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ON THE PATHOLOGY OF RHEUMATISM.

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The statement with which Dr. Copland prefaces his very able and comprehensive article on the subject in the *Dictionary of Medicine*, that "although rheumatism is, owing to geographical and social circumstances, one of the most prevalent diseases in the British Isles, yet it is one respecting the nature and treatment of which exists the greatest diversity of opinion and the least amount of undisputed knowledge," may be regarded as no exaggerated description of the state of medical science with regard to this frequent and important affection. Whether this state of obscurity be owing to the natural intricacy of the subject, and of the many collateral questions which must be answered before much hope can be entertained of solving the original problem, or to the various theories which have at different times, and in accordance with the dominant doctrines of the day, been invoked with the intention of throwing light upon it, but have served rather as *ignes fatui* to lead astray from the true path of inductive generalization, it is difficult to say: certain it is that to any one who peruses even the standard monographs upon the subject, putting out of question the articles upon it in systematic works, and the numberless brochures which are to be found upon the shelves of any large library, its pathology and the principles of its treatment must appear to be in a chaotic and most unsatisfactory condition. Perhaps there is no single disease with which the practitioner is brought so frequently in contact, and yet in whose alleviation he acts more empirically or with greater doubt than in acute rheumatism; nor is there any better indication of the distance that must be traversed before we can hope to arrive at the goal of its true and rational pathology, than an enumeration of the various medicaments which have successively been suggested for its cure, or which are still employed, though often the

inspiration of different and conflicting theories. Such a condition of the therapeia of any disease will probably depend upon one of two causes: either the disease itself must be of a Protean character and must require much careful investigation to establish the relationship of its varieties to one another, and to adapt to them a fitting curative treatment; or the remedies which have been used with alleged, though often with most questionable success, have not been directed so much against the disease itself as a whole, but rather to the subdual of its symptomatic and subsidiary phænomena, which upon this supposition must be of a numerous and complicated nature. In the case of rheumatism, which has been, as it were, the battle-field of a host of specifics and nostrums, the conviction forces itself upon the observant mind that the amount of relief which does in some cases unquestionably attend their employment, affects for the most part only the minor concomitants of the affection, and has little or no influence upon the progress or duration of the affection itself. The greatest triumph of science is not to find out that a certain cause produces a certain effect, but to discover why it does so: and although for philanthropic reasons the medical practitioner may be only too well satisfied that his practice, though empirical, is attended with a certain proportion of success, yet it will be ever his highest aim to investigate the path by which that success has been attained; and thus to eliminate, by a cultivation of the conditions under which alone it is attainable, all the elements of doubt and obscurity which perplex the course of the man who is influenced by the blind dictates of experience only. But to establish the connexion between the means thus employed and the results to be obtained, it is necessary that he have an exact and accurate knowledge of all the phænomena upon which he is to base his treatment, that he do not overlook any important ones, and above all that he know how to subordinate the less to the more essential, uninfluenced by the sometimes more apparent severity of the former, and the less obtrusive significance of the latter. An analysis of the nature of an attack of acute rheumatism, of its accompaniments and consequences, will show that it is upon this latter rock that the founders of the modern pathology of that disease have struck; that they have given a primary place to what is only a secondary phænomenon, and that they have ignored the most constant and essential feature of the disease. So long as investigations are pushed in this direction, and this most imperfect, and to a certain degree false, view of the affection is employed as a basis for working out its pathology and for bringing it into correlation with other affections, so long must all hope of explaining the import of the many phases which it exhibits, and above all of establishing a rational prophylaxis against its attacks, be abandoned. It is with the view of pointing out what the author considers to be the fundamental feature of the disease, and calling attention to some of its collateral peculiarities

which seem to be generally overlooked, that the present paper has been written. And though he cannot but feel that there is no little temerity in thus stepping forward where so many better men have stood before him but to fail, yet his consolation is, that although this individual attempt may not realize that success which in the cause of science he would desire, it may lead others to proceed in the path thus indicated until their investigations are crowned with happier results.

It will be well, before proceeding on an independent line of research, to examine briefly one or two of the more plausible theories which have been propounded to explain the phænomena of rheumatism, and which have seemed to meet with most support from clinical experience or scientific research. The commonest, and at the same time one of the oldest, refers the disease to the combined influence of cold and moisture upon the body, without indicating any constitutional condition as a pre-requisite to its development.* That cold alone is not sufficient to produce the disease is shown by the fact, that the results which that agent produces on a previously healthy body are of a totally different kind to those of rheumatism. We hear hardly anything of the disease—at any rate, in its acute form—amongst sailors, who have been exposed in whaling voyages and exploring expeditions to the severest cold of the Arctic regions. And even in the marine, of the royal and merchant services, as a class, we find much less of it than might have been expected, considering the exposed nature of its occupation to all the vicissitudes of temperature and moisture. In fact, it might almost be affirmed that *habitual* exposure to a low temperature rather counter-indicates than predisposes to acute rheumatism—the influence of cold, whenever the attack can be traced to its effects *alone*, being most powerful on those who, by their habits and occupation, are generally most protected from it. Nor does the additional influence of moisture much facilitate the explanation; for, of the thousands who are daily exposed to both cold and wet in these northern regions, but a very small proportion incurs the disease; whilst a very large number of those who do, have not been inordinately exposed to either.† Moreover, if cold were even its most frequent exciting cause, we ought to find the disease predominating in the coldest months of the year, and the coldest regions of the earth: whereas, M. Chomel states, that the months of April and May are those in which the largest number of rheumatic patients are admitted into the Parisian hospitals—a statement which will probably be borne out by English experience. The statistics of the army also, as quoted by Dr. Fuller, prove that for every 1000 men, fifty-seven cases of rheumatism

* Watson: vol. ii, p. 676; third edition. Bouilland: *Recherches nouvelles sur le Rhumatisme*; pp. 88, 91.

† For some good remarks on the questionable nature of the connexion between cold and many inflammatory diseases, vide *Jones and Sieveking's Path. Anat.*; p. 114: also *Henle's Handbuch der rationellen Pathologie*; b. ii, s. 234.

occur in the mild and genial climate of the Cape of Good Hope, fifty in England, and only thirty in the cold and variable climate of Nova Scotia. And, for my own part, I have seen more and severer cases of acute rheumatism in six weeks in St. Mary's Hospital, situated as it is in one of the warmest and most sheltered of the London districts, than I have witnessed in as many months in the Royal Infirmary of Edinburgh, with nearly three times the number of beds. It cannot be doubted, however, that many cases of rheumatism do appear to recognize a chill as their principal exciting cause; but it is probable that, if many of those in which this element, when discovered, is considered sufficient to account for the origin of the attack were more carefully examined, it would be found that a more potent influence still has been at work—that of fatigue; and any one who will carefully analyse a large number of cases of rheumatism will, I think, unfailingly come to the conclusion, that corporeal exhaustion is by far the most frequent exciting cause of the disease.

Another theory, which carries the etiology of rheumatism a step further back, by indicating the existence of a previous morbid condition of the blood, and has met with a ready reception on account of its support by men of deservedly great authority in the profession, refers the disease to a suppression of the elements of the cutaneous excretion—more especially of lactic acid, which, thus accumulating in the system, gives rise to the various phænomena of the affection. Unfortunately, there are two or three weak points in this otherwise very promising theory, which owes its existence, I believe, primarily to Dr. Prout, but has been subsequently adopted by Drs. Todd, Williams, Fuller, and most recent writers: 1stly. It has not been shown that any excess of lactic acid has ever been detected in the blood of rheumatic patients: indeed, the very question of the possibility of its detection at all in that fluid is so debatable, that it must be considered at present in too problematical a condition to render it a trustworthy basis upon which to erect any hypothesis.* On this point Dr. Garrod—a certainly very competent authority—says: "With respect to the presence of lactic acid in rheumatic blood, although we by no means wish to deny its occurrence, yet, we should hesitate to receive it as a fact until further evidence is produced."† 2ndly. Even if it were present in any excess, it is somewhat difficult to see any connexion between its presence and the phænomena of the disease. Why lactic acid in the blood should give rise to exudations in and about the different fibrous structures of the body does not seem at

* "The same may be said of the idea propounded in recent times by Williams and Fuller, which refers the cause of rheumatism to an extreme production of lactic acid in the organism. No one has either demonstrated this excess (the few cases in which the blood has been found to be acid prove nothing on this point), nor has it been experimentally shown, that the accumulation of lactic acid in men or animals produces such phænomena as we observe in acute rheumatism."—Vogel in *Virchow's Handbuch der speciellen Pathologie und Therapie;* b. i, s. 482.

† *British and Foreign Medico-Chirurgical Review;* 1854; p. 384.

all evident, nor has any one attempted, experimentally, to show that it can do so.* Were these tissues the natural emunctories of the acid, its excess might be supposed, under certain circumstances, to produce a hyperæmic and inflammatory condition of them; but the fact that its elimination takes place by the kidneys, alimentary canal, and skin, and that its principal seat of formation in the economy is amongst the muscular tissues, renders this hypothesis most gratuitous and groundless. 3rdly. There is an excess of other acids in the excretions, and, therefore, possibly in the blood, *e. g.*, uric, acetic, oxalic, &c., of any one of which the same effects might have been as reasonably predicated as have been assumed of lactic acid. 4thly. It admits of much doubt whether the disease is ever preceded by a greater suppression of the cutaneous excretion than the prodromata of all inflammatory diseases bring with them, as is asserted by the advocates of this hypothesis, or whether "rheumatic patients usually labour under a harsh and inactive condition of the skin during the intervals between their attacks," as they have been said to do. It is certain that in the chronic form of the disease this is far from being the case; and Dr. Graves particularly points out, in his *Clinical Lectures*, the constant and copious diaphoresis which often attends the subjects of chronic rheumatism. 5thly. The extreme tendency to sweating which occurs during an acute attack of the disease, and is cited as the effort of nature to get rid of the excess of lactic acid, may be explained in a much more satisfactory manner, as will be hereafter shown. Such are some of the principal objections which may be urged against the present popular theory of rheumatism, and which, however imperfect and unsatisfactory it may be, has at least this merit, that it indicates a tendency to the investigation of the chemical and physiological features of the disease, as something which must be studied in addition to the strictly clinical ones—a process which, if pursued but a step further than it has yet been carried, will lead to a correct appreciation of its pathology. And this theory is also a good illustration of the tendency of modern medicine in general towards a rational humoralism; to refer to a disordered state of the blood either in the quantity or quality of one or more of its constituents, diseases which had been previously regarded and treated only as so many local irregularities.

Now, before entering into a discussion upon the nature of rheumatism, the first question to determine is, In what does a typical acute attack of the disease consist, and what are its most essential features? They may be summed up as follows: an individual, previously perhaps in *apparent* good health, is seized with symptoms of pyrexia, generally after severe

* Dr. Richardson's experiments, though ingenious, cannot be held to have established this point; and any conclusions from them are worthless until it can be shown that other organic acids do *not* produce the same result.

fatigue, exposure to cold, mental emotions,* or some other depressing agency. The general disturbance increases, and at last culminates in a local affection of the larger joints, of the heart, or more rarely of some other internal organ; the local affection taking on the form of heat, pain, and swelling of the joints, or of effusion into the pericardium with or without valvular affection of the heart. The joint swelling and the pericardiac effusion consist of an exudation of a more or less fibrinous nature, deposited in the former case amongst the tissues surrounding the articulation, or in the synovial capsule itself.† This goes through the usual phases of exudations in general, its elements either breaking down after a time and becoming resorbed into the circulating current, or taking on a low kind of vitality and assimilating themselves to the tissues with which they are contiguous. As the process of resorption proceeds, the excretions are greatly augmented, evidently with the partial object, at least, of carrying out from the system the results of this local disintegrating process, probably also of a more general disintegrating process which is going on throughout the whole vascular system, concerning which more will be said hereafter. The perspiration is acid and copious, the urine is loaded with a dense sediment, and a critical diarrhœa is by no means uncommon. With the subsidence of the local effusion, and the general pyrexia, the disease disappears, leaving behind it nothing but a state of great prostration of the vital powers and anæmia, together with a certain amount of stiffness in the joints, dependent in some cases upon mere disuse, in others upon the presence of organized new growths —leaving behind it also, not infrequently, disease of the heart resulting from the same condition. The patient now enters upon the convalescent stage, but is generally long before he recovers his natural health and strength; and he is extremely liable to relapses, especially from bodily exhaustion, or too early a return to an animalized form of diet. The most noteworthy feature in his subsequent history is, that his future liability to the disease is for the most part vastly increased by the first attack; in fact, the liability increases in a direct ratio with the frequency, and to a certain extent with the protracted duration of the attacks—a sure mark of the diathetic character of the disease. These are the chief points which reveal themselves to the clinical observer; the researches of the chemist, however, add another and most important item to our knowledge. It is a fact, most thoroughly established by the researches of Andral, Becquerel, Simon, and other physiological chemists, that the fibrine in the blood of patients suffering from an

* M. Gubler gives three cases of rheumatic cerebral affection, in two of which it had been preceded by powerful depressing moral agencies.—*Archives Gén. de Méd.*; Mars, 1857.

M. Vigla also refers to the predisposing influence of moral causes in developing cerebral rheumatism.

† For descriptions of the state of the joints exhibited in several autopsies of acute rheumatism, see Cossy: *Archiv. Gén. de Méd.*; vol. xxxiii, p. 286.

attack of acute rheumatism, exhibits a marked elevation above the normal ratio, higher as a rule than in any other phlegmasial disease—even pneumonia rarely attaining so high a figure. This hyperinotic condition of the blood exists from the very first onset of the affection, and, according to Dr. Parkes, before any local symptom makes its appearance;* and it bears a definite relation to the general inflammatory disorder present in the first stages, as well as to the amount of excreta in the later ones. It is also accompanied by a most marked diminution in the number of blood globules and in the proportion of albumen. Taking the normal ratio of fibrine in healthy blood as about four parts in 1000, Andral has found that of acute rheumatism to range as high as ten, whilst other observers even give thirteen as the maximum: that of the worst cases of pneumonia rarely exceeding nine parts in 1000. So of the blood globules: taking 135 as their normal standard, they fall in phlegmasia generally to a mean of 123, in acute rheumatism to 118, and in pneumonia occasionally as low as 113 parts in 1000.† This altered relation of the fibrine and blood globules is the most important blood phænomenon in acute rheumatism; others have been recorded, but they are neither of sufficient importance, nor sufficiently well authenticated, to render a notice of them necessary here.

Now, on taking a broad view of these concomitants of the attack, what are those which would appear to be the most characteristic of the disease—to be the most typical, either by their constancy or their physiological importance—to be those from a study of which, in comparison with similar features in other diseases, fruitful results may be most rationally expected?

For any one who carefully considers this question, I think that there can be but one answer, viz., that the most important features of the disease are the exudation and the hyperinosis, most important because most constant, and because they are of weighty pathological import, as indicating considerable local and constitutional disturbance. The pain, heat, and swelling of the affected joints are merely local symptoms of the exudation, and depend for their intensity entirely upon its amount. The increased excretions, especially the perspiration upon which so much stress has been laid, are entirely of secondary importance, and are physiologically referable either to the breaking down of the exudation and the elimination of its elements in a soluble state from the system, or to a metamorphosis of the abnormal amount of fibrinous matter in the blood, lactic and uric acids and urea being only different stages in the transformation of the fibrine group of compounds from their place of highly vital tissue-elements to the inassimilable and inorganic condition which is necessary for their excretion when no longer subservient to the

* Gulstonian Lectures on Pyrexia.—*Med. Times and Gazette*; 1855; lect. ii.

† Becquerel and Rodier: *Pathological Chemistry*; Speer's translation, p. 99.

uses of the organism. It may, however, be alleged that the febrile symptoms are more important in a pathological point of view than the exudation or the hyperinosis, as they indicate a general state of constitutional disturbance, and are always proportionate to the severity of the disease. This idea derives some small support from the term rheumatic fever, which is so often used as a synonym for acute rheumatism, and from a corresponding notion which has grown out of it, and has led some nosologists to place the disease in the list of specific fevers, and has entirely lost sight of the local affection. Now to class rheumatism with the special exanthematic fevers, and to indicate thereby that they have anything more in common with one another than increased bodily temperature, rapid pulse, and altered relation of the nutritive and destructive powers of the system, is simply a confusion of ideas founded upon a misconception of terms. The facts, that the latter are for the most part epidemic and contagious, the former never; that the latter by one attack give a greater or less immunity from succeeding ones, whereas the case is just reversed in rheumatism; that this latter affection is invariably accompanied by a hyperinotic condition of the blood, whilst in the special fevers the fibrine is below its normal standard; and that the local affection in rheumatism is nearly always present, and comes on in the very commencement of the disease, while it rarely appears till late in the course of the exanthemata, and is often enough absent altogether, are, independently of other weighty considerations, distinctions of sufficient importance to sever completely the one from the other. The importance, however, of the febrile symptoms of rheumatism cannot be over-estimated, either in a pathological or a therapeutic point of view; but then it must be remembered that they are only *symptoms* after all—the outward manifestations of an internal derangement of the economy, and not the cause of the derangement itself. To address ourselves to them, therefore, in the hope of finding a solution of our pathological difficulties, would be about as reasonable as to seek for the cause of tubercular phthisis in the racking cough, the profuse night sweats, or the intense diarrhœa which characterize that affection.

Having thus endeavoured to establish the prior importance of the hyperinosis and the exudation over all the other phænomena of rheumatism, the question which now presents itself for consideration is, What is the relation which these two features bear to one another? Is the hyperinosis merely an effect of the reaction of the local disease upon the system at large, as is generally believed, or is it the primary source of the exudation, the causative agent of the latter, without which it could never exist? I believe this last to be unquestionably the true statement of the case, and shall endeavour to bring satisfactory evidence to show that it is so. Considered simply in a teleological point of view, the former supposition throws no light whatever upon either the economical

object of the hyperinosis, or upon the mode of its connexion with the local affection; whereas, on the latter hypothesis, it is highly probable, *à priori*, that an excess of fibrinous plasma rapidly accumulating in the blood to an amount which is incompatible with perfect health, should be attracted, as it were, out of the vascular system, in the shape of an exudation to those tissues for which it has a physiological affinity—such a process, in fact, being no more than a mode of excretion. In urging this consideration it must not be forgotten, as is well shown by Andral in his researches on the blood of patients who have suffered from burns, that *traumatic* lesions do most undoubtedly *give rise* to a hyperinosis, apparently as a result of their reflex upon the constitution at large, or upon the vitality of the blood in particular; but we are now dealing with the hyperinosis of *idiopathic* lesions which are clearly not amenable to the explanation that may be given in the former case: for though it is only reasonable that the economy should, on the reception of injury to any portion of it, prepare immediately an unusually large stock of that material which is specially devoted to its repair, to apply such an explanation to idiopathic lesions, and say that nature first inflicts injuries on herself and then makes preparations for repairing them, is surely most gratuitous and unnecessary. The most powerful argument, however, in favour of the sequential relationship of the exudation to the hyperinosis in acute rheumatism is derived from a study of the phænomena of the disease, and from a tentative application of the hypothesis to them with a view of ascertaining how far the one is a correct explanation of the other. Thus: a man is seized with the usual prodromata of what turns out to be afterwards acute rheumatism. At this period of the disease, on the hypothesis above suggested, the fibrine of the blood becomes rapidly augmented, from some cause or other to be hereafter discussed, and from being thus elevated above what should be its normal proportion in the blood, its vitality as a blood constituent is lowered, and it exhibits a greater tendency than is natural to it to leave the circulating current and pass into the sphere of the local attraction of the tissues with which it is homologous. At this juncture let us pause to inquire if we have any data for determining in what part of the body this exudation is most likely to occur. In this inquiry we shall be much assisted by bearing in mind that great law of nutrition, in accordance with which each tissue attracts from the blood, by a species of elective affinity, the elements of that fluid out of which it is built up; the muscles attracting the elements of muscular tissue, the different fibrous structures their constituents, and so on throughout the whole economy. But if each tissue attracts its elements from the blood, it is but reasonable to infer that there is a correlative affinity on the part of the blood elements each for its own tissue, so that the attraction is mutual, and may be represented as a constant tendency to union between the two, the mersion of

the mere tendency into actual union being brought about by local or particular causes—on the part of the tissues by their constant wear and decay, and consequent necessity for repair; on the part of the blood by the fluctuating preponderance at different times of one or more of its constituents: hence we have in the body, as in the great world of physics, a constant attraction of molecular particles for given centres, and at the same a certain bond of union between the particles themselves which can only be destroyed by a preponderance over it of the centrical attraction. Now if we have an excess of the fibrinous element in the blood at the onset of an attack of acute rheumatism, as is supposed, the *direction* of its localization when passing out of the vascular system will be evidently determined in favour of those tissues with which it has the closest homological affinities—affinities which, in most cases, as is shown by the results, lie with certain forms of fibrous tissue. Moreover, its local deposit will be influenced by the physical or physiological relations of each fibrous tissue; by the comparative proximity which it may exhibit to the special type of the fibrine exuding (adopting the supposition of Lehmann, that there are several allotropic varieties of fibrine, and consequently of fibrous tissue); by any special peculiarity which might confer upon one or more of them a claim, as it were, to the attraction of the fibrine when on the point of exuding; and by the local stasis of the circulating fluid, from any cause whatever, in or about any one of them.

Firstly: *with regard to the mechanical relations of the part.* We should expect a large exudation to occur most readily into those structures which exhibit the greatest facilities for its accommodation, *e. g.*, synovial and serous sacs, intermuscular and intercellular spaces, &c. Secondly: *the especial type of the fibrinous plasma.* In the acute form of the disease it would seem to assimilate itself to the fibrous tissues of the joints and heart; the fibrous tissue of the lung, as in the rheumatic form of pneumonia which M. Trousseau asserts that he has occasionally witnessed; or the subcutaneous areolar tissue, as in the rheumatic erythema nodosum described by Dr. Begbie, and MM. Hardi and Behier. In the chronic phase of the dyscrasia it directs its attacks upon the fibrous investments of the muscular and neural tissues, causing muscular rheumatism, thickened fasciæ, neuralgic pains, &c.; and occasionally upon the fibrous matrix of the liver, kidney, or other organs; of which I shall have more to say bye and bye. Thirdly: an important element amongst the determining causes would be either any natural abnormality of a fibrous tissue, laying it open to the attack of morbid influences—just as morbid growths, or scars, are often the first to feel the effects of prejudicial influences acting on the economy in general—or its temporary weakness from some accidental cause: in illustration of which latter circumstance we have the fact that one of the most frequent, probably by far the most frequent, of

the determining causes of a rheumatic attack of any particular set of joints is over fatigue of those joints.* For example, the occurrence of rheumatism in the knee and ankle joints after a long walk is sufficiently notorious; as it is also the tendency of the affection to the upper extremities of those who use them much, *e. g.*, blacksmiths, &c. The occasional tendency of the rheumatic dyscrasia to vent itself in an attack upon the knee joints of domestic servants after kneeling for some time to wash stairs, &c., which I have more than once noted, is an illustration of the same circumstance; as also is probably the tendency of patients with a nervous irritable heart to take on cardiac rheumatism. The affection of particular sets of muscles, as those of the back after violent exercise of them in rowing, cricketing, &c., is attributable to the same cause; as also may be the more problematical forms of rheumatism of the gravid uterus, of the pharynx, and of some other organs, described by French authors. The influence of mental excitement as a determining cause of rheumatism has been before alluded to, but much remains to be investigated on this branch of the inquiry. So also we may explain the effects of draughts of cold air on a particular joint giving rise to a subsequent attack of rheumatism in that joint, as is noticed by Mr. Paget: "The depressed nutrition of the joint makes it more liable than any other part to be the seat of inflammation excited by the diseased blood."† Thus there will be four sets of causes, at least, which will determine the point at which the exudation may localize itself, each complex in itself, and each modifying and modified by the others.

It is unnecessary to trace the course of the affection in one of its acute attacks any further, as its subsequent stages have been already briefly alluded to, and are perfectly similar to those of other inflammations. There is one phænomenon, however, attending its resolution, which deserves a passing notice, and that is the increase of the urinary and cutaneous excretions. In the former, urea and uric acid exist in large excess, whilst free acetic, phosphoric, lactic, and oxalic acids have been detected by Vanquelin, Henry, Dr. Bence Jones, and others. The most predominant constituent of the perspiration is lactic acid, but uric (Stark) and acetic (Prout) acids have also been observed in it. Most, if not all, of these substances are products of the degradation of fibrinous matter: the relations of urea and uric acid to highly nitrogenized matters, as exhibited by the experiments of Lehmann—by the recent manufacture of urea by artificially oxydizing albuminous substances by M. Béchamp‡—and by the general excess of these excreta in hyperinotic states of the blood, combined with that of lactic acid, to the muscular juice as determined by the researches of Liebig, amply corroborate this statement as

* Williams: *Principles of Medicine;* second edit., p. 151. This influence of fatigue is also noticed by Dr. Fuller, MM. Hardi and Behier, Gubler, and other authors.

† *Surgical Pathology;* vol. i, p. 442.

‡ *Journal de Chimie et Pharmacie;* Janvier, 1857.

far as these three bodies are concerned—the others, from the smallness of their amount, may be put out of consideration. The preponderance of either urea, uric, or lactic acid in the excretions may depend upon peculiarities of the fibrine itself and its tendency to be metamorphosed into one of these substances rather than into another, or to pass off by one excreting organ rather than another; but it is probably also referable in part to the vital power of the constitution and its ability to oxydize the fibrinous matter to one or other of these stages in its metamorphosis: for the atomic composition of these three substances and their chemical history gives ground for conjecturing that they are only successive grades in the resolution of highly complex albuminoid and fibrinoid bodies into much simpler forms; and it may be assumed, that the power of the system to resolve the whole of the hyperinotic matter in the blood into urea, or its inability to proceed further in respect to a portion of it than uric and lactic acids, depends upon certain vital peculiarities upon which we can do little else than speculate at present. A careful examination of the ratio of the urinary and perspiratory excretions to one another, and of their constituents, in a series of rheumatic cases, would do much to advance our knowledge on this point, but I have been unable to find any record of such an attempt.*

There are two rather singular facts connected with the pathology of rheumatism which the present seems a fitting place for noticing. The first is, I believe, pretty generally recognized as a clinical fact, though it is seldom or never referred to in books: I mean the great rarity of the combination of rheumatism and phthisis in the same individual. My own experience only supplies me with two instances of this concurrence; and in both of these the rheumatic affection was of a badly defined character; and, judging from that of the oral and written authorities whom I have been able to consult upon the subject, the coincidence is far from common. The only allusion which I can find to it is in Professor Bennett's *Treatise on the Oleum Jecoris Asselli*, where he quotes Brefeld, in illustration of the strong analogy which exists between the gouty, scrofulous, and rheumatic diatheses. That author points out, for instance, that gouty patients frequently produce scrofulous children; and that individuals who in their youth were scrofulous—and especially rachitic—exhibit at later periods of life great tendency to rheumatism. "It has been often observed," he says, "in families of the lower orders, that while the children are scrofulous, the parents suffer from obstinate rheumatism."† And this brings me to the consideration of the second fact, viz., the hereditary connexion of rheumatism and phthisis. I have carefully interrogated a considerable number of patients, who have

* The few, but admirably reported, cases of rheumatic fever given by Dr. Parkes in his Gulstonian Lectures *(Sup. Cit.)*, should be excepted from this statement.

† On the *Oleum Jecoris Asselli*; p. 49.

suffered from acute rheumatism, with regard to their family history, and have found, in a large per-centage of cases—especially in those in whom the dyscrasia seemed to have most firmly established itself—that strong evidence was obtainable of a marked family phthisical taint, to such an extent in some cases, that both parents, or two or three brothers and sisters, would have been carried off by the disease; whilst the patients themselves were at the time quite free from any signs of local or constitutional tuberculization. It may be thought that, on looking at the frequency of phthisis in families generally, such cases were only instances of coincidence, and do not warrant any positive conclusion: nor do I wish to place undue stress upon them, for that reason; but the history was generally so clear, and the fatality so marked in most of the cases examined, that I feel, for my own part, little doubt of the truth of the fact, even if it were not a probable one on *à priori* grounds, as well as in accordance with the above observations of Brefeld. From these two circumstances, therefore, I think that we are justified in drawing this conclusion—however paradoxical it may at first sight appear—viz., that there is a strong antithesis, and at the same time a strong connexion between rheumatism and phthisis: a strong antithesis, inasmuch as they rarely occur together in the same individual; and a strong connexion, in so far as their dyscrasiæ seem linked together by hereditary ties. How is this to be explained? I think that it may perhaps be explained in the following manner: Looking at the so-called dyscrasiæ as a group, we observe that one of their most distinguishing characteristics is chronicity. When once they establish themselves in the constitution it is a very difficult thing to get rid of them; and they impress their mark upon the whole system, stamping all its processes continually with the proof of their presence, and giving them a typical peculiarity, as distinct probably, in the case of each dyscrasia from all the rest, as it is from that of normal health. Moreover, though all these separate types appear in a classification to graduate into one another, yet each seems for the most part to possess a distinct and independent identity, and exhibits but little tendency to pass into another type, except under powerful and long continued influences. Such influences, however, are rarely brought to bear upon the individual, unless in the course of a long life, or under peculiarly favourable circumstances: thus, we rarely get examples of cancer and tuberculosis occurring together in their typical forms in the same patient in a state of activity, though we may find the one progressing where the other has been for a long while passsive; and, as shown by Rokitansky, may also find a combined dyscrasia not particularly typical of either. Nor do we often get phthisis and rheumatism occurring concomitantly; and there is very good reason for doubting whether we ever get the true sthenic pneumonia in a patient in whom the tubercular taint is well established, although an intermediate form

of affection is far from rare. The infrequency of pericarditis in phthisical patients, except from tubercle of the pericardium, is also an illustration in point. And the reason for all this seems to be that the dyscrasial taint, whatever it may be, has so moulded the economy in one way, and given it such tendencies to diseases of one type, that it is unable, except under rare stimulus, to take on diseases of another type: the momentum of its progress is, so to speak, too great to be overcome by any but influences of the most potent description. But though the transition of one type of dyscrasia to another be rare in the individual, it appears very probable from what we know of hereditary influences, that it may occur in the transmission of acquired peculiarities from parent to child, and that the inherited dyscrasia of the latter may be elevated or degraded in the scale in proportion as that of one or both parents is more or less marked.* I have not yet been able to determine whether the hereditary relationship of rheumatism and phthisis ever exhibits the phænomenon of atavism, though I suspect that it sometimes does. Of course, it must be perfectly clear that, for the present at least, this explanation of the facts in question is, and necessarily must remain, a purely hypothetical one; yet, it rests for its support upon a principle whose existence is as certain as that of any in nature, viz., that as there are different types of bodily organization—all referable to a primitive archetype—and as there are different types of mental character equally derivable from a psychological archetype, so there are different types of bodily constitution, characterized by definite pathological tendencies, exhibiting a marked influence over the discharge of all the animal functions and the development of all diseases, and bearing a definite relation to the physiological archetype of perfect health. This principle is not so generally recognized by medical observers as it ought to be; for it is one of primary importance in estimating any deviation from the strict line of physiological activity, and lies at the root of all rational pathology.

From the foregoing considerations it follows that rheumatism, like all other local idiopathic inflammations, recognizes in its etiology two distinct elements: a constitutional one in the altered relations of the fibrinous matters of the blood, both as to quality and quantity; and a local one in the general affinity which exists between the fibrous tissues and the hyperinotic plasma, just as there does between the adipose tissue and the fatty matters of the blood when the latter are in excess. It cannot be too strongly insisted on, that this tendency of the hyperinotic matter to the tissues with which it is homologous, is strictly a physiological process of temporary excretion, by which it is removed for a time from the circulating system, when existing in such an amount as is incompatible with the healthy performance of the functions of that fluid. The idea

* See the very interesting chapter on "Hereditary diseases," in Sir H. Holland's *Medical Notes and Reflections.*

of Treviranus, so admirably developed by Mr. Paget, that each individual organ and tissue stands to the blood in the relation of an excreting centre, in virtue of the power by which it extracts from that fluid the elements which are necessary for its own nutrition, but which, if retained in the blood, would be fatal to the nutrition of the rest of the body, no where finds a better illustration than in the case of rheumatism. Here we have a sudden increase in the fibrinous matters of the blood by which a state of things is brought about which is in every way incompatible with a healthy condition of that fluid. To obviate this, a portion of the fibrinous plasma is removed out of the circulating current by an exaltation of the affinity of those tissues which exercise a natural attraction over it. By this means the blood is relieved of a portion of the burden under which it laboured, and is enabled to eliminate the excess of fibrinous matter which remains behind by a proportionable increase in the vigour of its excreting processes, whilst the extra vascular exudation at the same time passes through its metamorphosis so slowly, and reverts to the blood so gradually, as not to embarrass greatly any of the excreting functions; whereas, had the whole amount of fibrine exuded disintegrated in the blood, with a rapidity proportionate to that with which it was produced, a state of intense fever would have been excited, and a sudden increase of labour would have been thrown on the eliminating organs, that would have been highly prejudicial to the general safety of the economy. This latter state of things, it is true, we do get occasionally in those cases where the febrile symptoms are strongly marked, but the local affection almost absent; and these are just the cases whose convalescence is most protracted, and which leave the constitution in the most shattered condition from the violence of the shock to which they have subjected it. The amount of fever is by no means necessarily in a direct ratio to the intensity of the local affection; but it is always so to the metamorphosis of the fibrinous matter, and, consequently, to the subsequent increase in the quantity of excreta. And this is very intelligible; for, the resolution of fibrine into its simpler and more excretable elements is a process of oxydation, and involves a consequent elevation of temperature: hence, it is only to be expected, that where we have this oxydizing process going on very vigorously, we should find a corresponding rise in the general temperature—a symptom too evident in rheumatism, as well as in other diseases accompanied by rapid histolysis, to need further notice. In this light, therefore, the local affection in rheumatism will appear as a sort of safety-valve against the general constitutional severity of the disease, and as a symptom that ought to be rather cherished and encouraged than feared and depressed, except where, from its mechanical obstruction of the action of important organs, as the heart, it is fraught with immediate danger to the patient. In this light, too, we may call the exudation a *primary crisis* of the disease, the sudden exaltation of the excretions

at a later stage will then become a *secondary crisis*; the phænomenon thus bearing a considerable resemblance to the form of crisis, to which Dr. Stokes has drawn attention, as occasionally occurring in typhoid fever, when that disease terminates suddenly during a protracted convalescence in acute tuberculization of the lungs.

The preceding observations, though referring primarily to acute rheumatism, will apply equally well to the chronic form of the disease, in which the joint-affection results either from a succession of subacute attacks; or from a constant hypertrophy of the fibrous tissues, dependent upon an excess of their proper pabulum in the blood. But I am anxious to draw attention to another chronic phase of this most protean dyscrasia which, as far as I am aware, has hitherto received but little notice from the profession, but which is of some importance in a pathological point of view: I allude to a connexion which frequently exists between rheumatism in early life, and fibrous degeneration of the liver and kidney in later years. The whole question of fibrous degenerations, complicated as it is by questions on which the greatest obscurity rests, must undoubtedly be considered as still *sub judice;* but there can be little doubt that the old idea of these changes being necessarily or even frequently the result of acute inflammatory processes, is fast disappearing from the mind of all reflecting observers.* With regard to one form of fibroid degeneration, however, that of the small, hard kidney, it appears to be acknowledged by many recent writers of repute as clinical investigators, that it is essentially distinct, both in its origin and pathology, from the enlarged forms of Bright's kidney, of which Frerichs and others have asserted it to be only a subsequent stage; and that it is closely allied to the fibroid degenerations of the liver, lungs, and other organs by which it is often accompanied.† It seems probable, also, that careful clinical observation will establish the fact, that there are at least two forms of cirrhosis of the liver, and that the small contracted form is distinct in its origin and pathology from the enlarged form, as has been suggested by Dr. Todd. However, this is no place to discuss these points: I only refer to them, *en passant*, as connected with the question of granular kidney and its relations to fibroid degenerations in general.

Now, that there is often a connexion between renal disease and rheumatism was indicated long ago by Dr. Christison; who, however, does not seem to have attempted any explanation of it. He says: "One of the most frequent secondary affections connected with granular kidney is chronic rheumatism. On investigating the early history of many cases which have come under my notice in the advanced stage,

* See especially Dr. Handfield Jones' articles on Fibroid Degenerations in *British and Foreign Medico-Chirurgical Review;* vol. xiv.

† See Dr. Wilks' cases of Bright's disease, in *Guy's Hospital Reports*, vol. viii; also *Jones and Sieveking's Pathological Anatomy*, p. 610; and *Clinical Lectures on Urinary Diseases*, by Dr. Todd.

my attention has been drawn to the frequency with which reference was made to rheumatic pains as one of the earlier symptoms: repeated instances of the same complications have occurred after the admission of patients into the Infirmary; and, in short, this connexion has appeared to me so far common that I never meet with cases of obstinate chronic rheumatism without being led to make inquiries into the state of the urinary secretion."* An additional argument for the existence of acute rheumatism in the earlier part of the lives of those who subsequently die with granular kidney, is the frequency with which old diseases of the heart are met with in their cases.† In Dr. Wilks' cases above referred to, out of thirty-three cases of death in which the hard contracted kidney was found, and in a large number of which no history whatever was obtained, there are four cases in which it is distinctly stated that the patients had laboured under acute rheumatism in early years; and it is open to doubt whether even in all those from whom a history was obtained this point was investigated. This, it may be said, is no large per-centage, and these cases may have been merely coincidences; but it is rather singular that out of fourty-four cases of the large white, coarse granular, and lardaceous forms of kidney degeneration in the same report, not a single mention of rheumatism is made! The fact, however, that the concurrence is a real and not an apparent one, is corroborated by many cases which I have met with in the journals, in books, or have myself observed, all of which have tended to convince me that the rheumatic dyscrasia is a very frequent, though not an essential element in the production of fibroid degeneration of the kidney, liver, and other important organs. To those who are acquainted with the literature of renal pathology, it does not require reminding that Drs. Todd, Johnson, and others have been in the habit for some time of using the term "gouty" as a synonym for the "contracted" kidney, and of pointing out how frequently it is connected with the gouty diathesis: the former author, indeed, gives some cases in his Clinical Lectures above quoted, which by many persons would be rather diagnosed as rheumatic than gouty in their affinities; but with this exception he has treated the subject in his usually clear and able manner, although, with all deference to his high authority, I cannot but think that he has failed to appreciate the whole of the pathology of the change which he discusses. The connexion between rheumatism and renal disease was not unnoticed by Rayer, who gives an excellent description of the condition of the kidney, both in the chronic and acute stages.‡ As far as my own observations go, these rheumatic cases of granular kidney are the ones which are most frequently accompanied by an allied degenera-

* *On Granular Degeneration of the Kidneys*; p. 97.

† Christison: *Id. loc.*

‡ *Maladies des Reins*: art., *Nephrite Rhumatismale.*

tion of the liver, and occasionally of the spleen and lungs; but more extended observations are requisite to set the matter in an indubitable light. In these cases it would occur as if the fibrine-crasis—so to designate conveniently a condition which could be only otherwise described in a periphrastic manner—at first directed its activity against the fibrous structures of the joints, pericardium, &c., and afterwards levelled its attacks upon the fibrous matrices of the abdominal viscera; first producing hypertrophy of them, and then leading to the contraction which often results from this (as in the cicatrices of burns)—to the atrophy of the organs into whose structure they enter—and finally to complete abolition of their functions. This change in the affinities of the fibrinous plasma must depend either upon an alteration in its type, or upon the exaltation of the fibrous structures of the abdominal viscera over those of the joints as attractive centres. In the latter case it becomes an interesting question whether the increased work which is thrown upon the liver and kidneys in the rheumatic diathesis may not serve as a kind of *point de départ* in exalting the affinities of their fibrous structures for their normal pabulum. In relation to this hypothesis the view of Henlé may be quoted, who considers the hypertrophy and subsequent contraction of the fibrous matrix of the kidney one of the weightiest elements in the production of Bright's disease (the contracted form), applying to it the name of cirrhosis of the kidney.*

In the foregoing considerations on the nature of rheumatism, I have endeavoured to exhibit the hyperinotic condition of the blood as the most important element of the disease, and to show that the different organs to which the rheumatic dyscrasia may direct its activity are probably determined to a certain extent in accordance with variations in the type of the dyscrasia itself. I have also attempted to show that all the phænomena of this disease are perfectly explicable on this hypothesis, and that any theory of their pathology which omits a consideration of the concomitant hyperinosis, is deficient in a most fundamental particular. I might further extend this observation to pneumonia, and to other idiopathic phlegmasiæ, were this the place to do so; but this would lead to a discussion of matters that could not conveniently be included within the limits of an ordinary paper. Indeed so obvious is the necessity of not overlooking this most essential feature in erecting a pathology of inflammatory affections, that it did not escape the notice of Andral, even writing at the time from what was, and almost still is, the hot-bed of the Broussaian doctrines; from whose philosophical and most suggestive little work on Pathological Hæmatology I shall take the liberty of quoting a few passages. "On the other hand," he says, after referring to that hyperinotic condition of the blood, which is undoubtedly an *effect* of a traumatic lesion, "there are of a truth cases

* See his *Handbuch der Rationellen Pathologie;* b. ii, s. 305.

where the alteration recognizable in the solids is so slight and fluctuating in its nature, and occasionally so transient, that one can hardly conceive that it can be the cause of the profound and persistent change which the blood exhibits in these cases. Is not this the case, for instance, in many articular rheumatisms? and is it not remarkable that this disease is one of those in which the increase of fibrine is most considerable? In all this there is much that is yet unknown to be investigated: we have to discover the mysterious relation which unites in those diseases which are called phlegmasiæ the alterations of the solid to that of the blood; but in all cases we may now conclude from this simultaneity of affection in the two, that what we call an inflammation is a disease which does not reside solely in the solid where anatomical examination reveals its existence—that it is not only by so-called sympathetic irradiations that it exercises any influence upon the rest of the organism, and that the alteration which exists in the blood must play an important part. An inflammation is not, therefore, merely a local affection. Is it upon the excess of fibrine that the fever which accompanies every acute phlegmasia depends? In answering this, it must always be remembered that there is an almost constant correlation existing between these two facts; and that if the fever arises at the same time that the fibrine increases, on the other hand it ceases directly the fibrine returns to its normal amount."* Everything which comes from so acute and sagacious an observer as Andral, must carry with it the greatest weight; and these views are confirmed by Becquerel and Rodier in their recent work on Pathological Chemistry, where they observe, "An increase of fibrine in the blood is characterized by a remarkable phænomenon, viz., a tendency to secondary inflammations. The diffuse nature of the joint affection in acute rheumatism, and the occurrence of endocarditis in the course of this disease, as well as of pneumonia, pleurisy, &c., is in all probability a result of the increase of the fibrine."†

There is a strong collateral proof of the connexion existing between phlegmasial affections and a precedent hyperinosis, in the fact established by Andral and other observers, that in the period immediately preceding and following parturition a hyperinotic condition of the blood is a normal and physiological state. The question, therefore, suggests itself —is this period fertile in inflammation? A glance at the nature of the puerperal class of diseases is sufficient to answer this in the affirmative; but the inflammatory tendency is chiefly confined to the pelvic viscera and the parts in their immediate neighbourhood; for the exciting causes of inflammatory affections of the joints, heart, and lungs, are generally absent in women who have been confined for some time to a recumbent or sitting posture, and have been exposed to no vicissitudes of tempera-

* *Hématologie Pathologie*; art., *Phlegmasies.*

† *Op. Sup. Cit.*; p. 57.

ture; whereas the local irritation in and about the uterine region forms an effective determining element for inflammatory affections in that quarter. This theory of the influence of the hyperinoses of pregnancy in predisposing to inflammatory affections is confirmed by MM. Becquerel and Rodier, who remark that "it is not improbable that this increase of fibrine [in pregnancy] may account in a manner for the facility with which the phlegmasiæ manifest themselves during the puerperal period which follows the act of parturition."* Chomel, however, distinctly states that acute rheumatism is by no means an infrequent sequela of parturition; in which he is borne out by Bouilland, though it might be suggested that some of the cases which have been described by that author, under the designation of *arthrite rhumatismale puerperale*, look uncommonly like purulent affections of the joints consequent upon pyæmia. It is to be presumed that these instances of puerperal rheumatism occur in patients who prematurely expose themselves to vicissitudes of temperature, or undertake labourious occupation before nature has removed the excess of fibrine by the usual outlets of excretion. To this condition is also probably attributable in great part the rapid progress which pulmonary tuberculosis makes after parturition; which Rokitansky explains by referring it to the increased facility which is given to pulmonary action after the uterus is emptied, in accordance with his theory of the dependence of tuberculosis upon a high development of the arterializing process; but looking to the recognized connexion between tuberculosis and a hyperfibrinated condition of the blood, it seems only reasonable that the spontaneous creation of the latter state during parturition should be followed by a tendency to deposit when the exigencies of the system no longer require the fibrine, especially if a nucleus of attraction exists at the same time in the shape of previously deposited tubercle. And as in the different forms of pathological hyperinosis the fibrine takes on a developmental type and power of independent vitality commensurate with the general constitutional vigour of the system, so in the puerperal hyperinosis the excess of fibrine which remains after parturition may do the same; hence the peculiar proneness of the patient to rheumatic or tubercular forms of exudation will depend partly upon the accidental circumstances under which she may be placed, but much more intimately upon the developmental type which the system may be able to impress upon the exudation itself.

Hitherto the pathology only of rheumatism has been discussed: it now remains to be seen what light can be thrown upon its etiology, and more especially upon the origin of the hyperinosis by which it is characterized. The sources of an excess of fibrine in the system probably fall under three heads: 1st, as a result of imperfect primary

* *Op. Sup. Cit.*; p. 56. See also *Mackenzie on the Connection between Phlegmasia Dolens and the Hyperinosis of Pregnancy—Medico-Chirurgical Transactions*; 1853.

assimilation; 2nd, as a result of metamorphic processes—normal in nature, but extreme in amount; 3rd, as a result of defective elimination of the fibrine by the excretory processes provided for that purpose. An excess of fibrine as a result of imperfect primary assimilation may be supposed to arise in two ways: either from a highly animalized form of diet, and a preponderance of the albuminous elements of the food, or from a comparative deficiency of the fatty matters which combine with them to form healthy chyle. With regard to the former source, I know of few facts which would justify our accepting it as the cause of a constant hyperinotic condition of the blood, at least in rheumatism; and the experiments of Majendie and others show that a pure albuminoid diet does not tend to produce exudations of any kind, but rather to develop a species of dyscrasia, which ends in the speedy breaking up of all the tissues.* The partial absence of fatty matters in the food, however, and the consequent formation of an imperfect chyle, is a very different question, and deserves a careful consideration.

The importance of a due proportion of fatty matters in every form of diet to a continuance of health, is so generally insisted on by all physiologists, that it is unnecessary to offer any proofs of it here. I need only refer to Professor Bennett's work on the Oleum Jecoris Aselli for an able summary of the histogenetic functions of fat in the animal economy, and a reference to its diminished amount in the food as an important cause of tuberculosis. The notion of the distaste of phthisical patients for fatty matters has long been entertained, and is, in fact, so patent in well marked cases, that it could not well escape notice; but I am not aware that it was ever regularly demonstrated, or its value statistically determined, until Mr. Hutchinson in an able paper on the "Dyspepsia of Phthisis," in the *Medical Times and Gazette*, about three years ago, showed, by a careful and detailed examination of a large number of consumptive patients, that its existence was even more common than had been supposed. He also pointed out the importance of this distaste in a prognostic point of view, and showed that the greater the distaste of the patient for fatty matters, the less hope was there of the toleration and consequent beneficial effect of the cod-liver oil; and that those cases, also, were the least amenable to any kind of treatment in which the distaste was most intense.

Whilst residing at the Brompton Consumption Hospital some time ago, I examined a large number of patients with the view of testing Mr. Hutchinson's statements, and I can most amply corroborate them as regards the importance of this distaste for fat in phthisical patients. It is true that there are many cases of phthisis in which no such a distaste is traceable; and, looking at the varied circumstances under

* The fact that a disease so closely allied to rheumatism as gout is generally connected with a highly animalized form of diet, is however worthy of consideration in reference to this point.

which that vitiated state of the constitution which results in tuberculosis may be produced, any theory which attempts to ascribe it to one uniform set of causes is illogical and absurd. But there cannot be a doubt when we consider the great histogenetic importance of fatty matters, and especially of those which enter into the constitution of the chyle, that this often long existing and intense distaste, explain its origin as we will, is a very serious and important symptom. From considering the relationship of this distaste to the hyperinotic state of the blood in phthisis, I was led subsequently to extend the same examination to patients labouring under acute rheumatism, and I found that in a very large number of the worst cases—in those in whom the dyscrasia seemed to have established itself with the firmest hold—the same distaste for fatty matters was present, extending in some cases almost to a disgust for them. I should state that I have found this symptom to exist more commonly in female than in male patients: why so, I am unable to suggest, unless it be that the digestive functions of the former class share in the more delicate sensibility and emotional susceptibility which their bodies generally possess as compared with the stronger sex. Many causes have prevented my tabulating the results of this examination in such a form as would have rendered them available for subsequent reference; but the impression produced on my mind by a considerable number of cases is, that in acute rheumatism the distaste for fatty matters plays nearly as important a *rôle* as it does in phthisis. Now, the result of this imperfect assimilation of fatty matters, continued as it is in some cases for many years, must seriously affect the composition of the chyle, and through it that of the blood; and the tendency of this deficiency of fatty matters in the chyle would probably be to leave its albuminoid elements in a state of imperfect emulsification, so to speak, and to give them an undue preponderance in the constitution of the blood, and more especially in that of the blood corpuscles. Now, it is very possible that this deficiency of fatty matters in the newly formed blood corpuscles may lower the standard of their vitality and give them a tendency to premature and sudden disintegration, under the influence of agents by which they would be normally unaffected—a state of things which we shall presently see to be of highly probable existence in the rheumatic dyscrasia especially, and to be one of the most important predisposing elements to acute attacks of the disease. To these remarks upon the importance of the fatty matters of the food, however, it may be objected that physiology has shown that one of the most important functions of the liver is the formation of fat, and that so long as this organ performs its office, the ingestion of fatty matters from without is not absolutely necessary. To which it may be replied—firstly, that in most of phthisis, as also in many of rheumatism, the functions of the liver are most imperfectly performed from a very early

stage of the affection, and, consequently, the supply of fatty matters to the system by that organ must be very inadequately discharged; secondly, that there are reasons for believing that the fat which is generated in the liver is sent into the blood in a free state, and is probably not destined for any higher end than the formation of adipose tissue, or by its oxydation to serve as a calorifacient material; whereas, the fatty matters which are absorbed by the alimentary canal are always intimately combined with albumen, and are probably incorporated at once into the vital unity of the tissues by being taken into the constitution of the newly forming globules. Certain it is that the globules contain a notable amount of fatty matters; and when we consider the whole amount of them circulating within the body, and the constant necessity for a new creation to supply the place of those that disintegrate, it will hardly appear that the quantity of fat taken up by the chyliferous ducts is too large to be accounted for in this way. At any rate, it is very clear that nature would never have dictated the necessity of the due admixture of fatty matters with the food with such force as she has done were such mixture a non-essential one, or could it be dispensed with for any length of time with impunity; and if nothing else were adducible to show the connexion existing in many cases between tuberculosis and *chronic* rheumatism at least, and a deficiency of fatty matters in the system, the sudden and marked improvement consequent upon the administration of cod-liver oil to patients labouring under those diseases would of itself be a highly probable indication of it. I have suggested that the long continued deficiency of fatty matters in the food would lead to a hyperinotic state of the blood: under ordinary circumstances the excess of fibrine would probably be eliminated by the excretions in the form of urates, lactates, urea, &c., a slight pyrexial condition being set up frequently, and the urine becoming more high coloured or the perspiration more copious than usual—a state of things which is notoriously of frequent occurrence in phthisical and rheumatic subjects. It seems, also, highly probable that the severe night sweats which generally distinguish the advance of phthisis are connected with an elimination of the results of the disintegration of this excess of fibrine, just as those of chronic rheumatism may be. It is certain that they must have some physiological function of an excretory character, and that they cannot be explained simply by a reference to the depressed vitality of the patient, inasmuch as they never occur in many diseases which are accompanied by extreme prostration, and in which the blood is in a much more favourable condition for the elimination of its watery portion than it is in phthisis—*e. g.*, dropsy.

We have now to enter upon the consideration of one of the most interesting, but at the same time one of the most obscure, questions connected with the subject of hyperinosis, viz., the rationale of its

immediate production, involving as it does the much debated question of the relationship of fibrine to the other elements of the blood. In the present undetermined condition of our knowledge on this point, it would obviously be unsafe to make any very dogmatic statements upon the positive origin of this substance; yet, on duly estimating the weight of the respective theories which have been propounded on this subject, it seems to the writer that the weight of evidence is decidedly in favour of the doctrine which regards fibrine as a further stage in the transformation of albuminoid matters, and as normally intended for histogenetic purposes; and, also, that the special theory of the late F. Simon, which regards it as a product of the disintegration of the globules, has many important facts to recommend it, though perhaps it may err in being too exclusive as to the possible production of fibrine from any other source. Much of the confusion that has arisen on this subject would seem to depend upon the failure of many authors who have discussed it, to apprehend the fact that there are in all probability several varieties of fibrine, perhaps, as suggested by Lehmann, of an allotropic character. In the present state of our chemical knowledge, our estimation of the nature of these varieties of fibrine can only be arrived at by a study of the affinities which they exhibit in certain pathological conditions for the various tissues of the body; and by such a study, combined with the prosecution of a more exact analysis of the blood in these conditions, it is to be hoped that we shall in time arrive at a more definite acquaintance with the subject than we at present possess.

According to the hypothesis of Simon above referred to, which he has developed with much ingenuity in his work on *Animal Chemistry*,* and which I shall here adopt, notwithstanding the weighty arguments which have been brought against them in some points by Zimmerman,† because in their general tenour they have been confirmed by the researches of subsequent investigators, the fibrine of the blood is the result of the metamorphosis of its globules: this metamorphosis is a vital phænomenon, and is due on the one hand to the influence of the oxygen with which the globules are brought into contact during their passage through the lungs, and on the other to the numerous reactions which occur to the blood when circulating through the various tissues of the body. Further, in addition to fibrine the disintegration of the globules gives rise to the transformation of their hæmatin into the hæmaphæin or colouring matter of the plasma. For the great ability with which Simon has stated his argument, I can only refer to his work above quoted; but a few of the facts upon which it is founded may be here mentioned, especially those which have a bearing upon the subject in hand. It follows from this hypothesis that if the fibrine

* Vol. i, pp. 147 *et seq.*; Sydenham Society's edition.

† See his *Ueber die Analyse des Blutes*, &c.

be the result of the decomposition of the blood globules, wherever the former is in excess the latter should be deficient: that this is really the case is proved by many facts. In chlorosis, in which the fibrine is increased, according both to Andral and Becquerel, the globules are correspondingly diminished; whilst the peculiar discolouration of the skin in that affection would seem to bear out Simon's explanation of the formation of the greenish-yellow hæmaphæin of the plasma. So important is the hyperinotic element in chlorosis in the estimation of Becquerel, that it suffices, according to him, completely to differentiate that affection from simple anæmia. Again: after violent exercise the amount of fibrine in the blood is increased, probably as a result of the unusual disintegration of the blood globules which is necessary to repair the waste of tissue consequent upon the exertion: hence the susceptibility of men who have undergone great exertion to inflammatory affections, rheumatism especially being more frequently induced by fatigue than by any other cause. In pregnancy, where the fibrine is increased, the globules diminish at a very early period of gestation.* Andral, Becquerel and Rodier, and Simon, all refer to the inverse ratio of the fibrine and the globules to one another in phlegmasial affections; and the latter expressly asserts that in all his analyses he has never found the one increased without the other being diminished. There is often a peculiarly chlorotic hue exhibited by the subjects of severe inflammatory diseases, especially pneumonia, which may also be susceptible of explanation on the theory of Simon in reference to the transformation of the colouring matters above quoted. In all the specific fevers, on the other hand, the fibrine is below the standard of health, unless local inflammations are present, whilst the globules exhibit their average amount.

The efficient agent in the transformation of the blood globules, according to Simon, is the oxygen received by the lungs; and in accordance with this theory he thus accounts for the increased activity of this process in inflammations. In all phlegmasiæ the circulation is more or less accelerated, and from its being thus quickened the globules acquire more frequent contact with the oxygen of the air, and pass oftener through the different tissues and organs. It follows, therefore, that more globules are converted into fibrine in a given space of time; hence an absolute increase of this element in the blood, and a decrease in the proportion of globules, which are no longer renewed by the introduction of food. To this theory it may be objected, as Becquerel observes, that these changes ought to take place whenever the circulation is accelerated; and we, consequently, ought to have a hyperinotic state in the specific fevers. To which it may be answered, that although the normal effect of an increased circulation of the blood may be to cause

* Becquerel and Rodier: *Op. Sup. Cit.*; p. 96.

a disappearance of its globules and an increase of its fibrine, yet it by no means follows that this should necessarily be the case under all circumstances; for it may happen that a change in the density or chemical reaction of the blood serum, or the temporary suspension of the functions of those organs whose duty it is to effectuate the disintegration of the globules, may put the latter in a very different condition to that in which they ordinarily exist.* Another objection has been raised to the exclusiveness of Simon's theory, founded upon the fact that not only do the corpuscles of the blood, but also its albumen, decrease in a direct ratio with the increase of the fibrine; hence it has been supposed that fibrine may be a result of the metamorphosis of the albuminous elements of the plasma. Of the fact itself the researches of Becquerel and Rodier leave no doubt, nor is the inference which is drawn from it by any means unreasonable; but is in accordance with other physiological facts. Hence it is possible that there are two sources from which fibrine is derived—from the disintegration of the corpuscles, and from the metamorphosis of the albuminous elements of the plasma; though whether these two may not be ultimately referable to a single one on the supposition that the albumen of the plasma is itself derived from the disintegration of the corpuscles, and an intermediate stage between them and fibrine, may be a subject for discussion. Upon the fact, however, that all these metamorphoses involve in some way or other an active oxydizing process and a consequent production of heat, many pathological phænomena, as well as the experiments of Drs. Richardson, Gairdner, and others, leave no reasonable doubt; but it may be questioned whether the theory of Simon, which attributes the first step in the transformation to the increased vigour of the oxydizing process, does not err in omitting to take into consideration the vitality and affinities of the blood corpuscles themselves, which must vary under different circumstances, and at all times must be more or less under the influence of the nervous system. The experiments of Bernard show that the sympathetic exercises a primary and most important influence over this metamorphosis; but whether it be in merely regulating the physical conditions of the vessels, or in directing the vital affinities of their contents, must be considered as yet undetermined. It must be unquestionably admitted that on many of the details of the formation of fibrine there still rests the greatest obscurity: no one, however, who considers carefully the physiological and pathological data here laid down, can have much doubt that they substantiate the general view of the process here laid down. That fibrine is not a result of the disintegration of the tissues seems to be clearly indicated by all that we know of the certain

* According to M. Cahen the proportion of fibrine in the blood is in an inverse ratio with the alkalinity of the serum, and in all hyperinotic conditions the proportion of the salts of soda is less than usual.—*Recherches Experimentales sur l'Alcalinité du Sang Humain—Bulletin de l'Academie de Médecine;* 1850.

products of the metamorphosis of tissue: nearly all the facts with which we are acquainted tend to show that when a member of the albuminoid group of elements has once entered into the constitution of the tissues, its removal from them involves the assumption of a metamorphic condition much more approximated to inorganic compounds than fibrine is—*e.g.*, fat, creatine, uric acid, urea, lactic acid, &c., all of which are known histolytic results. Finally, the doctrine of the formation of fibrine from the disintegration of the blood globules derives some support from the visible phænomena of inflammation, as they have been described by Professor Bennett, Mr. Paget, Brücke, and others: one of those which precedes the exudation of the fibrinous plasma being the aggregation of the corpuscles to one another, and then their disintegration and fusion into a solid mass, which appears to be the source of the fibrinous exudation. The increase of the pigmentary matter in the urine in phlegmasiæ is also a probable indication of an extreme disintegration of the corpuscles, as is suggested by Dr. Parkes (*Loc. Sup. Cit.*), and consequently of the accompanying increase of fibrine.

Before concluding, I wish to say a word or two by way of practical inference from the views above expressed as to the treatment of acute rheumatism. A consideration of the state of the blood in this disease clearly shows that there are two chief indications to fulfil in its treatment, viz., to check if possible the rapid formation of fibrine which characterizes its early stage, and to promote the elimination of that excess by the outlets which nature has provided for it in the renal and cutaneous excretions. The two most practicable methods of accomplishing the first point would seem to be, by the administration of large doses of caustic alkali, as frequently practised, and as indicated by the statement of M. Cahen above quoted; or by the employment of such neutral salts as possess at the same time a solvent action on fibrine, and diuretic properties. MM. Becquerel and Rodier assert that bleeding produces a decided diminution in the fibrine of the blood. This observation is certainly opposed to those of other authors; and, even if it were correct, it is a serious consideration whether this advantage of bleeding is not counter-balanced by the prolongation of the convalescence, and by the tendency to relapse which it produces: if justifiable at all, it certainly is so only in the earliest stage, in strong constitutions, and in a sthenic type of affection. The statement, also, of Becquerel—that immediately after a large bleeding in rheumatism the urine assumes for a period the characters of that of anæmia—is certainly corroborative of its effect in diminishing the amount of fibrine. The fulfilment of the second indication cannot be better carried out than by the treatment sometimes adopted, viz., the copious administration of slightly acid drinks, the necessity of an abundant supply of aqueous matter to the blood being indicated in this as

in all other febrile affections by the extreme thirst present. A knowledge of the true pathology of rheumatism will enable to estimate at their proper worth the real utility of the various specifics which are constantly being vaunted, even in the present day, as a cure for the disease, and to see that rheumatism, like all the other phlegmasiæ, has a definite course to run, and that when once completely established it cannot be cut short. The danger of relapse which a too early adoption of an animalized form of diet entails is well established by experience, and may be explained by reference to the fact that the fibrinous elements of the blood are already in excess, hence the premature indulgence in a meat diet is only adding fuel to the fire which has hardly yet burnt itself out in the system, and instead of giving strength is but arousing the slumbering energy of the disease. Bnt there is one medicament which both theory and experience indicate as a dietetic remedy as soon as the febrile symptoms have disappeared, and the patient is entering upon the convalescent stage; and that is the cod-liver oil. It is somewhat strange that this remedy should have been employed for years with success in the chronic form of rheumatism, and yet be rarely or never used in the acute variety of the disease. If the views which I have endeavoured to substantiate be correct, it is evident that this is almost the only article in the Materia Medica which promises to be of any avail in the constitutional and prophylactic treatment of rheumatism. Looking at the relationship which I have shown frequently to exist between a chronic distaste for fatty matters and the rheumatic dyscrasia, it can hardly be denied that we have here alone a very strong indication for the administration of the oil. All modes of treatment which we at present employ can only claim to be directed against the transient symptoms of the attack, the constitutional predisposition being untouched, and no immunity given against future invasions of the disease. It is with the view, therefore, of combating the dyscrasia itself that I would recommend strongly the administration of the oil during the convalescent stage, and a persistent continuation of it for some time afterwards, in proportion as the dyscrasia seems firmly rooted in the system, or when oily matters are specially indicated by the previous existence of a distaste for them. Of course, as this distaste itself must have a cause, probably in an atonic state of the digestive system, much of the good to be derived from the oil will depend upon the mode of its administration, and upon what measures are taken to ensure its toleration by the stomach. I would also refer to the necessity of impressing upon patients who may require such advice, the importance of cultivating the habit of eating a due proportion of fat with their meat—a habit whose establishment in early life is to be especially recommended.

The object of this paper has been to show that the fundamental and

characteristic feature of rheumatism is a hyperinotic condition of the blood, and that all the other phænomena can be most satisfactorily explained on this basis. I have also endeavoured incidentally to show that Simon's theory goes a long way to explain the method by which this condition is immediately produced, and that it is borne out by some of the symptoms which present themselves in the course of the attack. How far I have been successful, or not, the reader must judge for himself; but I must specially protest against the imperfect manner in which the theory has been treated here being allowed to militate against its reception. It would be impossible, even in a paper of double this length, to discuss in a sufficiently comprehensive manner the many important questions which enter into its consideration: any parsimony, therefore, which may have been exhibited in the treatment of its details has been dictated rather by the necessities of space than by a want of matter. No one can be more conscious than I am myself that even this theory, greatly as I believe it to be in advance of those which at present hold the field in the scientific world, leaves many points unexplained which can only be cleared up by the future progress of science, and many more in obscurity on which perhaps we may never obtain light. Still, however great our darkness may be on some points, it should form no bar to an attempt to methodize our knowledge on others; and there is the strongest ground for asserting that the gleaners in the field of science have already accumulated a sufficient number of facts in relation to rheumatism to enable us to place its pathology in a clearer light than has hitherto been done. Much yet remains to be accomplished; but the work of future explorers will be facilitated in proportion as they are able to rationalize the labours of those who have preceded them, and to interrogate nature by the aid of hypotheses which have a shadow of truth, at least, in their favour. For it is by the copious employment of hypotheses alone that the philosopher can hope to tread successfully the slippery path of scientific discovery. In the words of an able and profound writer, "He who has to discover the laws of nature may have to invent many suppositions before he hits upon the right one; and amongst the endowments which lead to his success we must reckon that fertility of invention which ministers to him many such imaginary schemes, until he finds one which conforms to the true order of nature."* It is hardly necessary to add that the general doctrines upon which this theory is founded are by no means new; but have been developed by some of the first continental authors, and have been warmly endorsed by many authors of great repute in our own country. To the reader who is *au courant* with the literature of the day, it is unnecessary for me to point out how much I am indebted to them; and I trust that any omission to cite

* Whewell: *Philosophy of the Inductive Sciences;* vol. ii, p. 54.

chapter and verse on many occasions may not be imputed to an affectation of originality on my part. I must, however, specify one work in particular as one from which much of the present paper has drawn its inspiration: I mean the *Surgical Lectures* of Mr. Paget, to whom, as much as to any living English writer, science owes an inestimable debt of gratitude.*

* Since the above was written, but contemporaneously with the appearance of the first part of the paper, an extremely interesting contribution has been presented to the pathology of rheumatism by Mr. Toynbee (*Medical Times and Gazette;* April 10, 1858), in which he advocates a view of the disease identical in many points with the one supported in the preceding pages. Mr. Toynbee's coincident and unexpected corroboration, independently of his merits as an original observer, will, I trust, give the theory a better claim to examination than it possessed before.