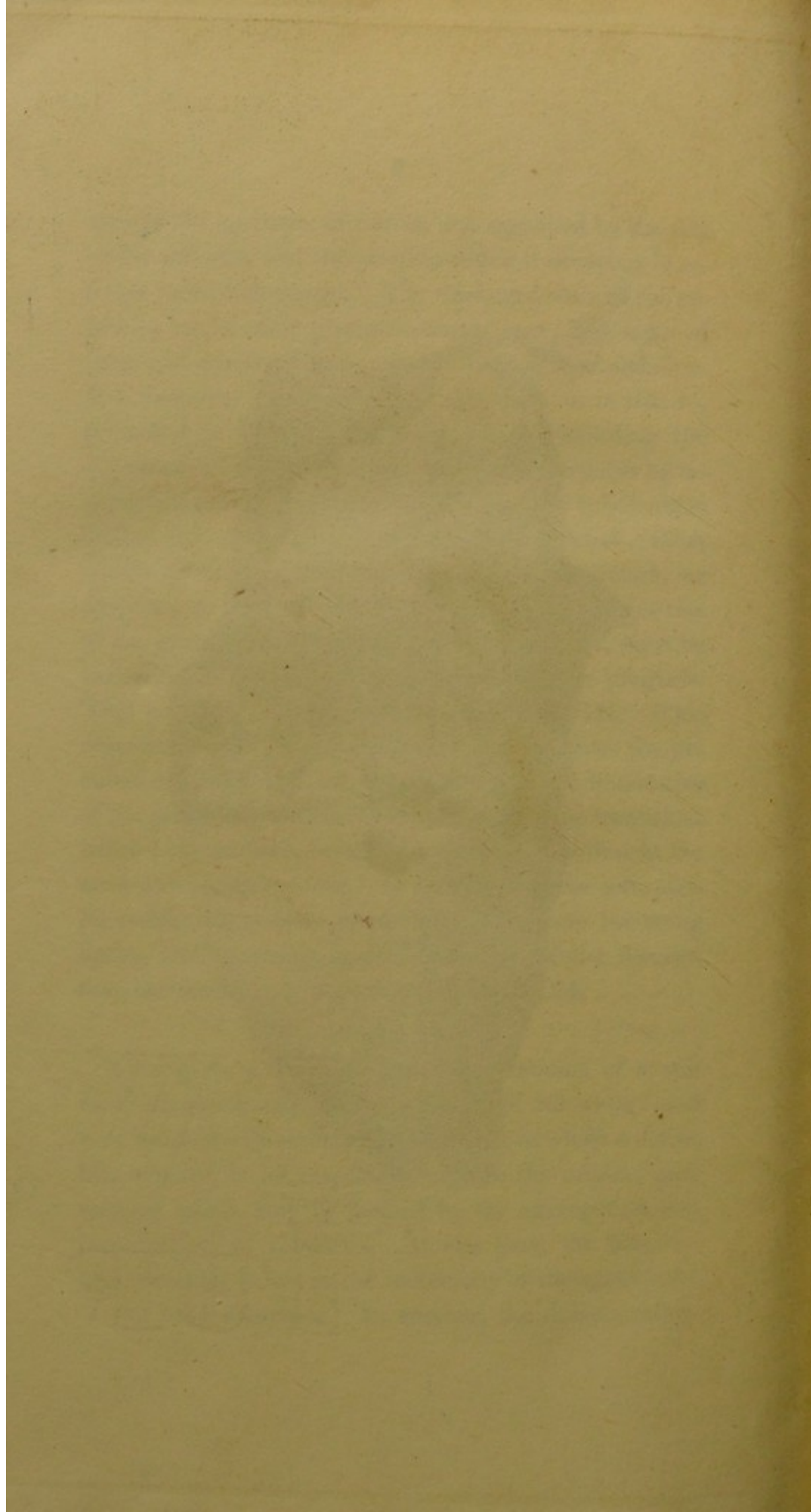
THESE were taken from the same subject; and, when examined in conjunction with that already described, show in a very striking manner the progress of pulmonary tubercle, of which a more full description will be found in the first chapter. In Plate II. many of the tubercles have little advanced beyond their vesicular stage. Where this is the case, the surrounding lung appears scarcely changed: but as they increase in size, and approximate each other, the colour and density of the lung is altered. In this specimen none of the tubercles were in a state of ulceration. The apertures which are visible, were occasioned by the knife; and the appear-

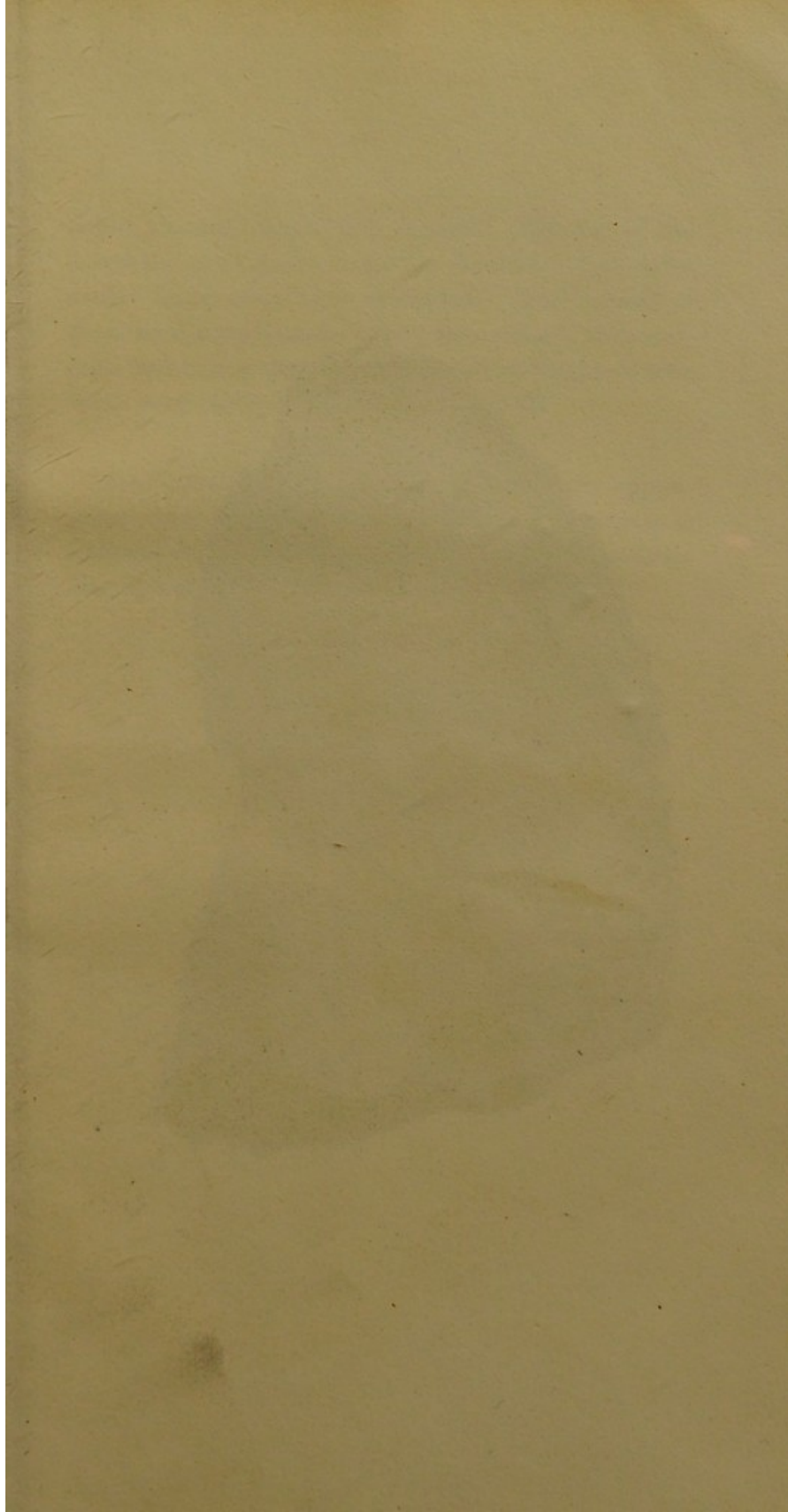
ance of the aperture, of course, was regulated by the size of the tubercle, and the position which it occupied in relation to the instrument. The thickened coats of the tubercles are in many places distinctly seen; but none of them had advanced to a complete state of consolidation. It is manifest, that tubercles in this state, or in that represented in Plate I. may exist without affording the symptoms of pulmonary consumption. There can be no expectoration of tuberculous matter, because tuberculous matter had not, strictly speaking, been formed. Such bodies existing in the lungs might occasion cough, or dyspnæa, or hæmoptysis: but the state which gives rise to the genuine character of tuberculous phthisis, must be looked for in a more advanced period of their progress. That period is, in part, represented in Plate III. This representation is valuable, in as far as it combines the peculiarities just described with those which are illustrative of the progressive condition of the disease; the transitions which I contend for, being thus rendered manifest in the most convincing manner. In one portion, the soft, elastic vesicles were seen: in another, they were becoming opaque, and increasing in size, &c. For a fuller description, the reader may turn to pages 23 and 24.

PLATE IV.

THIS gives a very accurate representation of a still more advanced stage of the progress of tubercles; and very satisfactorily elucidates the manner in which a dense, firm texture, in all respects foreign to the original pulmonary tissue, may be formed by the aggregation and consolidation of tubercles. At one part, the tuberculous character is lost in the uniformity of the appearance of the solid substance. In another, the distinct tuber-







entire character is perfectly manifest. The size of the
infusoria varies much; some are not larger than minute
beads; others are as large as almonds. The majority of
them were consolidated; others were not so; their con-
sistency had been experimentally found to be different; some
were easily broken, others not so.

These plants exhibit a singular character in the manner
in which they are attached to the surface of the water, in
different states of consolidation, and in different degrees
of description, which is not to be found in any other



