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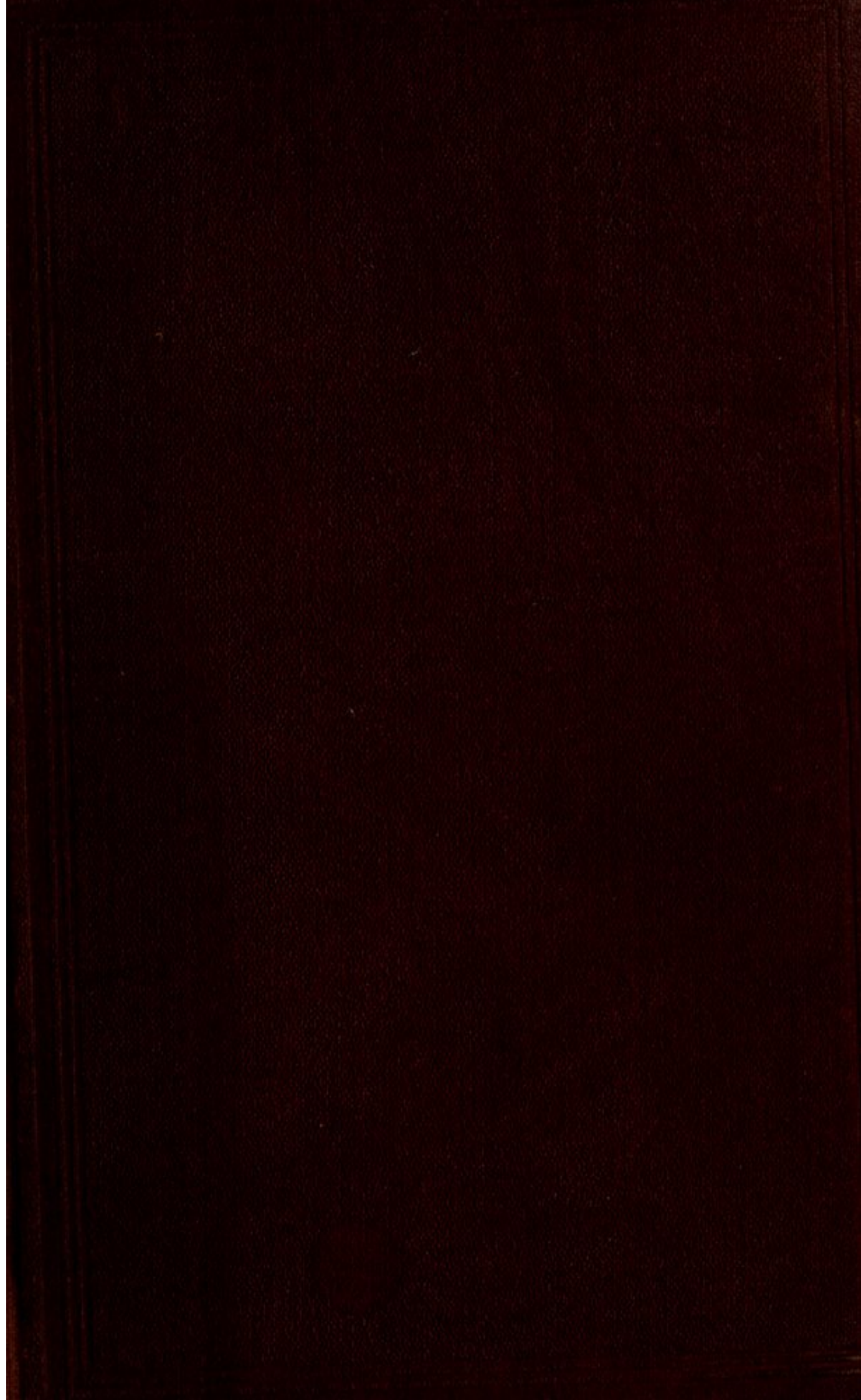
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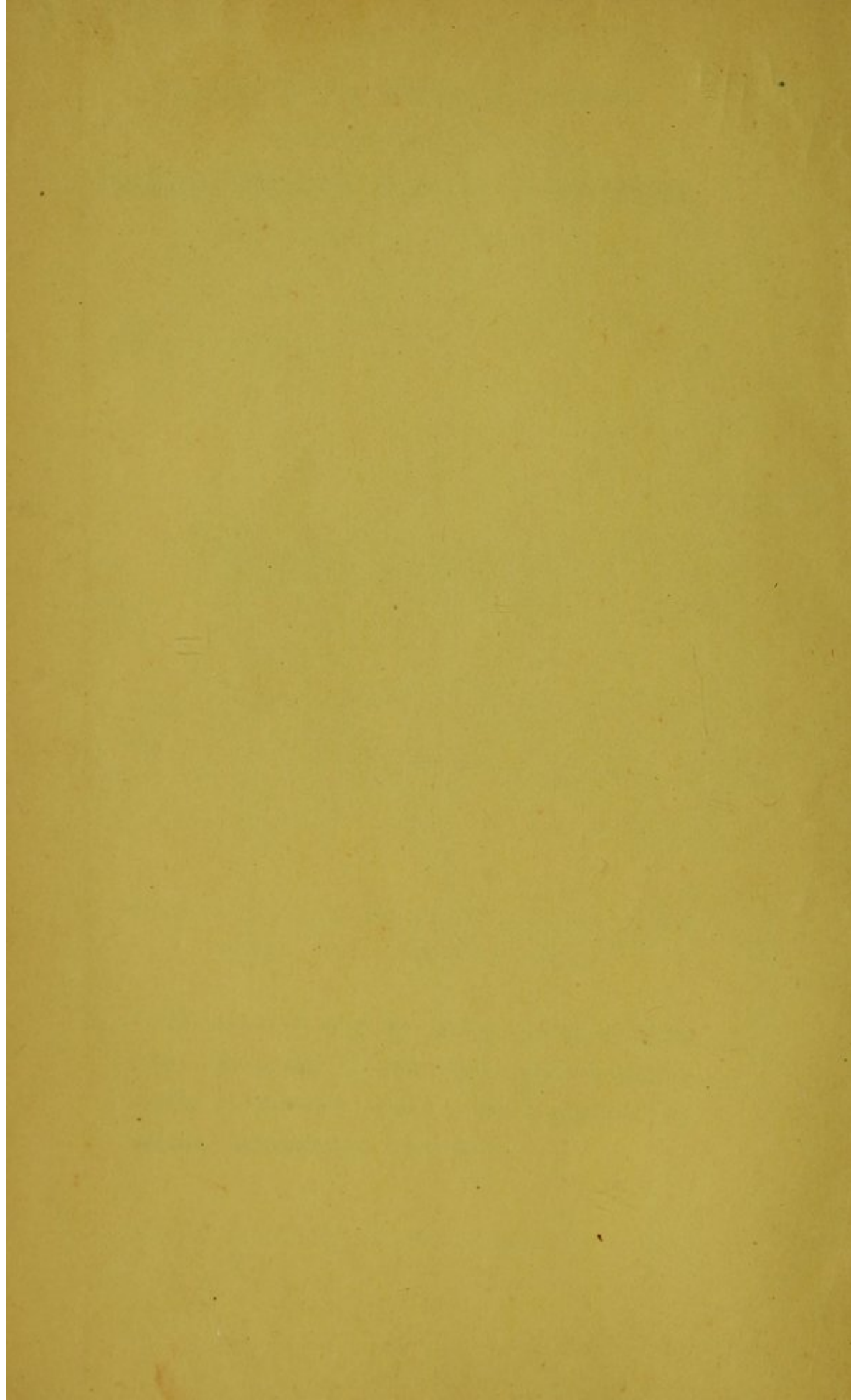
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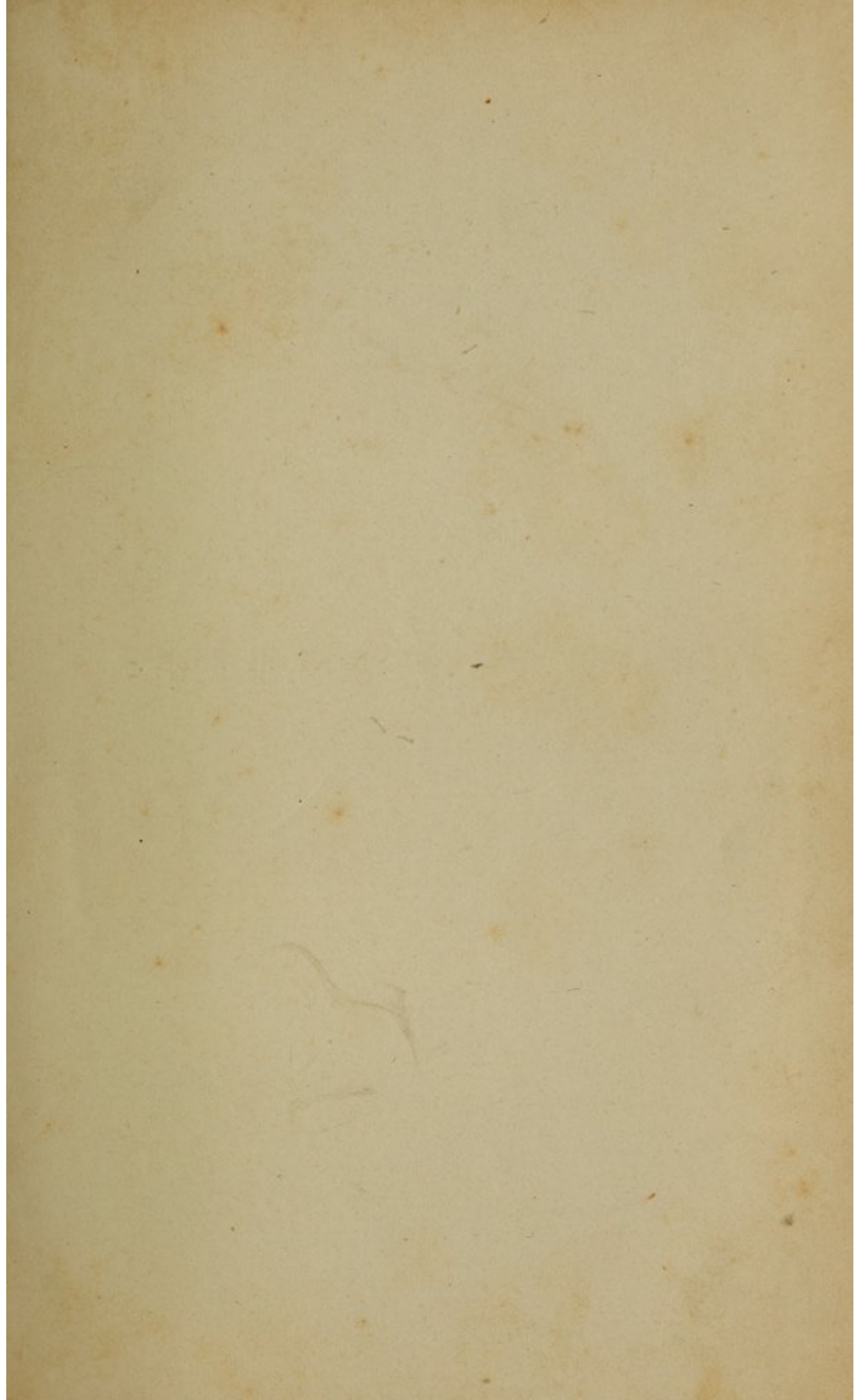
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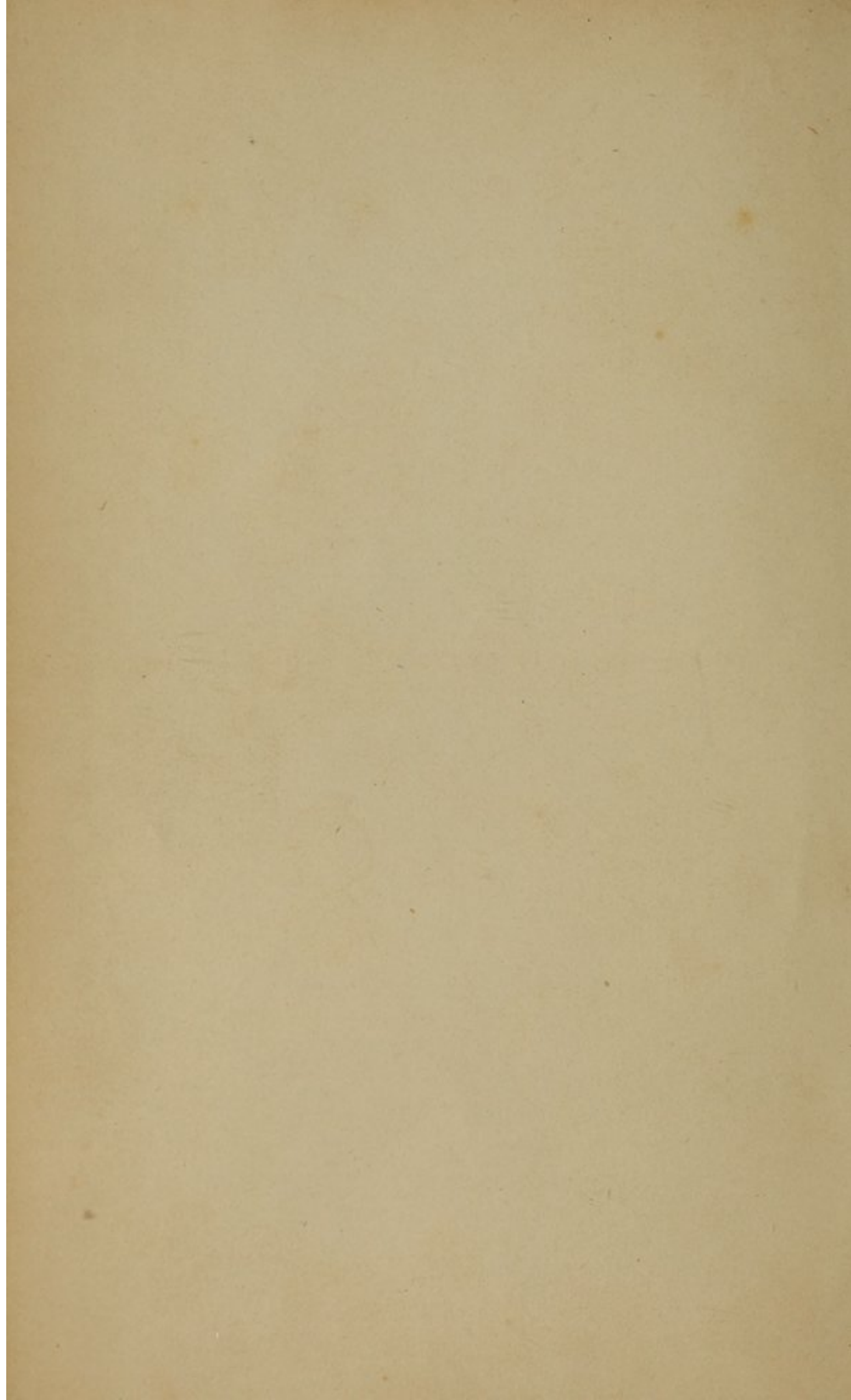
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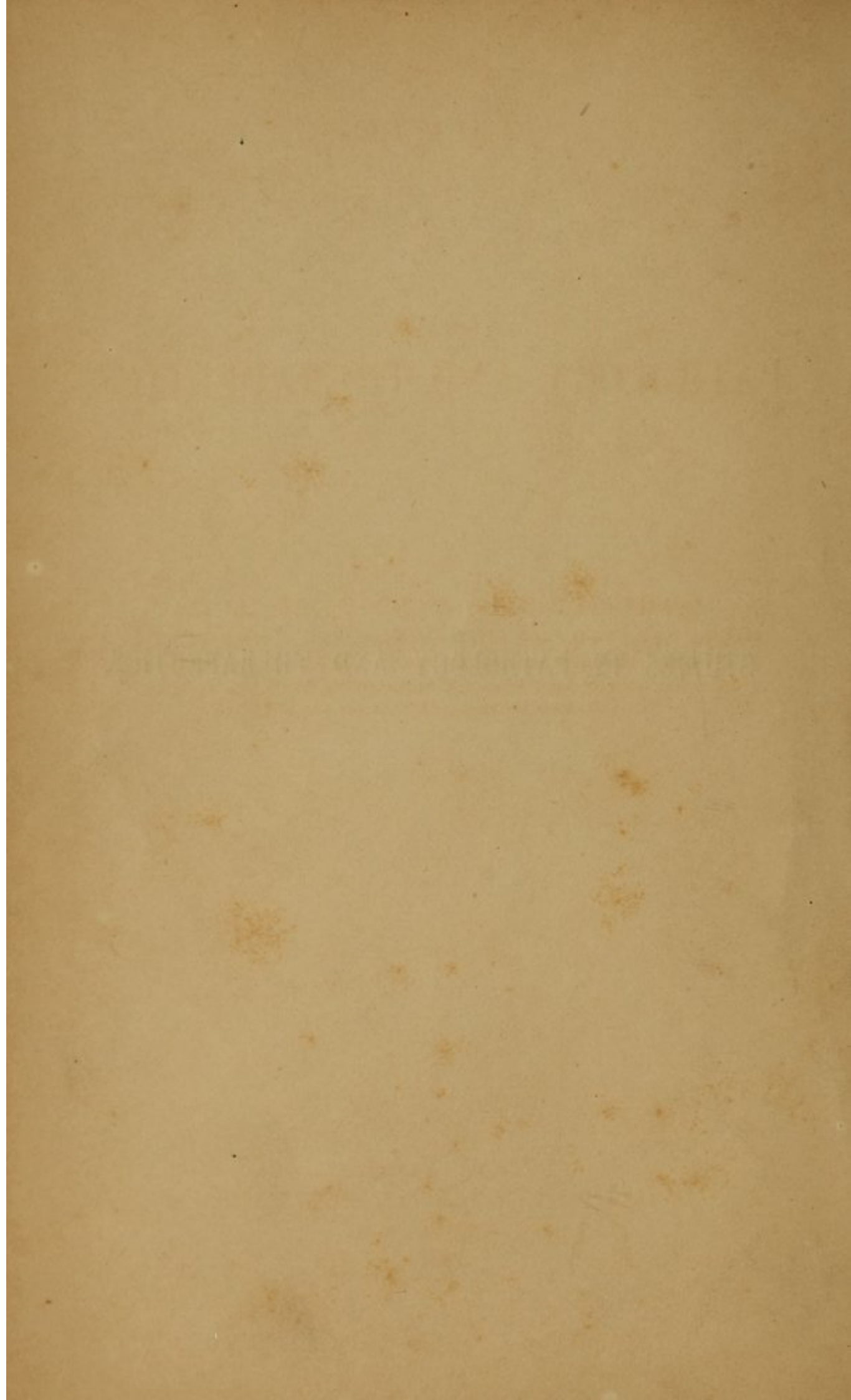
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STUDIES IN PATHOLOGY AND THERAPEUTICS.



STUDIES
IN
PATHOLOGY AND THERAPEUTICS.

BY
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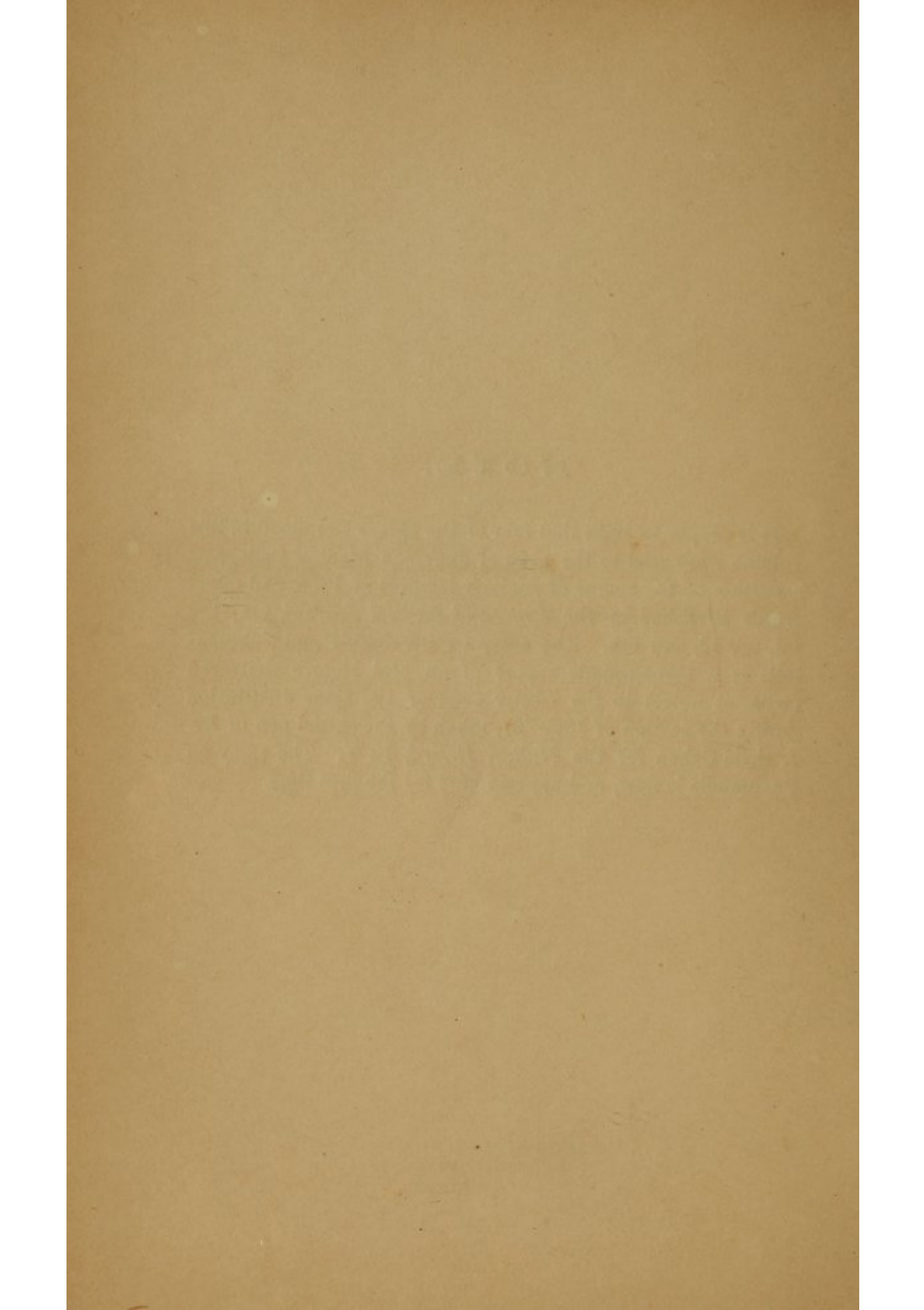
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NOTE.

It is proper to state that four of the essays contained in this volume were read to the class of Jefferson Medical College as a portion of the course of summer lectures of 1866, and afterwards published in the *Richmond Medical Journal*, viz., the 1st, 2d, 3d, and 6th. The essay on Scrofulosis and Tuberculosis is a stenographic report of an oral lecture delivered (without notes) to the winter class of the same Institution during the session of 1866-'67, taken by the skilful pen of Dr. J. Solis Cohen for the *Medical Record*, N. Y. The essay on Pneumonia is new, and appears here for the first time.



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WILLIAM B. DIXON

THE UNIVERSITY OF CHICAGO

PHILADELPHIA, PA.

1892

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DISEASE: ITS CHARACTER AND TENDENCY.

THE discussion of the nature, origin, and cause of diseases has become emphatically practical. In the progress of scientific research, our profession has proved itself unwilling to trust any longer to absolute empiricism in therapeutics, and we feel ourselves compelled to follow some definite methods of logical reasoning, in explanation of the various modes of treatment suggested for the vast variety of maladies which fall under our observation and care.

We must decide, each for himself, at the outset of our career, what course we shall pursue. If disease be merely an exaggeration and intensification of health; if fever consist merely in augmentation of vital heat, as Parkes and Ringer seem to maintain, and Aitken seems to admit; if inflammation be mere phlogosis; if all irritation, with its vast diversity of phenomena, be, as Broussais would have it, mere excitement of normal irritability; then, following Broussais, we shall be content with subtracting from its pabulum, with abating the inordinate *vis vitæ*; absolute diet and leeches and gum-water will constitute our therapeutic arsenal. If, with Brown, and Todd, and Chambers, we look upon it as essentially asthenic, as consisting in abatement of vital power on the other hand; we shall trust for "the renewal of life" in stimulants, cordials, and nutritious food and drinks. If we regard it as an effort of a special restorative, or protective agent—a *vis medicatrix*—"an inherent curative power," we shall be careful how we interfere with it; we shall be content with the most patient expectancy, and institute no measures, unless in subserviency and correspondence with the movements of nature. We shall

further endeavor to discriminate between the modes in which this great power is presumed to operate; whether by reaction against sedative or depressing agencies, or by the various processes of elimination, through which material causes that annoy, disturb, and derange the animal structures and functions, are thrown off.

We shall deeply ponder the question as to the tendencies of disease. Are they in their character injurious or beneficial? It may seem strange that such a question should be entertained, but we cannot escape its discussion. Diseases do not all terminate in death, or permanent lesion of any part. When they do, we must examine why it so happens. The orthodox teaching of the day is certainly favorable to the view that ordinary attacks, at least, are to be considered as "tending to recovery;" a phrase so paradoxical, so palpably contradictory, that I would not even venture to quote it, if I did not find it sanctioned by very high modern authorities. It seems to me, on the other hand, almost self-evident that the converse of the proposition is true, and that the slightest ailment by which health is disordered essentially tends to evil. If no permanent injury is inflicted, it is because the force of ordinary action and the current of habit have been too strong to be obstructed or turned aside by the obstacle. There are two tendencies brought into direct opposition, the natures of whose influence are in positive contrast: the tendency to the normal movement, the function for which each organ of the body is constructed, for which alone indeed it is fitted, and for which it can substitute no other, except in a transient way; and, on the other hand, the tendency of every abnormal or morbid impression to hinder or prevent such movement or function. This is merely a *dynamic* view of the relations of the two, but the contrast is even stronger and more palpable when we examine them *statically*. The materials of which the organs are built, as in all human machines, are fit or unfit for their purposes. If the latter, the mechanism will either act irregularly and improperly, or cease to act. Thus, if sufficient food be not supplied for the nourishment of the body, it will emaciate and wear away; if improper

or deleterious articles be introduced, the structures, the organism, cannot be repaired or renewed, but will be disintegrated. The degree and extent to which these results will be carried, will depend upon the degree and force of the impression made, and of the capacity for resistance, which it meets with in the dynamic mode of disturbance; and in the static, upon the rapidity of abstraction of supply, or upon the mischievous quality of the material introduced into the composition of the fluids and structure of the organs interfered with.

In the world of dead matter, a body put in motion has a tendency to continue that motion, and it will move in a given direction, until it is forcibly stopped by some impediment. The effects of its motion will depend upon its impetus, and the resistance made to its progress by the medium against or through which it is projected. A cannon-ball or shell will crush, and tear, and destroy; a sphere of wood will overturn and indent; a roll of cotton, or wool, or pith, will fall harmless against an ordinary impediment. But it is clear that the tendency in each of these examples is the same.

As to the resistance offered by the living organism, we understand well enough that, as a general rule, it is in proportion to the previous vigor inherent in it. All our hygienic maxims are founded upon this belief; it is confirmed by all our bills of mortality, all our tables of statistics. We have become sadly aware, however, that as there is no density of substance, no panoply of armor that can stop or turn aside a heavy solid-shot within a certain distance, so there is no perfection of health which can escape the overpowering influence of some known poisons, and of many unknown causes of disease. Beyond the extreme cases of slight familiar ailments on the one hand, which do not seem to interfere with a protracted longevity, and the choleraic or typhic or hydrocyanic poison on the other, which ends our life in a second, or an hour, or a day, there are all imaginable grades of impression, and of endurance, or struggle. So general indeed, nay, so nearly universal, are these conditions of protracted endurance or tenacious struggle, that it is not an unreasonable suggestion, on the part

of a recent writer, that we have been in error in taking for granted, that health is the normal and disease the abnormal state of animal existence, or of the human animal at least. We know not much of savage life, but we suppose that the ultimate effect of the perpetual contest with difficulties must be the weeding out of all the inferior order of forms, and the less elastic range of functional capacity. (Darwin's *Struggle for Existence*.) But when we consider the obvious and inevitable influences of domestication, and what we call civilization, we find a mingling of inevitable evil with abundant and admirable benefits; and we cannot help arriving at the conclusion, that as we reject the counteractive advantages of the sifting and cutting-off presented in the original state, we shall find civilization to have become in truth an enlarged study of "Asthenology, or the art of preserving feeble life." It is rare, indeed, to meet with an example of perfect health. In spite of all that we have done or can do, in our best populations, and in communities most remarkable for salubrity, our bills of mortality show a frightful extinction of infant life; scarcely half the number born reaching the end of the second *lustrum*; and those of us who attain the age of fifty, not more than a moiety of the years due to us, according to the calculation of Flourens, on looking around us for our school-fellows and contemporaries, find them, alas! sadly reduced in number—"rari nantes in gurgite vasto."

I have made these preliminary remarks, in place of offering, as is usually done at the commencement of a course of the Theory and Practice of Medicine, definitions, imperfect and inconclusive, of disease in the abstract. Our business is, and always shall be, with diseases, in the concrete and in the plural number. Their name, indeed, is legion. They are effects of causes; and as these causes are appallingly numerous, indefinitely various, and often highly complicated, so must the maladies which they produce be relevantly numerous, varied, and complicated. When we have succeeded in defining life and health, we shall define, clearly, their contrasts and correlatives, disease and death. Where life has been and is extinguished, there is

death. Where there is suffering, whatever be its character, mental or physical, negative or positive, from incapacity or tension, from imbecility or violence, or whatever mode of change or aberration, there is disease—the terms being, indeed, synonymes. At this stage of our inquiries, I would, however, fix attention especially, on the relevancy of all the several forms which diseases assume, to their essential and producing causes; if these are all essentially evil, evil in their tendency and in their effects, I shall claim it as logically established that all diseases are essentially evil in tendency and results, however feeble the tendency may be in certain cases, and however slight and inappreciable the results in many.

It is palpably and undeniably clear in regard to what are called the “specific causes of diseases,” that they are in their own nature injurious; that they exhibit a particular affinity for certain tissues and organs, with whose properties and functional capacities they hold an unintelligible relation, or that they inquninate one or more of the fluids in a peculiar manner; and that each of them has an individuality of impression which separates and distinguishes its influence from every other. This, I say, is palpably clear as to what we call poisons, including among them, a great majority of our most valued medicaments; all the known contagions, and also the so-called miasms which pollute the air, and those atmospheric conditions which give rise to so many endemics and epidemics, and wide-wasting pestilences, whether we regard them as parasitic, animalcular, or fungous, or inorganic effluvia or gaseous productions. In the disquisition as to the incidental sources of so much of our more familiar suffering from alternations of temperature, dietetic errors and excesses, and the like, we are apt to lose sight of the tendency, in dwelling on the easily-avoidable character of the cause, or the transient duration of the effect. Certain coincident circumstances occasionally attend, which confound or mislead us by indirect and compensating benefits, either occurring accidentally or wisely designed, and brought about under the guidance of experienced observation. Here lies the foundation of many of the most cherished beliefs of the

empirical practitioner, many of the traditions of the domestic prescriber, and the self-complacent nurse. A poison which acts promptly as emetic, comes thus to be regarded as no poison, because it occasions its own rejection; indigestible or irritating articles giving rise to diarrhoea, lose their terrors in the fact that they cause themselves to be expelled. The disturbance first excited is forgotten, in the compensative relief ultimately reached. It would be too long a digression here, to show how, upon this hint, grew up our present *materia medica*, with its thrice-blessed accumulation of means of indirect restoration; its paradoxical employment of the most deleterious agents in nature and in art, for sanatory purposes; its allopathic and homœopathic administration of drugs.

I desire here, that it should be noticed that the relevancy which, I contend, must always exist between the cause and the effect, between the disease and the agent which gives rise to it, does not, in cases properly "incidental," depend exclusively, or even prominently, upon the exciting or immediate mode of causation, the abrupt alternation of temperature, the quantity or quality of the food taken; but rather upon that other and very important element of causation, the state, namely, of the constitution, the organism acted on, the *predisposition*, as it is well called. I have said that it is rare to find an example of perfect health; which implies an absolutely exact balance of powers and susceptibilities, a due proportion of vital activity in all the tissues, and unmodified purity of all the fluids. Nay, it tasks our imagination to conceive of the existence of such an example. But if there be any impairment of this complete perfection; if there be something less than the proper vital force in any part; something of disproportion or incongruous material in any organ; something of defect or deterioration in any fluid, we have at once before us an inlet of evil. This "*pars minoris resistentiæ*," to use a phrase of Henlé, is ready to receive a morbid impression; and the nature, the character of this impression, will be determined by the specific function or adaptation of the tissue or fluid affected. Can any one deceive himself as to the tendency of such impression? The animal

economy, admirably arranged as it is, may, and usually will, get the better in the contest; but some power must be expended, and so lost, in such resistance; and the condition of the constitution, that has been dragged through it, will be at a somewhat lower standard than if it had not been subjected to the trial. If the cause ceases to act, the effect may disappear. If morbid material is not longer introduced, and the supply of proper nutriment is resumed, the solids and fluids may regain their original composition and powers of action. The organism is passive in its submission to external influences, both good and evil. The strange error prevails, of supposing that, while diseases are exopathic, or come upon us from without, and can hardly, in any imaginable case, be truly regarded as autopathic or self-originating, there is a self-acting, internal, spontaneous power or faculty of restoration, regeneration, disinfection, so to speak, elastic, and ever active.

We are born victims of evil influences, which have acted upon our parents. We make ourselves victims of such as we voluntarily subject ourselves to, or under the dominion of which circumstances have brought us. We are well or ill, as we have been forced to be by causative agents ever present and inexorable.

Yet, there is no error more universal, none older or more tenacious than the above dogma, assuming so many strange forms, that while the living body is passive and inert as a subject of morbid impressions, all of which assail from without, yet that it is furnished from within with a power of repulsion and readjustment ever prompt and efficient.

It is both interesting and instructive to trace this doctrine through its varied, phases from the earliest records to the present day. Let us select a few of these. Of course we must commence with Hippocrates, whose theory of Medicine, as condensed by his erudite translator, Adams, is "based on the belief of a spiritual essence, diffused through the whole works of creation, which was regarded as the agent that constantly strives to preserve all things in their natural state, and to restore them when preternaturally deranged." This is the prin-

ciple which he called Nature, and which he held to be a *vis medicatrix*, "the physician of diseases." (Vol. III. p. 19.) He taught that "the body is composed of the four elements, from which are formed four cardinal humors; these are liable to change; health consists in their right constitution and adjustment as to quantities; diseases in their impurities and irregularities; the disordered humors undergo spontaneous changes, or what he calls coction, a process requiring time; hence critical days and critical discharges. The primitive disturbance of the humors he ascribes to a great variety of causes, chiefly the influence of surrounding physical circumstances, such as air, water, heat, and cold. Disease continues so long as the humors occupy the body in an unnatural or adulterated state; as they ferment, or undergo coction, various characteristic symptoms occur; and when their elaboration is completed, they are discharged by perspiration, or otherwise. But when such relief is not accomplished, the peccant humors may be localized in some organ;" and the field for the display of the physician's skill is in aiding the discharge or elimination.

Observe here the principle laid down, that disease is from without—health from within—being defended and restored by spontaneous changes, coction, discharge, elimination, under the guardianship of the presiding spiritual essence—the *vis medicatrix naturæ*. I will not dwell upon the *archæus* of Van Helmont, the *anima* of Stahl, or any other of the vague embodiments of the same notion; but, passing over some centuries, bring you down to the speculations of our greatest modern, Cullen. Discarding the superstitions of his predecessors, he reasons from the assumed vital properties and susceptibilities of the living solids, "condemning all notice or introduction of the fluids," which, as you remember, play the principal part in the scheme of Hippocrates, and, indeed, denouncing indignantly "the whole humoral pathology." His method, says Buckle (Vol. II.), is entirely deductive. He assumes the premises, or first principles, and reasons therefrom. His rash and groundless assumptions are most ingeniously wrought out. He assigns the chief place among the vital

functions, to an occult principle—the animal power or energy of the brain: his views are best displayed in his theory of fever. “The cause of all fevers,” he says, “is diminished energy of the brain, which is produced by various sedatives, effluvia, marsh and human, intemperance, fear, and cold. Rapidly passing through the nervous system, its first palpable effect is a cold fit, or chill, which is accompanied by a spasm on the extremities of the arteries. This spasm on the extreme vessels excites the heart and arteries, which excitement continues until the spasm is relaxed; at the same time, the increased action of the heart restores the energy of the brain, the system rallies, the extreme vessels are relieved, while, as a consequence of the whole movement, sweat is excreted, and the fever abates. Such is the nature of the animal economy, that the debility proves an indirect stimulus to the sanguiferous system. With regard to the event of fevers, this is the fundamental principle: nature cures the disease; certain motions tending to death continue the disease; but in consequence of the laws of the animal economy, other motions are excited by those, which have a tendency to remove it.” Observe here, again, that the source of the disease is without—exopathic; the restorative action is internal, automatic, spontaneous; the necessary result of some imagined property of the organism, or law of its being.

In the recent writings of our most scientific authorities, a modified humoral pathology again finds a prominent place. Coction is never, or very rarely, spoken of; but allusions to a ferment, not exactly that of the ancients, but altogether undefined, are permitted, and we have a large class of *zymotic* maladies. Vitiations of the blood and other fluids are freely assumed; acid, or other inquinations of the system are admitted as the probable pathological condition—contagious and infectious miasms, it is not denied, must penetrate the circulating mass, and find their way to the organs and tissues upon which they expend their force. In all the diversity of hypothesis, however, the same course of thought will be found to prevail; the evil agency impresses from without; the resisting and

restorative principle is energetic within. There is a struggle, spontaneous, or resulting from a happy necessity, originally devised and prepared for the emergency. Practically, we have fallen back to the ground occupied by Hippocrates, and regard the office of the physician as simply a ministration in aid of Nature, whatever we understand by that comprehensive word; we wait her movements, her preparatory ferment, her coction and concoction, and then we interfere, merely to assist her in the elimination of the morbid matters which are to be got rid of.

That I do not overstate the predominance of this therapeutical theory of elimination, a few instances will show. Gregory writes strongly of "the mischief which may take place while the scarlatinal poison is in process of concoction, and struggling like a giant in prison, to work its way out." Bennett, boasting of uniform and hitherto unparalleled success in the treatment of pneumonia, administers restoratives chiefly; that is, nutritious food and drinks. He speaks of "crisis by sweat, or stool," which he takes care not to check. "As the period of crisis approaches, he gives a diuretic to favor excretion of urates." Another writer, more explicit in his speculations, maintains "that pneumonia is the result of the presence in the blood of a poison which nature seeks to get rid of, through elimination effected by means of the inflammatory exudation which fills the air cells." Rokitanski represented the typhosis deposit in Peyer's patches, in enteric fever, as a mode of eliminating the typhous poison. Simon speaks even of cancerous tumor and ulceration as "an eliminative effort of nature." "It is highly probable," says Aitken, "that malignant tumors eliminate something specific." Todd suggests of epilepsy a cause in renal inaction, and seems to consider the convulsive paroxysm as a sort of eliminative explosion. Prof. George Johnson has convinced many of our brethren, that cholera depends upon a special poison in the blood, which seeks an outlet, and escapes by a gastro-intestinal excretion; and that the choleraic vomiting and dejections are eliminative and salutary, and ought to be encouraged, not repressed. Aitken recommends the promo-

tion of the sour, offensive sweats in rheumatism on the same principle—that thus the lactic acid poison is eliminated and removed; and others apply, with this purpose, repeated blisters to the diseased parts to give this relief locally. “The *materies morbi* is obviously got rid of by sweating, and the natural cure of the disease is effected by these profuse sour-smelling perspirations.” Somewhat inconsistently, he continues, “Whatever, therefore, the poison may be, which induces the rheumatic state, it is one which appears to be generated within the system, and not absorbed from without. It is inbred, and not derived from extrinsic sources. The researches of Dr. Parkes lead to the belief that it is some substance rich in sulphur.” But *unde derivatur* the sulphur?

A very remarkable exemplification of these views is found in the recent work of Dr. Rennie, Staff-Surgeon of the British army in China, published in 1865. He mentions a form of pestilential fever, which he met with in the neighborhood of Peking; he calls it “spotted fever”—“a contagious fever, not dangerous, if the eruption comes out freely”—an old notion prevalent with practical results before the time of Sydenham. And afterwards, relating a case of small-pox in a child three years old, he tells us that he “explained to his father that the most prudent course was to follow the dictates of nature, and to encourage as much as possible the process which she had commenced, by throwing the disease out on the surface of the body, and thereby save mischief being done to some internal organ by a part of the poison remaining unexpelled. With this view I provided him with means of furthering the development of the small-pox eruption on portions of the skin on which it had not appeared. The treatment involved nothing more complicated than the application, by friction, of a little croton oil to the parts free from the eruption.” He alludes more than once complacently to this his mode of converting distinct into confluent small-pox—a therapeutic as strange and original as the ancient practices based on similar notions are, happily, obsolete.

Yet, after all, perhaps the most emphatic utterance of this

paradoxical opinion concerning the utility, benefit, the restorative, and, in a general sense, salutary influence of disease, or diseases, is found in the recent writings of the distinguished Dr. John Brown, of Edinburgh. He is speaking of his father, who, after a long course of usefulness and honor as a clergyman, died at a ripe old age. "I have said, that though delicate, he was never ill; this was all the worse for him, for, odd as it may seem, many a man's life is lengthened by a sharp illness, and this in many ways. In the first place, he is laid up out of the reach of all external mischief and exertion; he is like a ship put in dock for repairs; time is gained; a brisk fever clarifies the entire man; if it is beaten, and does not beat, it is like cleaning a chimney by setting it on fire; it is perilous, but thorough. Then, the effort to throw off the disease often quickens and purifies and corroborates the central powers of life; the flame burns more clearly; there is a cleanness, so to speak, about all the wheels of life."

These hypothetical speculations, abstract as they may seem, thus bear immediate fruit in practice, and suggest two contrasted methods of treatment of diseases. Some regard this *vis medicatrix*, this *archæus*, this defensive and restorative power, as all-sufficient for its beneficent purposes, and decline, therefore, to interfere impertinently and uselessly in the contest, beyond the mere avoidance of all *lædencia*, and the careful institution and observance of proper hygienic regulations. This is the "*medicine expectante*," which has always included many of the sagest minds of the profession; being, however, very seldom followed out with logical completeness, almost every member of this school entertaining a greater or less degree of favoritism for certain agents which he regards as safe from injurious liability, and capable of aiding Nature in her efforts. Others, not admitting the omnipotence of this preservative energy, look upon the office of the physician as though not altogether negative, yet, at best, merely protective, secondary, and adjuvant. Of the wisdom of nature they entertain no doubt; and are thus led to esteem all the phenomena of disease as manifestations of the true method of re-

lief and recovery—indications of the course we are to follow. Thus arose Homœopathy, though all who maintain these doctrines do not allow themselves to be called homœopathists. G. Johnson gives purgatives and emetics in cholera, and Rennie solicits more diffused eruption in small-pox less than confluent. I am far from doing such men the injustice of confounding them with those who, professing to hold that *similia similibus curantur*, indulge their imaginations in ideal analogies, and dupe themselves or their followers with the absurdities of infinitesimal prescription. I have much respect for many of those who, with Bennett, wait for crises, and do not oppose, but timidly promote them; or, with Todd and Chambers, support the burdened constitution with tonics and stimulants, under the modest notion that, as they do not clearly see what Nature is doing, they must only seek to strengthen her blindly for her work.

I believe, and have endeavored to maintain—

A. That disease—never a salutary process—is essentially evil, and always tends to evil.

B. That spontaneous recoveries take place, not because the diseases cure themselves, but because they fail to kill.

C. That there exists in the organism no special or separate power of restoration, under whatever name. Life itself is the *vis medicatrix*, and nutrition the mode of its action.

D. That the elimination of poisons, or of diseased products from the system, is not effected by the diseases they excite and attend on.

E. That such elimination, whenever it occurs, is effected by, and is proportioned to, the vigor of the remaining healthy functions, and not to the extent of diseased actions.

F. That true therapeutics consist, in our present ignorance, chiefly in fostering the *residual* powers of natural action and function, and

G. That our hopes point to antidotal means of arresting, correcting, and counteracting the causes.

Disease is not a unit. Diseases vary infinitely, and require to be studied in their individuality. I shall not hesitate to

speak of them, whenever it suits my convenience, as entities—not mere conditions, but real things. The imputation of the error, or weakness of “Ontology”—almost a crime in the eyes of some of our medical philosophers—has no terrors for me. I fully agree with Mansell, that “ontology is but a higher kind of phenomenology; the object of any consciousness being not necessarily a thing in itself, but a thing as we are compelled to conceive it.” When I speak of small-pox, I am understood at once, whether he who hears me fastidiously resolves the term into an expression of numerous concurrent morbid conditions, or cuts a “gordian knot,” very hard to untie, by accepting the word as a brief definition of an actual object of thought. Of fever in the abstract, we may acknowledge that it is very difficult to form a conception; but when I refer to intermittent, or typhus, or yellow fever, or hectic, no one is at a loss to know my meaning. I am not to be silenced by the taunt of uncertainty and vagueness of definition, or differences of opinion among ourselves. The same difficulties are presented when we deal with the coarsest and simplest material objects. We all think we know what *coal* is, for instance; but it is not long since, in the course of a law trial in Edinburgh, so many experts were brought up, who swore as to the same masses, that they were, and that they were not coal, that the court and jury become absolutely mystified, and the case had to be decided upon other than scientific grounds. (*V. London Quart.*, April, 1866.) When we use technical phrases in pathology, that are not definite or closely significant, such as tone, spasm, irritation, or the like, our meaning is vague, because our ideas are uncertain, or confused. The wise poet, Tegner, chants in his Northern Saga—

“Thou knowst not what thou canst not clearly say;
Man’s lips give birth to words and thoughts together—
The obscurely uttered is the obscurely thought.”

Diseases are relevant to their causes, which differ infinitely. Their phenomena, or symptoms, as we call them, must differ also with the structure and function of parts affected. The

most familiar diseases, both local and general, present us with obvious examples. Inflammation is not the same when caused by fire, or by small-pox poison, or a blow; nor is it identical when affecting different organs and tissues—the mucous and serous, the areolar and glandular, croup and pleuritis, variola and erysipelas. Typhosis and carcinoma are essentially variant; but in every instance, so far as it goes, and in whatever mode it may act, the cause of disease—the remote cause—is harmful in its energy, its influence, its course; the proximate cause, the immediate morbid result—a departure from the normal action, or condition, a perversion, an effect becomes, in its turn, a source of a series of evils, transient, if the original impression is transient (*causa sublata tollitur effectus*); and if it has given rise to no permanent change it is limited in degree and extent proportionally to the degree and extent of impressive force. This may be so violent as to be fatal at once, as a stroke of lightning, a drop of prussic acid; somewhat less so, as the choleraic poison, which kills in an hour; insolation, fatal in a few hours; malarial congestion, or typhus infection, which brings on death in a day, or a few days: or, it may be so potent in extent, as to subvert the whole of the original forces of supply and renovation, as in some of the cachexias and constitutional inquinations, leprous, scorbutic, strumous, syphilitic, carcinomatous. I need not formally lay down the proposition, that all these evil agencies impress the system from without. This is obvious, as I name them. Life itself, many physiologists say, is a forced state, dependent entirely upon the continued application of *stimuli*. Whether this be true or not in the abstract, or philosophical sense, which I am disposed to doubt, we cannot help admitting it to be practically true in the most absolute degree. All the manifestations of life will cease with wonderful promptness, when these *stimuli* are withheld, and in different degrees of rapidity, in proportion to the unknown variations of instant indispensableness. Thus, death from want of air, will take place in a few seconds. No diver can immerse himself more than two minutes, or two and a half; and burking, garrotting, and other

modes of suffocation, extinguish life rapidly and easily. Want of fluids will destroy a man in a few days; he will survive the want of food for two weeks, perhaps. His existence will be feeble without light; and a temperature around him which subtracts heat more rapidly than his food and digestion generate it, will soon freeze him to death. Nutrition, in this wide and general sense, is the only restorative power which I recognize; the only means of "renewal of life;" the exclusive *vis medicatrix*. Thus, he depends for existence and *à fortiori* for health, upon a due supply of all these necessities of life; and if they are supplied in an insufficient quantity, or adulterated condition, he will enjoy a life and health below the perfect standard. If the adulteration be of positively and impressively injurious character of poisonous quality as we express it, a direct and immediate abatement of vigor, an immediate arrest of living action, an absolute extinction of vital power may take place, instantaneous or progressive; or an ingraftation or implantation of productive germs, which shall supersede the original molecular or requisite atomic material of growth and supply.

It would be absurd to deny—it is not my purpose to suggest a doubt—that in many instances, all the disturbances which arise in these several modes, will subside and disappear without our interference. But I hold that it is of great importance to understand how this happens, and I propose to enter into the inquiry with entire impartiality. I am prepared to maintain most distinctly, that we shall not find this restoration to health, however it is to be accounted for, to depend upon or arise from the processes in which disease consists; most assuredly not by elimination or extrusion of any poisons—by actions excited by those poisons. "*Abominandum remedii genus debere salutem morbo*," is a line of Publius Syrus, well applied to describe this strange pathology, which regards disease as its own cure. Vomiting,—a familiar example,—like sneezing and coughing, is an action partaking both of pathological and physiological character. It is the result of any mode of irritation affecting the special nerves or certain sur-

faces, and this irritation may be mechanical or static, dynamic or sympathetic. It is not only the specific effect of some specific poisons, but an incidental consequence of the impression of a great variety of articles of food and drink. For the few instances in which it may be adduced as relieving the system of disease or of the cause which gave rise to it, there are thousands in which it is only evil, tending, as in yellow and remittent fevers, and cholera, and gastritis, and sea-sickness, to increase the irritation on which it depends. The same observations may be made in regard to diarrhoea, so confidently referred to as proving the truth of the same doctrine. Many modes of irritation excite in the intestines the instinctive effort at defecation, which was originally purposed to get rid of the *débris* of food, and probably certain hepatic and other *excreta*. This instinct, inordinately aroused and perverted, becomes tenesmus, which is augmented by its own indulgence, as in dysentery and typhoid fever. It is thus often a source of fatal disturbance and exhaustion.

Diseases are fatal in many ways; in other words, there are many modes of death, which pathologists have arranged and classified according to their several views. Death may first be considered as positive and negative, but the distinction is by no means uniformly easy or clear. The promptest mode of negative death is *apnœa*, as it is styled by Williams,—deprivation of air—suffocation. This requires a very brief interval—two minutes' immersion, or burking, or hanging, or choking, will probably suffice. I do not now speak of the molecular death, which is rather a chemical change of condition, than properly physiological; but the somatic, by which is intended that contrasted state, the true opposite of life, in which the organism has lost, or been deprived of its susceptibility of being impressed by, or reacting upon, the agents with which it is in normal affinity or harmony. The heart, the brain, the lung, may be wounded grievously, and the subject may continue to live; but if you touch the *nœud vital* of Fleurens, you can scarcely remove the instrument you employ before life shall be altogether extinct. A fowl decapitated, will flutter about for

some minutes; its eyes and bill will open and shut; but I saw one gaffed in the neck, at the junction of the head, succumb and collapse, without the quivering of a feather. An animal falls stunned by a merciful blow of a mallet, who would, if pithed, suffer intense pain, or rigid spasm; or gasp with prolonged depression, or vehement convulsion, if bled to death. More prompt, even, than suffocation, is death from concentrated hydrocyanic acid applied to the tongue or *conjunctiva* of the eye—a positive death, the most instantaneous; approaching it, is the death from a large amount of alcohol taken into the stomach, which acts like a sudden, violent blow on the *epigastrium*. Next, perhaps, is the death from the inhalation of chloroform. This appears to me to be clearly positive, and not an *apnoea* merely, as some regard it. A man who is choked, or half drowned, and relieved, is himself at once again. Chloroform is, in a marked degree, and for a considerable time, depressing, as I have often experienced; and it is in this way that it kills. It is always dangerous, unless when counteracted by pain, or by the condition of which pain is the manifestation. None die of it, except those who take it as a mere preventive, while free from pain. Ether, which displaces as much, if not more respirable air, does not kill.

The poison of cholera may, perhaps, be mentioned next in order. I need not say that we know nothing of its nature, but by its effects; and it is probable that some allowance must be made for the natural liability to exaggeration in the received statements. But, with all this allowance, it must be regarded as singularly prompt, when concentrated. The Pindarries, or grass-cutters of India, have been found lying upon little heaps of fresh-cut grass, under circumstances which rendered it probable that they must have been ill but a few minutes. Sir Stamford Raffles speaks of terminations within half an hour, as not uncommon in cases in Batavia. And all must remember Magendie's famous phrase concerning it, that "it commenced where all other diseases end—in the death and cadaverization of the patient." Nelson, of New York, denies that cholera is a disease, insisting that it shall be regarded exclusively and

directly as a special mode of poisoning, as the deaths from arsenic, strychnine, prussic acid, and opium are regarded.

Spotted fever, in its recent visitations, exhibits the effect of a typhic poison very rapid in its influence. A physician, who saw much of it, told me that a considerable number of his patients died within seven hours from the invasion of the attack; it is true that there may have been a more gradually progressing influence exerted latently. I am inclined, however, to admit that a fair calculation may commence at the first moment of shock, or complaint of suffering. Up to that time causation has not begun—has been suspended—the relation between the amount or force of the poison and the predisposition, or the contrasted state of resistance or insusceptibility, has been unchanged. Thus, a little boy was seen by Drs. Morrison and Lodge eating, in apparent good health, and with appetite, less than two hours before his death—spotted. Malaria sometimes, capriciously as it seems to us in our dark ignorance, strikes a deadly and direct blow at the sensorial and circulatory system, stunning, with what we call congestion, algidity, pernicious force, which cannot be resisted or endured; and life goes out hastily with swift and steady progress, in a few hours from the moment of attack. Yellow fever is seldom quite so prompt; yet often kills in three, four, or five brief days. In these deaths, the body, to use the language of John Hunter, has, in typical cases, “all the composition it ever had.” No organ has undergone such perceptible structural change, as to unfit it for its function; no function has been obstructed in any palpable way; and thus, when we are discussing diseases *seriatim*, we shall frequently be obliged to confess that the cause of death, whether in particular individuals, or again in whole classes, is unknown and undetected. In the progress of our science, aided, as it always is, by improvement in all other sciences and all other arts, we hope that we shall acquire more and more knowledge of this sort; diminish the realm of the *quid ignotum*, and widen the small circle of light, surrounded by the vast impenetrable obscure which encloses and oppresses us.

But already we are occasionally able to see why a patient died, although we cannot detect the relation between a disease or its cause, and the fatal event; for that may often be inscrutable, even when the special condition of the individual shall explain why it has become inevitable. To illustrate my meaning: When we find a perforation opening together the bowel and the peritoneal sac, which has taken place during apparent convalescence from typhoid fever, we know the immediate cause of death; but we have not prevailed to trace the links of the chain of causes and effects which connect that fact with the history and pathology of the case, either as a typical or exceptional one. Why was it typhoid, or abdominal, and not typhus, or cerebral? Why was the mucous surface of the intestine affected? Was there any typhous deposit? Was it a pathological accident merely, or a normal physiological effort to eliminate a poison? Let us exclaim with Laplace—"Our knowledge is a little matter; our ignorance is immense."

Upon careful reflection, we shall thus be led to comprehend the true value of morbid or pathological anatomy. 1. It will manifest to us the immediate cause of death, as in the example just given. We must make distinctions here, too. These manifestations may be rare, or merely accidental, as when we find *emboli*—clots—in the heart, the large vessels, the vessels of the brain, or hemorrhages, as in the cerebral tissue. I recollect an instance, in which a patient dying suddenly in typhoid fever with coma, presented a large clot at the base of the brain. 2. It will show us lesions as usually occurring, but not to be explained, and not accounting for death, as in yellow fever, where we have, in most cases, fatty liver and fatty heart. Bell tells us that he often met with tubercular deposit in the typhoid of Glasgow. 3. It will offer us products of diseased action, whose presence is coincident with fatal results, but which cannot be received as causative of them; thus, the melanotic exudation, "black vomit," in yellow fever, and the sordes of the typhic class; pigmentary change in the liver of bilious remittent; intussusception in children, loose

and harmless, as well as inflammatory and hernial. 4. We shall derive satisfactory information as to the pathology of certain obscure cases, as in the several varieties of Bright's disease, or acute and chronic nephritis, where the changes of the structure of the great excretory organs prevent the elimination of urea, and other effete matters which, when retained, act as virulent poisons.

We shall further apprehend the modes and causes of death, by careful observation of the contingencies observable, whether during life, or in the act of dying. Death may be brought on by the mechanical interference of the product of diseased action with the vital functions; in croup, by the closure of the larynx or glottis, as by membrane, swelling, or œdema; oppression from serous exudation, as in hydrothorax; or purulent, as in empyema; or gaseous, as in pneumothorax and in tympanitis. Pneumatosis may destroy life by mere distension, as in some colics, where the tone of the bowels is lost with their expulsive power, and the abdominal, pelvic, and even the thoracic *viscera*, are rendered incapable of function—intussusception takes place, or rupture or laceration of ulcerated or softened portions of the tissues. Purulent effusion in the lung or the brain, may thus be fatal, or exudation of serum in the ventricles.

Insolation may terminate life promptly, by engorgement of the lung, as Russell and Dowler have proved, and by similar engorgement *quasi* apoplectic of the brain, as I have seen. Thus, malarial pulmonary congestion suffocates. More slowly we witness death, sometimes from the hæmal infiltration of pneumonia, or the suppuration which fills the air-cells and tubes, and still more gradually from the numerous maladies which interfere with the great functions of digestion, assimilation, and nutrition, and sanguification. Many of these are purely dynamic, and exhibit no statical or organic change in the most minute inspection of the parts affected—the stomach, the pancreas, liver, and spleen.

I need not dwell on death from exhaustion, as in the hemorrhages and profluviae in albuminuria, diabetes, diarrhoea, etc.

But we must, for a moment, consider the modes of death from actual or positive poisoning. This event results from retention of injurious effete matters; or introduction of malignant agents from without; or the new production of such viruses or evil and destructive material, through the special action of diseases themselves. Of the first, I need only observe, that there is a perpetual metamorphosis of tissue going on in the living body, most active where there is most life, atoms becoming transformed or transubstantiated, from dead to living, to supply the place of atoms used, which, like all earthly things, "perish in the using." These last find natural outlets, always open and flowing in health; obstructed or closed in disease. Such obstruction, indeed, constitutes some of the most familiar among the symptoms of disease, and the retention in the body—whether in the solids, or the circulating, or secreted fluids—of such effete, dead, decaying matters must be dangerous, and if continued, fatal. Even the materials of nutrition, as Paget reminds us, must, if they are not deposited when and where they are due, in a certain sense, inqurate the blood, and load it hurtfully. This mode of non-elimination, however,—this failure of appropriation,—cannot be very directly or uniformly mischievous. If it were, we should find it difficult to account for the good health of so many persons truncated by amputation of one or more limbs. In these, the unused, and now surplus material of supply, must find ready removal through the ordinary excretories. Henlé accounts for the frequent conversion of ordinary into malignant forms of disease by the accumulation of effete matter in the system, from failure of excretion; and, indeed, it seems strange that this does not happen far more frequently.

Of the intrusion of poisons from without into the organism, we need only cite the instances of malaria, or atmospheric miasms of infinite diversity, and in a special way, of contagions. How these are introduced it is not always easy to say; some can be inoculated, others are probably taken by the lung and the skin. The stomach may admit many mingled with saliva, with the food, and in our drinks.

In the discussion of the third method of poisoning, we plunge at once into the bitter waters of controversy. No positively new formation is admitted to take place, under any imaginable conditions, by a large school of medical theorists. Virchow lays down, as an inexorable axiom, *omnis cellula e cellula*. All differences, however marked, are resolved into mere variations of plus and minus. A cancer cell is only distinguished from a pus cell, or a tubercle, by its higher development of comparatively gigantic corpuscles, nuclei, and nucleoli; tubercle being a diminutive granule, with little cells, and a pus-corpuscle, only discernible from a colorless blood-corpuscle by its origin: "If you do not know whence it has come, you cannot say what it is." (P. 155.) We must not be deceived by these resemblances, which cover, to our imperfect capacities of perception, immense intervals of the most positive difference. The early germs, the ova of many animals, especially those of the same species or genus, are not to be distinguished from each other. A little oil in a pellicle of albumen—this is all. "If you do not know whence it has come, you cannot say what it is," in the phrase of Virchow. But if we give full play to the principles of differentiation existing within them, one will develop into a whale, another into an elephant, a third into a mouse, a fourth into a man. So it happens, in the progressive stages of such maladies as are marked by material products—organic changes of palpable structure and composition. However the diseased movements originate, whatever may be the primary perversion and derangement, nothing is more certain than that, in their course, they generate out of the elements which, so to speak, they manipulate, new and special creations, which arise independently of existing substances, and are possessed of entirely novel properties. In the pus of a small-pox vesicle, and on the surface of a chancre, there are formed new atoms, not to be detected by any methods of examination yet known, which have been elaborated within the body, and which are capable of impressing other bodies in a peculiar and essentially exclusive way. So it is with all the contagions, equally and

alike. I have mentioned two, both of which may be imagined to be cellular in form, and derivable as cells from cells.

But there are many that exhibit very different conditions; small-pox in atmospheric diffusion, scarlatina, mumps, whooping-cough, measles, present no visible *materies morbi*; and how shall we account for the spontaneous origin of cancer? Paget says of it: "A cancer is, from the first, both a constitutional and a specific disease. I believe it to be constitutional, in the sense of having its origin and chief support in the blood; and I believe it to be specific: 1st, In the sense of its being dependent on some specific material which is different from all the natural constituents of the body, and from all the materials formed in other processes of disease; and 2d, In the sense of its presenting structures which are specific, or peculiar, both in their form and in their mode of life."

These reflections upon the several modes of disorganization and death, as shown in the progress of disease, in the act of dying, whether prompt or gradual, and in the lesions discoverable in the dead body, lead us to the most important practical and therapeutical results. They constitute, by instructing us in the disorganizing changes which belong to each of the varied forms of disease, our best guides in the treatment of these maladies. They establish our true indications, and point out our clearest and most urgent professional duty—to ascertain, as far as possible, in every instance, and having ascertained, to set ourselves to "obviate the tendency to death" most prominent and menacing. Thus, in the examination of our patients, we shall not merely look to the general diagnosis, but we shall not be satisfied until we have set apart every individual case, and have discovered its special peculiarities; informing ourselves of all the weak and strong points in the constitution, guarding the one, and building upon the other—surveying the whole ground, and laying out a correct chart by which we shall be hopefully and rationally, safely and successfully guided,—if safety and success be by any possible effort attainable.

THE CAUSATION OF DISEASES.

DISEASES are effects of agents which have impressed the system inharmoniously, and therefore morbidly. The result sometimes depends on mere excess; sometimes on abruptness of transition; sometimes on conditions of transient or permanent unadaptedness. Thus, there is but a certain quantity of gastric juice secreted; the stomach can endure only a certain degree of repletion or mechanical distension; therefore, if filled too full, or loaded with too much of the best food, it must suffer. So, also, abrupt changes of temperature, or of habits, or of diet, will do injury. We are apt to lose sight of the fact, that the functions must be in accord with the situation—the surroundings; we cannot respire comfortably on a high mountain—we cough, pant, spit blood; sudden heat distends the vessels dangerously, insolation follows, and apoplexy; the fluids secreted in the stomach of an Esquimaux, for the solution of his accustomed seal's flesh and blubber, will act imperfectly upon the rice of the Hindoo, or the dates and gum of the Arab. When fatigued, we must rest before our appetite or assimilative powers are fitted for indulgence in a full meal, and no one unaccustomed to sup late at night can safely eat largely just before going to bed.

I do not propose to enter here into the discussion of these incidental causes of disease. We shall find in specific causes—poisons—a most interesting and fruitful topic; and I shall treat at present of those which are known to affect numbers at once, rather than confine myself within the limits of toxicology proper. History abounds with records of ghastly and wide-wasting epidemics. In all ages, men have been found

liable to loathsome forms of death, carrying off promptly whole armies, and depopulating fair and flourishing cities. The wrath of Apollo decimated the ranks of the Greeks in the war of Troy, and the story of the pestilence which scourged Athens is familiar to every school-boy. The "Black Death," the "Sweating Sickness," and the great plagues of Florence and London, need only be alluded to. These maladies are known in Europe no more, and have never reached America. They are assumed, in a general way, to have arisen from the defects of hygiene in those times, aided by the presence of some infectious or contagious pollution. Such, at least, is the almost universal belief. When we know, with precision, how such terrible disorders are generated, we shall learn why they have died out or disappeared. There are in London, and all other large towns, at this day, purlieus as vile and filthy as the worst dens of pauperism in the middle ages; and, happy as we are, that we have not these products of pollution to contend with now, we have in their stead, others little less to be dreaded: Typhic fevers, spotted and putrid, small-pox and scarlatina, diphtheria, yellow-fever, and cholera. Progressive civilization has had, doubtless, a strong influence both on the production and extinction of diseases; but it contains so many elements, that it is difficult or impossible to point out which of them is to be considered the efficient excitor—the *causa causans*—in any given instance, or the beneficent exterminator in any other. Leprosy, still permanent in the unchanging life of the Easterns, pervaded all Europe but a few centuries ago, to an extent that seems to have been overlooked by modern medical writers. It was, and is still, an incurable malady; it was universally regarded as contagious; it was believed to be hereditary. Yet, it has gradually diminished, and died out so completely, that it is now scarcely ever seen in private practice, or in any of our hospitals. It is hardly credible, that in the fourteenth century, there were about twenty thousand leproseries in Europe—more than two thousand in France alone—asylums for the wretched lepers; one for the *noblesse* in Dauphiny, and one in the environs of

Paris, exclusively for females of royal houses, "*pour les femmes des maisons royales*," the exact phrase of Monteil, in his "*Histoire des Français des Divers Etats*." And one of the large old churches in London was built by the Queen of Henry II., as an "asylum for lepers." But there are now neither patients, thank Heaven, nor hospitals of this kind.

The occupations of civilized life afford us examples, on the other hand, of the creation or origination of diseases, as well as of their abolition. The one often follows the other, with gratifying readiness, but not always. Lead, as used in the arts, gives rise to severe, and sometimes fatal ailments; but we have learned to evade these by tinning our pipes, and in manufactories by the employment of sulphuric acid. Phosphorus matches, the invention which, more than any other, separates and distinguishes the cultivated from the savage man, which realizes the old legend of Prometheus, and endows us with control over the most beautiful, useful, and necessary existences in the creation, Light and Heat, brought on, in their preparation a frightful caries of the mouth and face, now almost unknown. The injurious influence becoming obvious in its nature, was scientifically and thoroughly corrected after a brief course. Mining, whether for coal or ore, does not exhibit in our new country, and in the conditions of progress active here, the deleterious effects upon health and longevity connected with it everywhere in Europe. The wet and dry grinders in England have long been doomed to premature old age and early death. "A grinder," says Chambers, "sits on a block of wood, which he calls his 'grinding horse,' and his grindstone is before him, turned on an axle by steam or water. To this, he applies the article to be ground, and a spray of fire arises at every touch. But the fire is not the worst. The stone itself wears away in foam-like surges, that fill the lungs, and in a certain number of years, calculated by statistics to a nicety, kills the principle of life. A dry-grinder does not reach thirty-five; a wet-grinder may defy death for nearly ten years more. The men themselves—and they number between 2000 and 3000 in Sheffield alone—like their fate

rather than otherwise—insist on their trade retaining its fatal noxiousness ; because, if this were removed, there would be a greater competition of hands, their high wages would come down, and their deep drinking be cut short.” As Elliott has it, in his “ Corn-Law Rhymes ”—

“ There draws the grinder his laborious breath,
There, coughing, at his deadly trade he bends ;
Born to die young, he fears nor man nor death ;
Despair and riot are his bosom friends.
Bid science on his cheek prolong the bloom !
He will not live—he seems in haste to gain
The undisturbed asylum of the tomb ;
And, old at two-and-thirty, meets his doom.”

But, in spite of his insane resistance to corrective measures, science has reduced, very greatly, his peculiar risks and liabilities, and indeed, among us, may be said to have obliterated them by improved mechanical processes.

Scurvy, once a generally prevailing condition in all dense populations, has been almost done away with, by the varied diet of vegetable and fresher animal food, attainable under our better husbandry, and is now seldom seen, except on board ship in long voyages, in ill-supplied armies, and ill-managed prisons and poor-houses.

A constantly-increasing attention to cleanliness and proper clothing, in addition to the better food just spoken of, have done much to mitigate the prevalence of scrofulous and cutaneous affections. I wish I could say the same of many con-nate maladies, depending on the nearly universal neglect of ventilation in our domestic architecture. Not to speak of the hovels of the old world peasantry, or the crowded dens in cities, take from the *Atlantic Monthly* for July, 1866, the following recent description of the “ Homes of New England : ” “ In the winter, the windows are caulked up and listed, the throat of the chimney is built up with a tight brick wall, and a close stove is introduced to help burn out the vitality of the air. In a sitting-room like this, from five to ten persons will spend

about eight months of every year, with no other ventilation than that gained by opening casually and shutting doors. A late writer raises the question, whether the community would not gain in health by the demolition of all dwelling-houses. Through the Valley of the Connecticut, the bedchamber will be the concentration of all forms of bad air. The house is redolent of the vegetables in the cellar, and this fragrance is confined and retained by the custom of closing the window-blinds, and dropping the inside curtains, so that neither air nor sunshine enters in to purify. Thrifty country house-keepers, often adopt the habit of making their beds on the instant after they are left, without airing the sheets and mattresses; and a bed so made, gradually becomes permeated with the emanations of the human body, so as to be a steady corrupter of the atmosphere."

Collate the above with what is said by Bartlett, when discussing the continued fevers of our country, contrasting typhoid with true typhus, and insisting strongly upon the error of ascribing the two to similar modes of causation, and on the inefficacy of hygienic measures, to prevent the former: "Do my friends believe that any cleanliness, any ventilation will avail here? Let them try it when typhoid fever prevails, as it so often does, and so extensively, during the beautiful and breezy Indian summer, amongst the most cleanly, the most temperate, the best clad, the best fed, and the best sheltered people that this world has ever seen—the rural population of the Eastern States." (P. 214. *Note*.)

It is certain, that mere domestication, a word which implies warmth and shelter, as well as the confinement within warm and sheltering buildings, is unfriendly to all wild animals. The caged lion, tiger, and monkey, are miserable, scrofulous, or tubercular, and short-lived; the horse and the cow, and even the sheep, have become delicate and sickly, and liable to destructive epizootics, the murrain, rinderpest, pneumonia, malignant pustule, and glanders. Even the hardy wild-boar is so deteriorated in constitution by the artificial modes of living, and breeding, and feeding, that his descendant races are

now very widely regarded with distrust and suspicion, as proverbially strumous; and of late, still more emphatically, as the chief focus of parasitic intrusion, as in trichiniasis.

Tuberculosis is unquestionably spreading its gloomy sway over the highest types and conditions of our modern civilization; the ravages of phthisis are annually extending more and more widely, and occupying always, and everywhere, the highest place in our bills of mortality.

When we take up the study of endemics, or local diseases, we shall also find it difficult to ascribe the proper share in their production to the several elements present and influential; the social state already treated of, with all its involved modes of agency; the peculiarities of tribe or race, and the topographic or climatic circumstances. Why should the Pole alone be subjected to plica? There is no observable peculiarity in his scalp, or its hairy covering. If he is filthy in his habits, so are the poorer classes of many tribes and nations, savage and cultivated. Why should the Lombard suffer exclusively from pellagra? Maize, used for food by the people, has been suggested as the source of that terrible disease which, if not exclusively confined to Northern Italy, is scarcely known elsewhere. Maize is eaten as abundantly by many other populations—in all its varied qualities has certainly long formed as large a proportion of the diet of our Indians, and of the white men who have taken their places on the earth's surface. Hameau suggests that we get it, somehow, from sheep; but sheep exist everywhere.

Why is yellow-fever the constant resident of certain situations—and why, under contingencies apparently similar, is it in certain other places altogether unknown? In the Gulf of Mexico, and the islands of the Caribbean sea, it does not fail to show itself during the hot season; on some portions of the African shores, while the natives seem exempt, it is always ready to assail an exposed visitor. It has never been met with in Asia, and rarely visits Europe. Familiar on our Atlantic coast, the cities of the Pacific seem almost free from it.

It is easy to mention numerous instances of still more

remarkable narrowness of domain, occupied by particular maladies. The "Aleppo Evil"—"Aleppo Boil"—"*Bouton d'Alep*," is one of these. Unheard of, except in that immediate region, on the banks of the Coik, the Tigris, the Euphrates, at Moussoul, Diarbekr, Bagdad,—its centre is the city whence it derives its name. "The natives of Aleppo," says Willemin, "are universally subject to it. It develops itself in early life, generally during the three first years. Strangers sometimes escape it, even after long residence, but some are attacked after a very short stay." Such was the fact with the celebrated female traveller, Madam Pfeiffer. It may show itself in one month, or not until some years—even after removing from the dangerous region. It mostly attacks the face; there may be but one button or tubercle, or many; it often lasts a year.

Equally obscure and undetected are the local causes which produce the gangrene of the rectum in Brazil—the Barbadoes leg and the radesyge of Norway. Boudin furnishes us, in his valuable work on Medical Geography and Statistics, with many other similar instances, in which some local peculiarity, whether of air, water, or diet, hitherto undetected, inflicts its evil impression—more on the indigenous population, yet not altogether sparing the occasional visitor or temporary resident.

Of still more general interest, are those maladies, which, while specially prevalent in one region, are traceable in other localities presenting similar topographical contingencies. Of this sort, we may mention goitre—bronchocele, with its attendant cretinism. Terribly frequent in the Alpine valleys, it is also familiar among the Pyrennean Cagots, and may be met occasionally in all parts of the world—even where it would be difficult to detect any of the circumstances to which it is ascribed as an endemic. McGowan, in a paper read not long since to the American Ethnological Society, says that he finds it prevalent on the Juniata, and in the lateral valleys of the Susquehanna, and on the acclivities of the Blue Ridge in Virginia—mostly restricted there to negroes and mulattoes. Dr. Merrick spoke of its prevalence in Panama. Mr. Squier

had observed it in Peru and Nicaragua. S. Navarro, Mexican Consul-General, stated that in certain portions of Mexico, goitre and cretinism prevailed, both among Indians and Spaniards. Our own Indians are exempt from it—McGowan thinks from their migratory habits. Sporadic cases are met with everywhere, however, as if spontaneous, and from unknown causes. Snow-water, lime-water, want of ventilation, as in pent-up valleys, monotony of diet—all these are assigned as producing it.

Is the question absolutely settled yet, when and where began the scourge of the venereal pollution? Was it American in origin, and unknown in Europe until after the voyage of Columbus; or a new development in Europe, at about that time; or an old European disease, suddenly aggravated into a wide explosion—or, as some have ingeniously suggested, the result of promiscuous indulgence of two races, uncongenial in constitution and habits? I will not pronounce here any dogmatical decision; nay, I do not hesitate to say that a ready debater might accept and maintain either of these views, with very plausible tenacity.

When and where, too, did small-pox first show itself, or scarlatina, or measles, or whooping-cough? If any of these seem to originate now, without obvious source, the fact is looked upon as curious and unintelligible; nay, we would be as likely to expect the advent of a new animal, as either of them, notwithstanding the speculations so often indulged in, which we shall hereafter take into consideration.

Some diseases remain tenaciously fixed as we have seen, in certain localities, while others disappear with the progress of time, and the changes of condition and circumstance. Instances are numerous of both kinds. Fevers and intestinal diseases are rife as ever in the Roman Campagna, and many parts of Asia and Africa. Cutaneous affections, as old as the races, continue to haunt the poor of Syria and Persia; and in the same regions, lepra still exists, and the plague. But the plague no longer assails any portion of Europe, out of Turkey; and leprosy, once common, is so rare, that we may consider it

virtually extinct. Here and there, too, in Europe and America, we can point out particular spots, whence the ancient fevers have been driven, or have disappeared, and new ones have taken their places. We learn, that even in Africa, and in the hot climate of Algiers, a thriving city of colonists has been planted in one of the most fatal districts occupied by the French; and Bou-Farik flourishes in comparative salubrity. It is not uncommon to notice such a change in our own country; towns grow up in low places at the cost of a sad proportion of lives. Bou-Farik presented a bill of mortality, for some time, of 70 per cent., during its struggle for existence; and New Orleans was, for a long while, a most insecure residence, while going through the series of changes, which, as Harris tells us, have resulted in its becoming as healthy a place as New York. We learn thus, to make distinctions among endemics or local diseases, and to ascribe some of them to causes that may be controlled, and others to unchangeable and permanent conditions. Some of these we think we know, and, discerning, feel ourselves able to contend with them at advantage.

The most familiar and widely-spread of all endemics is periodical fever—fever and ague. Our profession, generally, has agreed to ascribe it to a special cause, which Headland calls the “ague poison,” and which we conventionally designate as malaria,—a word insignificant in itself. There are a few dissidents—some maintaining that there is no proof of the existence of any specific poison; others going further, and contending for the sufficiency of incidental, or patent and obvious causes, heat, cold, moisture, and their alternations. I consider the point to be as well settled as any other in pathology. Intermittent fever is confined within known limits, uniform in its features, characteristic in its history, amenable to a specific remedy; in all these circumstances differing from all incidental maladies.

When we set ourselves, however, to investigate the nature and source of this ague poison, we are met by almost insurmountable obstacles. Exceptions to all apparent rules beset

us on every side. We find these fevers most certain and most rife where heat, moisture, and vegetable life are prominent in degree and amount: yet, we also find them where each of these elements is wanting, and fail to find them, occasionally, in places where they are all combined. Heat alone is present in Malta and certain other rocky islands in the Mediterranean, notoriously subject to periodical malarial fevers. These are among the most arid spots on the globe—no vegetable growth existing there. The same fevers are found in the cold climates of England, Holland, and North America, but are unknown in Australia where intense heats prevail. Moisture abounds in some of the above-mentioned places; but moisture alone, or even with heat, does not always suffice. These fevers are unknown in mid-ocean, under the line. Moelmyn in Hindostan, Point de Galle in Ceylon, and the bayous of Louisiana, and the Dismal Swamp, are exempt. Vegetable matter, so generally present where the fevers abound, may be dispensed with, as on the sandy plains of Spain and Portugal, enumerated by Fergusson, and numerous localities specified by Davy.

Certain points in the history of this poison, assuming its existence, seem to be established; it has weight, low places and ground floors are most subject to its influence; obstacles, walls, hedges, etc., arrest it; it has not self-repulsion, or diffusive quality, as is proved by the singular limitation of its agency recorded by the best authorities—one side of a road, a river, or even of a house, being affected; it is moveable by winds, however, over distances variously stated—Lind says, at a cable's length from the productive shore, ships were safe; Allan and Davy affirm the perfect security of vessels moored, even within the reefs. Exceptions here occur; it has been blown a mile or two off shore, and up the side of a mountain. Scherzer tells us of some persons taken ill on board the ship, while others of a shore party escaped.

The failure of all chemical analyses and all eudiometric tests, seems to prove that it is not a gas. Until very recently, no one has announced, with any confidence, its detection in any palpable shape. Boussingault and Hume, it is true, found some

dark carbonaceous matter left after evaporation of dews from foul places; but the experiment has been frequently repeated by Drake and others, with no similar result.

My distinguished predecessor, Prof. J. K. Mitchell, was led, by reflection on the symptoms and history of intermittent fevers, to the opinion that their cause must be a living organized agent—ultra-microscopic, as not having been detected—vegetable from its connections, and from the fact that animalcula are rarely poisonous—a fungus, because fungi are thus deleterious, very generally, and fungi usually abound in fever spots. This hypothesis he has most ably argued in a little work containing the substance of his opinions on the subject.

Prof. Salisbury of Cleveland, Ohio, impressed with the same views, pursued a similar series of researches, employing more hopefully, and with intense zeal and perseverance, the improved means of physical exploration. He announces to us the success of his inquiries, in the discovery of the ague poison, which he finds to be an alga of the species *palmella*, a minute but visible sporule. He examined, patiently and assiduously, the excretions of the sick, and the surface of the soil where they were attacked. Amongst great varieties of algoid and fungoid cells, he found this only to be uniformly present. Covering the soil, or destroying its surface with lime, he remarked that the fevers ceased to occur; removing a portion of the soil with this algoid growth upon it to healthy spots, in three or four instances he found that fever attacked those who were in close proximity to it.

In all malarious places that he examined, the ague-palmellæ were found growing in profusion. If his statements are confirmed by further repetition, and in the hands of additional observers, we shall admit without hesitation that one source of periodical fever has been fairly detected. But we must not generalize too hastily. Even after the relation between the poisonous palmellæ and the febrile attacks shall have been established to be causative, and not merely coincidental, we shall by no means regard it as proved that these algoid spores constitute *malaria*, or are to be considered as the exclusive

producers of periodical fevers. These maladies occur, as Davy has shown, in great numbers, in every form and every degree of intensity, on some of the most arid spots on the surface of the earth. We have already mentioned Malta as notorious for its fevers of this type, and Via, and several other rocky islands of the Mediterranean, where the tables kept by the British army surgeons are full of mortality from, and their hospitals crowded by, every variety of intermittents and remittents. The seeming contrasts and caprices too, which present themselves, must be fully weighed as objections to this assumption. Low, moist places in Ceylon, likely to abound in algæ and fungi, are habitually healthy; others similar, are healthy at one time, and then unhealthy. Livingston tells us, in his Travels, that on the Ue, where everything was wet and sloppy and covered with fungi, there was no fever. And when we reflect on the tenacity of these fevers, the obstinacy with which they recur, even when patients have been long removed from the point of first attack, we find still greater difficulty. Dr. S. tells us that the palmellar spores may be evolved in the body, and that they are eliminated from it, for he finds them in the expectoration and urine of patients. But how long will they continue to find a *nidus* for the development of successive germs in cases of intermittents, which we know may endure for months and even years? And again, as to the interval, Dr. Mitchell ingeniously suggests the death of one generation of fungous sporules, and the development to maturity of the next, thus constituting periods. But when these periods change, and a quotidian becomes a tertian, and a tertian a quartan, is it possible that the periods of development and growth of the germs can thus vary?

After the interval of a winter, intermittents often return upon a patient, even after he has removed far from their source. Have the palmellæ spores remained dormant in the system thus long? I know that none of these unanswered questions involves an unanswerable objection. I admit that our ignorance on these obscure points in pathology must not stand in the way of any truth—when it is proved to be the truth—

established; but meanwhile we require their due and impartial consideration.*

Can there be any connection between the bodies found in the blood, in marsh fever, by Frerichs, and described as pigment granules, and these fungi of Salisbury? Dr. Meigs, of this city, has also observed them. On pricking the finger, and placing a drop of blood under a quarter-inch lens, it was seen that the red corpuscles were not more than one-quarter as numerous as in health, the white corpuscles normal—while in the field there were numerous minute particles of irregular shape, with angular edges, of blackish color, and entirely opaque. It is supposed that the red globules are destroyed, and their red matter converted into black pigment, which is carried into the vascular apparatus of the body. But—conjecture for conjecture—if there be fungous sporulæ (palmellæ, etc.) diffused through the blood, they must die and change to carbon, assuming a dark color, and presenting the appearance above described.

The universal family of man seems to be subject to the evil influence of the agents which cause malarial fevers. But there is much difference in the degree of liability. The white races are specially prone to suffer. Boudin and Brace tell us that the Hebrew tribes are obviously less so than all the other white peoples. They alone increase in population in Algeria; they alone "keep up their number" on the Red Sea. The black races enjoy great comparative immunity; the Kroomen of the African Coast, and the inhabitants of Fernando Po, most strikingly. Yet Livingston had many cases of fever among his native attendants. In our own country, the blacks escape best the more intense malaria of the low swampy rice-fields. In the less fatal uplands, where the white man may live doubtfully, and cultivate cotton, they are more liable to

* Since the publication of Prof. Salisbury's interesting paper, several claimants have presented themselves in Europe, who urge the right of prior discovery. Among them, are Lemaire, of Paris, Vin den Curpet, of Brussels, and Dr. Hannon, for Prof. Morren, of Liege. None of them, however, has, like him, stated his views clearly and definitely. He is the true discoverer, if the discovery be established and confirmed by future experiment.

fevers than below. Paradoxical as this may seem, it is established upon the best authority—especially in Georgia and Alabama.

Even in the birthplace of the red man, as Gibbon and Henson assert, the negro is safer than the autochthon—the Indian, who ascribes to the presence of the white intruder and his plough, the destructive fevers, unknown to his savage ancestors. The tawny tribes are not universally exempt. In Hyderabad, Napier tells us, they suffered equally with their white invaders; and Colonel Shakspeare, in announcing the value his Hindoo mountaineers set on tobacco as a febrifuge, incidentally informs us of their susceptibility to fevers. So the Chinese suffer in Panama; yet there must be a great difference between them and the whites. Martin affirms that the whites cannot become acclimated in Hindostan; nay their progeny has never, in any instance, arrived at the third generation.

It is among these tawny nations, however, that we find examples of the most complete immunity from malarious fevers. The Tamul tribes, generally, are affirmed by both Brace and Hodgson to enjoy this great privilege; the Bhotiga, the Mishnies, some of them living on the declivities of the Himalaya mountains, others on the lowest region at their feet, the basins of the Tistá, Gand'achi, and Burrampootra. "They are generally fine, healthy races of men," says Brace, "though dwelling where no other human beings can exist."

I have already mentioned the capricious appearance and non-appearance of the periodical fevers, which show the presence of malaria in certain places. Attempts have been made in various localities to calculate the frequency and rate of these occurrences and to reduce them under some rules of periodicity in these respects. What we know of the matter clearly, is this; that where these fevers exist permanently, where their cause is annually reproduced, or perennially present, as proved by the sporadic occurrence of cases in every hot season, there happen, from time to time, regularly, or irregularly, occasions of epidemic spread of the endemic calamity.

It will require a long protracted series of careful observations to determine why it is so: whether because circumstances have favored the development of the material cause in greater quantity, on a wider scale, or in enhanced intensity; or have acted upon the exposed bodies, so as to produce a greater susceptibility to its impression; or whether there is a recurring cycle of activities of any special nature, obvious or undetected, which we may refer to.

But I must pretermit for the present, any further discussion of a topic fruitful of interest in itself—full of obscurity and difficulty, and, as yet, admitting rationally of much scepticism and difference of opinion.

Not less important among the causes of diseases is contagion, the subject upon which I shall next direct your attention. It is among the most lamentably familiar facts, which are universally known, and have been observed from the earliest times, that certain maladies spread themselves, as from centres, extending in various directions, independently or indifferently as to the topographical conditions which limit the prevalence of the epidemics of which I have been speaking. Known under the heads of General Epidemics and Pestilential Maladies, they obviously divide themselves into two classes, recognized by general or universal consent in certain strongly marked instances—the one resulting from undefined, unrecognized contingencies, diffused widely over earth and sea, as influenza; and the other class as clearly manifesting a palpable connection, or interdependency among themselves; these latter we call contagious affections. They are numerous and varied, and are found among all the classes and orders of Nosologists. The modes of their connection, or mutual relation, vary; no two, indeed, being precisely similar in this regard. There are few points in pathology upon which the profession is so much at dispute as these. An intense unwillingness to accept the allegation of a contagious character, as belonging to any epidemic, exists in the minds of a large school of physicians, who demand what the Irish orator calls “an Ossa upon a Pelion of evidence,” before they yield their incredulity. For my own

part, I will avow at once, and freely, that in the Natural History of Diseases, the more I have studied them, the more reason I have found for laying down the general rule, that "like has a tendency to produce like," and that the exceptions among specific diseases are comparatively few. The *causa causans* of any malady, whatever it may be — a pestiferous exhalation; an altered secretion; a new deposition; a cell formation, of peculiar character; a morbid excretion; a parasitic production, animalcular, fungoid, or algoid, vitalized or semi-vitalized, individual, or, in Vogel's phrase, endowed with semi-individuality—if generated within a diseased human body, or any part of such a body, and capable, when brought in any manner to act upon a healthy human body, or any part of such a body, of afflicting it with the same diseased condition in which it took its rise, is a *contagion*. Any disease so conveyed or communicated, is a contagious disease. The matter of contagion—the substance possessed of this subtle property of self-production or generation, has never yet been clearly detected or set apart from its surroundings. Even in the parasitic maladies, it is doubtful what share the parasite has in the actual causation. Sir H. Cooper affirms, that the *acarus psoræ* gives rise to various, not one, exclusive cutaneous disease. "There appears no one specific eruption, which indicates the presence of the *acarus scabiei*. The nature of this is determined by the temperament or predisposition of the patient. In one case vesicles, in another papulæ, in a third pustules, are the result;" and, indeed, if the creature himself be the exclusive source of irritation, the itch is no more a contagious disease than the *morbus pediculosus*, or *trichiniasis*, or *tenia*. Its reception into this class of maladies properly depends upon the answer to a question, never yet raised, so far as I know. The propagation of itch may be ascribed to one of three sources; the animal himself to be transferred, or the *ova* within or without the insect, or the fluid of the vesicle in which he lives. The latter may be capable of reproducing the disease, of which the animalcule is an appendage. So it may be inquired of *mentagra*, *favus*, and other maladies of fungus character—Are the vegetable growths

the cause or effect of diseased action or condition? The crust being transplanted, the disease, doubtless, is extended—but is it not by the animal portion of the crust? If it be the fungus which constitutes the disease, must we not pronounce upon Prof. Salisbury's views, that malarial fevers are contagious? The *palmellæ* are as transportable as psora or muguet. Indeed, Londe maintains, in an essay read before the French Academy of Medicine, that every contagious virus, must necessarily act as a living being—it must possess vital properties. Mere poisons, he argues, of whatever nature, affect the body mechanically or chemically, overpowering the vital forces, and being destroyed by their own action upon the tissues and fluids; this decomposition being a law of chemical affinity. "Any heterogeneous substance, which, when introduced into a living body, shall remain inactive in it, for a certain length of time, then become active and multiply in it, and then leave it to act upon another body in the same manner, must possess a living principle." In a paper read before the Epidemiological Society of London, 1851, by Mr. Grove, the philosophical author of the received theory of the "Correlation of Forces," these views are most ably wrought out. His object, he avows, is to show that the poisons inducing epidemic, endemic, and infectious diseases, must be ranked among the things endowed with life. He argued, "that the faculty of reproduction was a distinctive mark between animation and mechanism, or between living and inanimate matter; that it was during the reproductive period of existence, that the most energetic operations of the vital force were manifested; and that it was during the reproduction of the poison-germs, within the body, that the force of diseased action called for our special notice. He directed our attention to the fact that, whether we examine an epidemic or infectious disease of plants, or of animals, or of man, we find that the essence of the affection is something which has the power of reproduction."

Taking this power of reproduction as indicative of the existence of a germ, he classes these agents of disease among living things, and regards reproduction as their primary law. He then goes on to infer, that if this be a correct interpreta-

tion of nature, the germs of diseases ought to acknowledge obedience to the same subsidiary laws which regulate and influence animate existences. He divides those laws into two classes: the first acting upon faculties or properties inherent in the germs; the second governing the action of external agents or influences upon them. The laws which he designates, ten in number, he tells us are not laid down as absolutely definite expressions of a comprehensive idea, but merely as the readiest approximations:—

Objective Laws.

1. The diffusion or dispersion of germs.
2. Their static existence.
3. Their duration of active existence.
4. Their period of development.
5. Their intermittent reproduction.

Subjective Laws.

1. Seasons of activity.
2. Climatic influence.
3. Relation to latitude.
4. Subjection to physical forces.
5. Influence of locality.

He went on “to demonstrate the application of these laws to the agents of disease, and their analogies in the vegetable and animal kingdoms—and show the impossibility of accounting for epidemics and infection upon the chemical basis; seeing that in no purely chemical process, was there any multiplication of the agents, and that as far as physical forces are concerned, one always increases at the expense of the other; as heat increases by the chemical change taking place in combustible bodies and their destruction.”

The matter of contagion is the usual generative cause of the contagious disease whence it takes its origin; but can we pronounce it to be the only cause—the exclusive source of such disease? The assumption, I think, would be hasty and untenable, though it has been so generally received and held so strongly, as to have become the foundation of one of the most familiar objections against the alleged contagiousness of any malady; and the argument is often pressed, as if conclusive, that “a disease, having arisen spontaneously, as it were, sporadically or independently of previous cases of the same kind, from whatever contingencies it may be derived, is proved not to be contagious in its character.” But let us consider. Every

malady must have had a beginning; it must either have been created as a separate entity, which will scarcely, we think, be suggested by any scientific inquirer, or it must have arisen from and under certain contingencies, which developed it, or favored its development. If the latter be the correct view, it is unsafe to affirm that the same contingencies can never again occur. Take the case of small-pox. No one expects or fears to meet with a sporadic or spontaneous case of small-pox. It would be entirely out of the course of our ordinary experience. But observation and experiment have proved, that if such a case should present itself, there would be no necessity for recurring to miracle, or even special providence, for an explanation. Many erudite physicians admit the identity of vaccine and variola. Bouley and Dupaul maintain that they both find a common origin in aphthous stomatitis of the horse, from the *grease* on the heels of which animal Jenner himself thought vaccine, at least, was derived. But nobody doubts the frequent spontaneous occurrence of vaccine in the cow, or of grease or stomatitis in the horse; animals with which our human race is so frequently in close contact, that we may readily derive from them the germ of small-pox, which shall only require certain fostering conditions for its modified development into variola. Again, Thompson, Cross, Hennen, and many others, are satisfied of the close affinity in nature, if not the absolute identity, of varicella (chicken-pox) with variola (small-pox). I confess, I have very little doubt of their intimate connection. The authorities above mentioned recite numerous instances in which chicken-pox, which we often meet with sporadically, has produced small-pox. Davy speaks of their closely intermingling, in Malta, under his careful observation, and confesses that he regards the diagnosis as doubtful.

Scarlatina was once observed by Gregory himself, as he supposes, to have occurred sporadically, from exposure. Measles, or a cutaneous exanthem very like it, has been excited by the fungous emanations from mouldy straw, as Salisbury contends—and so of many others. The typhic group of fevers—true typhus, typhoid, relapsing fever—may all happen as the

result of conditions, that newly affect individuals, or masses of men; may be generated in a close ill-ventilated dwelling an ill-ordered prison, a crowded emigrant ship, a foul alley. I know that Watson and others, still affirm that these continued fevers arise from contagion exclusively, but this is a narrow view of the matter. On the other hand, Bartlett and the majority of our American brethren, show a reluctance to point out the cause of our familiar typhoid, or to ascribe it to its true source in human exhalations, neglected and festering; and many absolutely deny that it ever arises from contagion. That all these fevers are capable of springing into existence under the circumstances indicated, seems to me as clearly proved, as that they become contagious after they are developed. Contagious diseases, then, may have more than one source. Contagion has but one, that is in the specific actions of the malady, from and during which it arises and is generated. Its characteristic is its power to reproduce that malady. Each variety of contagious matter has its special elective affinity, as well as its special potentiality. One acts on the skin, another on the digestive organs; one on the mucous surfaces, another pollutes the blood. Some are volatile, and diffuse themselves in the air; some are fixed, but obey the universal law, *corpora non agunt nisi soluta*. We may keep small-pox and vaccine matter in the dry state, but they must be dissolved before they will act. The cholera poison is plausibly supposed to be mingled with the dust of the soil, where a regiment or caravan have suffered the disease, and when raised by the winds, to be driven against and inhaled by unfortunate passengers, acting as soon as moistened by the sweat or the mucus of the mouth and nostrils. Some—and among them, happily for us, the most deadly, the hydrophobic virus—require to be intruded beneath the protecting integument, and will not affect us but through a wound or abrasion. I have said that it has never been discerned—that we do not know the appearance of any form of contagion. It is connected with the cell, as in small-pox, syphilis and cancer; with the amorphous stroma, or deposit, as in tubercle; with the blood, as in scrofula and scarlatina.

But neither by the microscope nor by the minutest chemical analysis, can we distinguish the pus globule of small-pox or syphilis from the most laudable pus of the surgeon, nor detect any peculiar substance in the blood of the most pestilential malady, nor in the saliva of the rabid animal. Nor does the chemist detect in the atmosphere the cause of those contagious diseases which spread only through the atmosphere, or chiefly in this way. We are aided by the sense of smell, very usefully, however; and ought to be warned by our instincts that offensive air is apt to be injurious. It is unquestionable, too, that many diseases give out peculiar odors. Scarlatina is in this way remarkable, and measles, and puerperal, and several other fevers. But there is nothing of the sort observable, so far as I am aware, or have ever heard alleged, in pertussis or mumps, both of which diffuse themselves atmospherically.

The epidemic spread of all the febrile contagions and of some not usually ranked as febrile, is a very striking and obscure phenomenon. In all civilized, and many savage populations, variola is a constant denizen. Our bills of mortality in large cities never fail to enumerate a death or two weekly or monthly, many, more or less, annually, of it, as well as of scarlet-fever, measles, typhic fevers of every diversity, diphtheria, etc. But, as you are aware, every now and then we are annoyed and panic-struck by the large and unaccountable increase of the deaths ascribed to one or another of these diseases—which we then denominate epidemic. The suddenness of their explosion is, at times, appalling.

We have a minute and interesting history of the outbreak of scarlatina at Eastbourne, a very healthy and popular watering-place on the English coast. An imported case occurred there on the 2d of September; another was seen on the 4th; then "the virus lay dormant, seemingly, until the 13th, when seventeen cases presented themselves to one physician, and on the 14th, twenty cases to another; the experience of other practitioners was similar. There was no indication of the silent danger; no apparent evil to be averted or shunned.

The epidemic, too, made its appearance in the highest circles; in the best and largest houses; in no isolated area; in the vicinity of no palpable nuisance; but instantaneously at various points, in fine, new, well-fitted dwellings." Prof. N. S. Davis gives a somewhat similar account of the epidemic spread of small-pox in Chicago, in 1855: "Previous to the 1st of January, they were so few cases in the city, that the subject had attracted no attention, only one instance having come under his observation. Between the 6th and 10th of that month, it sprung up, as if by magic, in almost every section of the city." Similar is the history of the outbreak of cholera in Sunderland, in 1831; similar the record of the visitations of dengue to Charleston, in 1828 and 1850; similar the sad invasion and spread of yellow-fever, almost always.

"Epidemic influences" are not seldom spoken of, as something peculiar and apart; as differing in nature from the known causes which produce the ordinary progressive extension of diseases. "It is clearly apparent," says Prof. Davis, in the conclusion of his instructive paper, quoted above,—"*It is clearly apparent that the city had been visited by a distinct variolous epidemic influence.*" He does not suggest in what this distinctness consists. The subject is too obscure, almost, for a reasonable conjecture; but as we are in this case of winter prevalence, and in that of Eastbourne, precluded from any reference to the insalubrity of atmosphere or anti-hygienic conditions, I will venture to propose the hypothesis, that the difference between the instances of slow diffusion, and those of dispersive explosion, rather lies in the character and type of the malady itself. It seems certain, that cases of small-pox, or any other contagious affection, differ greatly in the degree of germinative power they possess: one will affect many or all within reach—another none, or a small proportion. Nor is it the most violent example in degree, which would be most likely to prove efficient; protraction into maturity, and a prolongation of the stage at which virulent emanations are given off, would be most dangerous. It appears altogether probable, too, that the condition of a population may be of importance,

regarding them hygienically—that a well-fed, well-clothed, well-sheltered community would be less liable to these all-inclusive pestilences, than another not so reasonably cared for. We cannot carry those measures of protection and improvement too far, but we should be prepared to witness their occasional failure. Scarlatina respects them little ; indeed, Hirsh thinks that the rich are apt to suffer more from it than the poor, whose condition is less thoroughly hygienic. My own experience agrees with this view ; at any rate, I am sure that negroes, poor and slaves, whether from peculiarity of race I will not say, suffer less than their masters. And the same has been remarked as to diphtheria, both here and in South America ; by Odriozola in Lima, and Arch Smith in Peru. Verrollot affirms that the Kalmuk, in his vast steppes and open plains, “ the type of man in robust health,” lost as large a proportion, 51 per cent., as the pent-up inhabitants of crowded cities, from cholera.

Its promptness of action, and the tenacity with which it retains its hold, are qualities of *contagion* deserving of remark. Bosquet and Itard tried to wash away vaccine just after insertion, but in vain. Bosquet also destroyed the pustule when formed, but the constitutional affection, he says, went on. Renault, of the Alfort Veterinary College, attempted to arrest the poison of glanders forty-eight, twenty-four, twelve hours after insertion, but failed. Yet, it will be inactive in some cases, as in hydrophobia, indefinitely ; in all of them for a certain time ; not dormant entirely, perhaps, but going through regular and well-defined stages of incubation, varying for each, and subject to a certain extent of modification. This latent period not only varies in different diseases, but may present striking diversities in the same malady. When the contagious matter is introduced by inoculation, we have a well-defined period of development usually calculable, as in small-pox, vaccine, chancre, etc. In vaccine, however, it may be delayed a few days or a week ; a case is on record, in which it lay dormant six months, and then proceeded regularly through its stages.

Of those that are not inoculable, or not usually communicated in that way, we cannot speak with so much precision. It is the general impression, and I think very well founded, that there is a correspondence observable with the septenary periodicity—the latency enduring fourteen, or seven, or twenty-one days. Yet the exceptions noted, are numerous and well marked. Bigelow recites a case of scarlatina, in which circumstances seem to prove that the disease either broke out only two days after exposure, or remained latent forty days. Typhus has seemed to sicken its victim at once. I need not remark upon the importance of these points in reference to prophylaxis, quarantines, etc.

Again, the tenacity with which contagion will adhere to certain substances and remain in certain localities, is astonishing. Mills relates an instance in which the clothes of an infant dying of small-pox, washed carefully and put away, communicated the disease to another child of the same parents, placed in the same cradle more than a year after. Burridge gives an example of this tenacity: "A noble and magnificent collegiate hall in Taunton, being infested with this contagion of scarlet-fever, for more than three years, in spite of all that skill and science could suggest for its removal," and concludes that there is no known limit to the durability of the scarlet-fever poison. I have known the odious smell of this dreadful pestilence persist in a house for months, in spite of ventilation, scouring, paint, whitewash, and all disinfectants.

It is a curious property of some contagions, that they destroy the susceptibility upon which they have acted, and thus protect the system against their own recurrence. This effect would be less strange and more explicable, if it were uniform; but it is not so. Some of them seem devoid of this power, and the others possess it in different degrees. Nay, it would seem, that one at least, erysipelas, in contrast, rather increases the tendency of the subject to a second or third attack, and so on indefinitely.

I know not that this quality of self-protection belongs to any other than contagious affections, and it occurs in so large

a proportion of these, that it suggests a fair presumption of the contagiousness of any malady that exhibits it clearly. It is not probably absolute in any. Measles, as far as my observation goes, is most remarkable for it. I have never yet seen a second attack of this very frequent and familiar disease. Next in order, perhaps, stands small-pox; yet many exceptions are recorded here. Some persons, nay, we are told that some whole families never lose their liability to it, and deaths have been mentioned from a second, third, fourth, and one from an eighth attack.

The typhic fevers present, in this respect, all of them of continued type, an impressive difference from the periodical, malarial, intermittent and remittent fevers; all of which pave the way for their own return. Typhoid fever rarely assails again a subject remaining in the same locality.

It is scarcely worth the time occupied in stating them, to notice the two hypotheses that have been offered as vaguely explanatory of the observed facts. One is, that all human subjects are born with certain unknown elements—solid or fluid—bound up in the tissues or component parts, solid or fluid, of the body, which have some relevancy to the nature of the diseases alluded to, as furnishing seat, nidus, or pabulum to them; and that in performing this office, each according to its elective affinity, they are worn out, consumed, eliminated, or undergo a metamorphic change, which unfits them for a repetition of the same purpose. The analogy suggested is agricultural, as when a certain vegetation uses up all the material in the soil fitted for its growth.

Others, following Daubeny's view, that plants, like animals, give out exhalations or exuvial matters, which are not only effete but deleterious, have ventured the strange guess, that these maladies leave something behind them in the system, which prevents their taking root again in it.

Yellow fever, whose whole history is peculiar, has been somewhat too emphatically spoken of as the *Acclimating* Fever, because a subject born within its habitual domain, or once recovering from it, and not estranging himself by absence, will

scarcely ever be attacked by it a second time. Such is, indeed, the very general rule, and happily we are thus provided with exempts for medical attendance and nursing.

I wish I could speak with the same confidence concerning cholera. I know not what degree of exemption, if any, compensates for suffering from it. I have a friend who has gone through it thrice ;—in Asia, Europe, and America.

This property affords us always the choice of time and circumstances, for passing through dangers almost inevitable ; of which, however, little avail is made except in reference to small-pox. All the world rejoices in the discovery, made so long, and so widely acted on in the East, both in Africa and Asia, that the memory of man runs not beyond it, that inoculated small-pox, for some mysterious reason, is vastly less severe and dangerous than when taken through the atmosphere. Still further, and, if possible, more mysterious is the fact, for which we revere the memory of the illustrious Jenner, who reduced it to practical utility, that a different contagion, or the same, modified to a most harmless mildness, would also exhaust the susceptibility, and protect the system from it.

Whether we can carry this plan of inoculation into the other contagions remains to be ascertained. It is asserted by the physicians of Northern Europe to be applicable in lues venerea—the constitutional liability to which is affirmed to be exhausted by repeated insertions of syphilitic matter. Scarlatina and measles have, it is also affirmed, been conveyed by inoculation with blood and tears, and Schneiderian mucus ; but the statements require confirmation and repetition. This we know, however, and I think should unhesitatingly, though prudently, act upon it with proper reserve :—as every child seems destined to undergo, at some period, an attack of scarlet fever, of measles, of hooping cough, and of mumps, and as these diseases, occurring as local epidemics, exhibit at different times marked differences of violence and pestilential malignity, it would surely be a wise and rational course to submit the child to the chances of contagious impression at some time, and on some occasion when the malady was prevailing in a mild type,

and when the constitution of the subject was in proper condition, or could be properly prepared.

A reasonable anxiety to prevent the influence of contagion, and arrest the progress of contagious diseases, urges us to repeated and varied experiment, in which the most unwearied perseverance is to be encouraged. I need not surely say a word in favor of hygienic measures, as calculated not only to destroy sources from which such disease may arise, but to lessen unquestionably, if not the liability or predisposition to these maladies, as to all others, at any rate the tendency and proclivity to yield and sink under their violence. I will, however, refer again to efforts directed against the noxious action of the matter of contagion, when subjects have been exposed to it. Poisons inserted in wounds may be rendered innocuous. We cannot, perhaps, wash them out, but Ricord says that venereal inoculation may be rendered inert during three or four days by the use of chemical caustics. Poisoned arrows were formerly used in war, and it was fully believed that suction within a short time after the wound was received, would remove the venom. Barry says, that pressure on the part will prevent their absorption. I will propose, in all cases of poisoned wounds, the employment, in preference, of the actual cautery, as alone worthy of our confidence. It destroys whatever poison may remain unabsorbed, and also reduces to an amorphous and inactive mass, the solids in the neighbourhood changing by chemical annihilation, so to speak, the whole composition of both solids and fluids implicated. It would be well, I think, to extend the same treatment to dissection wounds received in the examination of diseased bodies.

The celebrated St. Simon, having been bitten by a mad dog, cauterized the wound himself, with a glowing red-hot coal. We read of him, that, for a long while after, he kept a loaded pistol at hand, resolved to commit suicide if ever he became aware of the approach of hydrophobia. I have always burnt with the white iron, subjects so unfortunate as to be wounded by rabid animals, and have not known this method of prevention fail in any case. But I have known the lunar caustic fail,

both in this, and in dissection wounds. Youatt trusted to it, and applied it in his own case, having been bitten by a mad cat. It is stated that he died by his own act, leaving no explanation, and I cannot avoid the conjecture, that he carried out the intention of St. Simon, under the fearful expectation of the terrible malady.

Prophylactics are proposed for more than one contagious affection, and they should not be set aside lightly without fair trial. I mention, as worthy of further experiment, belladonna, for scarlatina; *sarracenia purpurea*, for small-pox; for typhic fever, as well as for the malarial, quinia; for cholera, Bowerbank recommends the drinking of sulphuric acid diluted, which is also valued as antidotal to lead-poisoning. The Easterns, from time immemorial, have employed inunction as a general prophylactic; and indeed, as some of the contagions, perhaps most of them, act upon and enter through the skin, this protection, by oil, seems reasonable enough.

Such are the means of individual precaution, but we must not neglect to consider briefly, the important question of prevention in behalf of the masses of communities of cities and of nations. Is it possible to circumscribe the spread of a contagious epidemic? Can we arrest its progress—assuming the correctness of the common belief that it comes, that it goes? Are the modes of extension, progress, communication, or conveyance so definitely understood that we can interfere with them, and oppose a check, or interpose a barrier of any kind? What is the experience of the past upon this subject?

A natural and powerful instinct teaches us to avoid the sick laboring under any known or suspected contagion. The love of life, the fear of death, the dread of suffering, all impel us to measures of self-preservation. But our instincts, when inordinately active, when carried into excess, become degrading, vicious, cruel, criminal. Friends stand aloof from friends, the lover deserts his mistress, the husband his wife, brothers and sisters forget the fraternal tie, and even the parent and the child fly from each other under the disgraceful panic. We shudder with horror, and blush for our common humanity,

when we read the stories recorded by Thucydides, De Foe, Hodges, and Brockden Brown, of such abandonment of the victims of pestilence. While leprosy prevailed in Europe, the miserable wretch who manifested the early symptoms of this frightful malady, was at once expelled from his home, and driven into exile, and his whole future life burdened by most stringent and odious regulations. The Church, itself, laid its heavy hand upon him, condemning him to a living death, of which Monteil has given us the repulsive details. Led in gloomy procession to the chapel by the clergy of his parish, they chanted over him the prayers for the dead, with the ordinary aspersions and incense. He was then taken out of the town or village to the hut where he was to reside; there he was again prayed with and exhorted to patient submission under his irreversible doom in this world, and to hopeful faith in another and better, where he would again bloom in health and joy. Clothed in a particular and well-known dress, and furnished with a rattle, which he must always use to give notice of his approach, he was enjoined never to leave his abode in any other costume, never to go barefooted, never to pass into a narrow street, never to speak to any one, unless standing to leeward, nor to enter a church, a mill, a fair, a market, or any collection of persons whatever, nor to drink or to wash his hands, either in a fountain or any river, to take up any article of merchandise before buying it, to touch children, or to give them anything; finally, earth was thrown over his head, and the poor outcast left by all—alone—not even to be buried when he died, in consecrated ground. "In some provinces," says Montiel, "they burn the clothing, furniture, and even the house of the leper; everywhere, his vineyard, his cow, his sheep are safe; no one will touch them; for it seems that he and his land, and all that belongs to him, are struck with the same plague."

Such was then the treatment of the man whose condition was believed to endanger the health of his fellows. The system of quarantine, which has long prevailed, and still exists in some parts of Europe and Asia, and especially on the shores of

the Mediterranean, is little less inhuman. The real objection to any of these measures of precaution, however, is not their cruelty, but their uncertainty or inefficiency. The sacrifice of individuals, even in large numbers, for the good of the whole, is a familiar idea, and defensible enough, as well in peace as in war, provided the result is certain, and the infliction indispensable. Gamgee, and many other physicians in England, have urged that every ox seized with the rinderpest should be killed at once, even if the whole present generation should thus be exterminated. The oïdion of the vine and orange would have been cheaply got rid of on those terms. But we cannot so deal with our fellow-men; we cannot reason thus *de vitâ hominis*, nor extinguish human life in cold blood to arrest a pestilence. Our rights over each other are too limited; and besides this our success is always problematical. And if we cannot sacrifice a life, it becomes a question how far we may rightfully proceed to put it in special peril, and inflict certain and special suffering, by seclusion, by imprisonment, by great and varied privation. To make a quarantine available and efficient these inflictions are inevitable; it is clear that nothing beyond the inevitable, the indispensable in the interests of humanity, admits of justification or defence.

But greater hardship, more despotic tyranny, cannot be imagined than to stop a traveller on his way homeward, or in the pursuit of his lawful business; to confine him closely, nay to insulate and disconnect him from all that concerns or belongs to him, and this for a length of time always tedious and often indefinite, and under contingencies that subject him to peculiar risk of disease and death. Before we sanction such measures, let us obtain a distinct view of the good which is to compensate, if anything can compensate, for so much positive evil.

The absurdity of the quarantines in operation in our own enlightened age and country cannot be exaggerated. Our great commercial metropolis offers an almost incredible history of error and mismanagement. She kept up a receptacle for all forms of pestilence in the narrowest part of the entrance to her magnificent harbor, in the midst of an increasing population of

thronged and busy villages, at an easy distance from her wharves, and still nearer those of another large city, with ready access to both, and to the surrounding country, by boats and steamers and railroads. When the hideous nuisance could no longer be tolerated, and was ended abruptly by fire and force, look for a moment at the arrangements substituted. A vessel or two at anchor—no footing or shore for many months, and through a hot and plague-threatened summer, with most imperfect shelter for the sick detained—no accommodation prepared for the healthy whom they held for precaution—no place nor means for separation!

A quarantine on a dairy farm near London, Friern, is described by Dickens in his "Household Words," which might serve, if not as a model, yet as affording valuable hints: "All newly purchased cows are first placed in a separate field, during four or five weeks; the man who attends on and milks them is not permitted to touch, nor indeed to come near any of the cows in the great pasture. The proprietor himself, when he has purchased strange cows, or even been to market, invariably changes all his clothes, and generally takes a bath, before he ventures among his own herd." Would to heaven the obstetrician and the general practitioner would observe similar precautions! We should be apt to hear less, I think, of puerperal and infantile mortality.

A rational quarantine should be established at some insulated point. Besides the residence of the officers and attendants, there should be a hospital to receive the sick, and a comfortable hotel, at a sufficient distance, to accommodate the sound, whom it is necessary to detain during the latent period of infection, and until the last appearance of disease has passed away. A separate convalescent house should also be provided. No communication beyond the limits of the institution should be permitted during the presence of any pestilence. The term of confinement should be prudently calculated, and rendered as brief as is consistent with the public safety.

But after all, such are the facilities for evasion—such the wide outlets for escape, that if there were not, every now and

then, some undisputed and remarkable examples of the beneficial working of even the present insufficient and impotent establishments, one would be disposed to advocate free trade in disease, as well as in commerce, and shrink from the responsibility of imposing the terrible burdens which a strict quarantine brings upon every city, in the immediate financial injury, the pecuniary losses, the obstruction of all business, and the cessation of the several employments by which the poor are enabled to provide for their subsistence. Believing, as I do, in the contagious communicability of many varieties of disease, I can hardly consent to the entire abolition of protective restrictions; but I would have them regulated with special relevancy to the ascertained risk, and with the most reticent regard to the personal rights and necessities of all those unfortunately involved; while I would everywhere, and at all times, place a more confident dependence upon the institution of the widest, most unsparing, repressive, and exacting observance of all the rules of a scientific and enlightened hygiene.

OF CERTAIN MORBID CONDITIONS OF THE SENSORIAL SYSTEM.

THE study of the phenomena of diseases constitutes a vast field of observation and record. They manifest themselves by signs and symptoms which, on the one hand, may only affect the consciousness of the subject, and on the other, such as may become obvious to those about him. These latter, the palpable tokens by which we recognize its presence, it behooves us to regard with special consideration.

No differences among individual men are more remarkable than their several habits of attention, of observation, of vigilance, apprehension, promptness; their curiosity to see and know, their inclination and capacity for inquiry. We meet occasionally with natural-born policemen, sagacious detectives, lawyers dreaded for their shrewd cross-examinations; and, in the ordinary affairs of business-life, with a class noted for watching the currents in motion around them, and swift to avail themselves of their advantage over those who may be deficient in these useful qualities. Thus, in the sick-room, while one physician will consume his own time, and wear out the patience of his suffering client by vague and inconclusive questioning, another will, at a glance, discover and fix upon some circumstance which will serve to guide him in his investigation; notice some wandering look, some movement, some disorder of condition, some departure from familiar and natural modes of thought, speech, or action. In certain instances we shall be forced to admire the rapid and almost magnetic affinity thus established between them, to the great delight and hopeful excitement of the sick man, who is always—if of average intelligence—conscious of the existence or default of this mutual understanding, so important to the proper comprehension and treatment of his case.

This quick, nice readiness is doubtless a natural gift. Those who possess it are bound to cultivate and perfect it; if not cultivated indeed it will avail them little, as would the talent for music or painting or sculpture. Nor need those despair who are not originally thus gifted. The art of close observation, the habit of diligent and watchful attention, may be acquired; the capacity of delicate apprehension may be built up and improved. Every student should make it his determined resolve—to whichever of the above categories he may belong—that he will not fail to mark everything that may present itself to his notice, in and about the chamber and the person under his care, letting nothing escape his eye, his ear, or any of his awakened senses.

We divide the phenomena of diseases into the *objective*, those of which I have just spoken, of which we can take cognizance by sight and touch, and hearing and smell; and the *subjective*, the latter developed in and by and through the consciousness of the subject, from whom we are to derive all our information concerning them, all our knowledge of them. In the correct appreciation of all these there is often great difficulty. He who underrates the effort necessary for proper performance of his task, or enters upon it carelessly, is destined to mortifying failure. It requires indeed all his philosophy, self-possession, command of temper, and acuteness. Language, as an instrument of communication, as an exponent of feeling, is susceptible of infinite abuse and misuse, of misapplication, exaggeration, suppression of the true and suggestion of the false, either voluntarily or involuntarily; either by ignorance, indifference, or design. The nice diagnostician needs all the ability requisite in a good cross-examination at the bar.

Again: We speak of symptoms and signs with a sort of vague discrimination; vague because the terms are employed arbitrarily. We are apt, however, to reserve the word *sign* to indicate special relation, significance etymologically; we say physical “signs,” not physical “symptoms;” we apply the word “symptom” more in reference to the general history, as

we speak of the symptoms which distinguish the separate types of fever; the word "sign" rather in regard to the individual patient—as, the signs or indications of a nervous temperament, a lymphatic habit, a strumous diathesis, a feeble constitution. Besides this diagnostic selection, we point our prognostic; we speak of bad "symptoms" generally, but we denote some special fact in a given case as a bad "sign." Yet, it must be acknowledged that the terms are often used convertibly or promiscuously.

I propose at present to offer some brief and condensed views of certain of the phenomena of what are called nervous diseases, characteristically or emphatically; manifestations of disordered condition or action in the organs and textures composing the sensorial system. It is through, in, and by these organs that we feel, think, and know. Our individual integrity, our consciousness of self, or personal identity, depends upon them; it is they that connect and bring into relation with each other all the organs or parts of which we are composed. Nothing can be more obscure and inscrutable than the mode in which the several functions of this system are performed. In all other cases, our anatomical, mechanical, and chemical science avails to enlighten us with some instructive suggestions and analogies; as in the valvular structures of the heart and vessels, the arrangement of the joints and ligaments, and the changes of digestion and respiration. Not so in the field now before us. A little albumen and fat and phosphorus in soft masses, or filling hollow membranous tubes—"only this, and nothing more"—serves the most wonderful and mysterious purposes. The tubes, Beale maintains to be endless, turning back in loops, not losing themselves by distribution; Rouget describes their finely granular terminal expansion in disks. At any rate sensibility or sensitiveness is so thoroughly diffused, statically, as Radcliffe supposes diffused electricity to exist in muscle, that I do not doubt the distribution everywhere of nervous matter, the substance endowed with the capacity of taking cognizance of impressions from without. Nor can I conceive of this capacity except as multiform; so

far from being a unit, that as Brown-Séquard contends for nerves of heat or temperature, resistance or solidity and the like, in addition to the special nerves of the senses, touch, hearing, etc., so I assume as of absolute necessity the presence of nervous matter all over the surface, at least susceptible of pain in all its varied characters. The alternative of this belief in the universal diffusion of sensitive tissue or nervous matter, is simply the admission that the property or quality of sensitiveness belongs to all the solid textures, promiscuously and independently of the presence of nerve or nerve-fluid. Parts insensible in health become acutely sensitive when inflamed; neuralgia often fixes its concentrated agonies in localities of no special nervous development, proving a diffused capacity of suffering, if of no other mode of perception.

It is not probable that any physiologist or pathologist would venture to maintain the absolute electrical character of the nervous influence. An afferent nerve conducts, as the phrase is, something from the periphery to the great central column and ganglion; an efferent nerve conducts something from the centre to the circumference. By the first function it is given to us to perceive, to know; by the second, to will, to act, to move. The nerve will conduct the electric fluid, if it be a fluid, and so will a copper or iron wire; but the metallic thread will not conduct volition downwards, nor convey a sensation upwards to excite perception. Nevertheless, the books are full of assumptions or suggestions of this identity—for the phrases mean more than the assertion even of the closest analogy. Radcliffe makes the state of the muscle-fibre both in repose and contraction, depend upon its electrical condition; and Todd and others, indulge in frequent reference to the polarity, polarization, and depolarization of nerve-textures. No one can doubt the ready susceptibility of the body to electrical influences; so great is it indeed that it would seem scarcely possible to exaggerate it. Yet, surely this has been done by Radcliffe, as above, in ascribing all muscular action to such influence.* It is among the

* It is still more exaggerated by Brown-Séquard when he affirms, as he does in

most important agents with which the animal constitution is arranged to harmonize, and without whose concurrence animal life would be imperfectly, if at all developed, heat namely, oxygen and light. With all these the nervous fluid or force is in close accord, ready to receive and transmit their impressions and a thousand others to the seat and principle of vitality, with which it is more intimately interwoven than any other of the tissues, fluids, forces, or parts which go to constitute our complicated organism.

Among the most frequent symptoms of disease in general, are pain, wakefulness and coma, spasm and convulsion, on which I shall proceed to treat briefly and comprehensively. Pain is a topic of so wide import that it would be vain to attempt here its full discussion; my remarks will include but a few of its vast and interesting relations. It is an almost invariable concomitant, an expression we might call it, or familiar synonym of disease. It is a simple idea, therefore indefinable—an inexorable fact, undefined. All attempts to effect such definitions are utter failures, or mere tautology. The states of sensation which are agreeable to our nature, says Stoddard, "we call properly pleasure; those of an opposite kind we call pain." This is nothing more than Plato's—"the effect of interruption of the bodily harmony." Aristotle defines it as "the reflex of bodily imperfection." Sir William Hamilton attempting to combine the two and freely paraphrasing both, tells us that "pleasure is the result of certain harmonious relations, certain agreements; pain the effect of certain inharmonious relations, certain disagreements. Pleasure is a result of the spontaneous and unimpeded exertion of a power, of whose energy we are conscious; pain a reflex of the overstrained or repressed exertion of such a power." "Pain in neuralgia," says Romberg, "is the prayer of the nerve for healthy blood." Is not all pain truly neuralgic? Anstie and Radcliffe agree in ascribing it to "low action;"

his "Experimental Researches," p. 11: "I have seen muscles created by galvanism and becoming as strong as they are in healthy men—in cases of lead palsy, in which the extensor muscles, as far as I have been able to judge, were completely destroyed and replaced by fibrous tissue."

"impaired vitality." I need not offer any comment upon these phrases, which it will at once be seen, upon reflection, evade the point of inquiry by reference to alleged causes rather than description of the effect.

When we use the word we must take it for granted that we understand each other, and mean the same thing. It may be dissected, so to speak, discriminated as to modes, separated under several categories, distinguished by epithets conventionally descriptive; thus we talk of aching, cutting, tearing, burning, oppressive, sharp, dull pain.

It is not, as is affirmed in the quotations above given and indeed very generally received, it is not the correlative of pleasure. Many parts and textures suffer pain, that are incapable of definite local gratification or enjoyment, that are indeed absolutely indifferent and unconscious physiologically. This is true of the brain substance, of the serous and glandular and other tissues, which when diseased become seats of severe pain. Organs and surfaces that are susceptible of pleasure may suffer correlatively or in contrast, but they also suffer in modes which possess no relevancy with their special or functional sensitiveness. Thus the eye, the inlet of exquisite enjoyment, is shocked by deformity as it is pleased with beauty, and dazzled, confused, and pained with discordant tints, as it is delighted with soft hues, and gorgeous coloring. But these are slight annoyances compared with the gratuitous and unassociated pangs inflicted by the intrusion of a grain of sand, the presence of a hair, the pungency of a wreath of smoke, or a drop of acid. The ear entranced with the soft, melting song, the soul-stirring clang of martial music, and the subduing inflections of the human voice, is hurt correspondingly by the grating of harsh, rough sounds, and the jar of loud and violent explosions. Yet these are trifles in contrast with the tortures of mechanical injury, and of the inflammation to which it is so often subjected.

Final causes can hardly be entirely banished from the domain of philosophy; they must be referred to, however, with much reserve always and caution. On every side we hear of

the uses of pain, as warning and protective against lesion of parts; as indicating the point which stands in need of attention and care; nay, as suggestive often of the mode of relief required.

Sir Humphrey Davy has ventured the greatest hardihood of assertion on this point. "Pain seems," he says in his *Salmonia*, "in all cases to precede the mutilation and destruction of those organs which are essential to vitality, and for the end of preserving them." And again: "Pain seems intended by an all-wise Providence to prevent the dissolution of organs, and cannot follow their destruction." The facts are not as he states them, nay, not even in their seeming, and the reasoning shadowed forth is most inconsequent. Pain does not always precede the destruction of organs; if it did, we would be forced logically to consider it as causative, not preventive. It can and does sometimes follow it. The destruction of an organ is not necessarily its death. The eye and the womb, unfitted for their physiological purposes, are often seats of intense agony. To all such eulogy I would reply further: That pain as warning or protective always comes too late. It is the product, the result of diseased action, which, so far from having any tendency to prevent or arrest, it excites and exasperates. If I am told that the remembrance of suffering on one occasion serves as premonition on the next, I refer to the familiar readiness with which the past is forgotten. Martineau indeed dwells on the extreme facility of oblivion in regard to suffering of every kind, as one of the happiest traits in our natural constitution. And besides this, I would ask whether men would not as well profit by the experience of loss or injury, even if painless, as we protect our persons and houses from the perils of water and fire.

It must be admitted, too, that the warning, the information, the instruction thus given is often insufficient or defective, and frequently irrelevant and illusory. Some of the most fatal diseases imply little or no suffering, as in tuberculosis and certain cerebral affections; and others that involve little danger, are full of agony, as toothache, gout, neuralgia. I would not

undervalue the utility of pain as indicating the organ assailed; but let us reflect how often this is deceptive. In a febrile attack we have pain all over the body; and the most dangerous fevers commence frequently with least suffering. We can rarely depend with any confidence on this diagnostic indication. I will not dwell on the sympathies which occasionally lead us astray: but where is the seat of diabetes—of leuchæmia—of epilepsy?

It is true that the kind of pain felt is sometimes instinctively suggestive of relevant methods of relief, and here is the foundation of the exaggerated notions concerning its usefulness, upon which I am commenting. I would avoid extremes. The pain of fatigue solicits repose; the pain of tension demands relaxation; the pain of burning calls for the application of cold. Perhaps this list may be extended; let us enlarge it to its fullest natural limit. And then—what a vast number and variety of modes of pain will remain that are purely evil, unmitigable, merciless evil. Aching in all its diversities—toothache, facial and general neuralgia, the cutting pain of this last and of the sorrowing parturient woman, the mingled lancinating and burning of carcinoma—what shall we say of all these?

For my own part I am no stoic. I regard with contempt the stolid philosophy of Zeno. I am professionally the sworn enemy to suffering in all its forms. I will endeavor to remove it, to put an end to it whenever it is possible. I will never inflict it unless satisfied that it is the least of evils forced upon my selection, and then I shall consider it regretfully, and almost remorsefully, as the true surgeon an amputation.

Instinct as well as reason has led all men everywhere to seek the abolition of pain. Hence the universal delight in narcotics, greedily sought for, discovered, invented. We hail with wide acclaim, with joyful exultation, the introduction of anæsthetics; the nitrous oxyde of poor Wells, the æther of Morton, the chloroform of Simpson. Old Burton spoke like an oracle when he summed up the account of suffering and enjoyment for our common humanity. "For a pint of honey

thou shalt find a gallon of gall; for a drachm of pleasure a pound of pain; for an inch of mirth an ell of moan; as an ivy doth an oak, these miseries encompass our life." Bayle, in his great dictionary, places the matter in a different and very striking light. "It may be said that the days in which men enjoy their health are in greater number than those wherein they are sick. But there is perhaps as much misery in fifteen days' sickness as there is pleasure in fifteen years' health." (Pericles.)* Let us reflect, too, that pleasure is always fleeting—neither to be protracted nor promptly renewed; when intense, its ecstasy is momentary, and dies away in indifference, satiety, and disgust. Yet I would not have you lose sight of the reverse of the picture. While pleasure enervates and emasculates both the mind and body, pain makes heroes; develops courage, patience, and defiant strength of will; and giving opportunity for sympathy and benevolence, raises man above the angels, who can only pity but cannot sympathize or offer self-sacrifices, in all which man approaches and imitates the God-like character.

Considering pain as a symptom in diseases, we remark that it connects itself with the most opposite conditions of the organism and its parts; with hyperæmia and anæmia; with too much and too little action; with tension and laxity; it belongs to both local and constitutional derangements, and results as well from mere functional disturbance as from structural and organic change. So far as I am aware, it is physically compensative only in its general and vague indication of the viscus attacked, and its equally vague suggestion of the mode in which the suffering originates. I need not repeat that even

* H. C. Lea, in his erudite and interesting work on "Superstition and Force," confirms this view when treating of the endless variety and unspeakable intensity of the various modes of torture invented and employed by different nations in successive ages: "These trials (he says) seem to transcend the possibility of human strength. The limitless capacity of human nature for inflicting is not complemented by a limitless capacity of endurance on the part of the victim." (p. 383.) The agonies of many forms of disease are beyond imagination; not only as grievous as any artificial inflictions, but enhanced by a correlative morbid sensitiveness, a hyperæsthesia generated by the malady itself.

in these relations it is insufficient as well as uncertain, and not seldom actually delusive. Headache and gastric disorder, cerebral and cardiac affections, spasms, convulsions, and numerous other instances might be offered in which it gives no sign, in which it is in no degree to be depended on, and if depended on would be altogether likely to mislead.

On the directly evil and injurious influences of pain, it cannot be necessary to dwell. It excites morbidly an already perturbed system; it irritates the part or constitution of which its very presence arouses and intensifies the irritability; it exhausts the vital power; it disorders the intelligence; inflicts fear; depresses and dejects the spirits, and paralyzes often the functional capacity of the tissue which it attacks.

The eulogists of pain have, however, generally failed to lay the proper stress upon its incidental therapeutic efficacy under occasional contingencies. Prof. Hamilton (*Med. Rec.*, Sept. 15) assents to the ancient doctrine that pain is a stimulant—so totally inconsistent with the views of Radcliffe and Anstie, who look upon it as essentially connected with “lowered vitality”—and in surgical emergencies would not hesitate to resort to it as such. It is often employed with this purpose in medical practice. The pain which they produce, for example, makes blisters serviceable in certain stages of fevers and other maladies, in which prompt or sudden prostration threatens without actual exhaustion of the *vis vitæ*. I was once engaged in attendance on a case of myelitis presenting paroxysms of the most acutely agonizing spasm, quasi-tetanic, for the relief of which chloroform was freely administered. During its inhalation my patient was accustomed to announce the grateful approach of anæsthesia by simply raising a finger, when the remedy was withdrawn. One day a friend held the chloroform to his mouth and nostrils, standing so that I could not see the face of the sick man, but sat watching his hand for the usual signal. Thus occupied, I was startled by my friend’s suddenly turning on me with the announcement, “he’s dead!” which he repeated quickly and emphatically with an oath. Our patient lay before us, pale, breathless, pulse-

less; seizing a vial of hartshorn, fortunately at hand, I lifted his eyelid and touched the conjunctiva with my finger wetted with the sharp spirits of ammonia, and he sprang up into life again with a shrill cry.

Wakefulness—insomnia, is the next of the symptoms of sensorial disease to be noticed here. Sleep, the repose of the nervous system emphatically, the renovator of its exhausted power, is one of the most absolute necessities of animal existence. Sir John Sinclair, in his inquiries concerning longevity, declares that there was but one uniform circumstance attendant upon or coincident with protracted life. He had seen extreme old age in the glutton and the ascetic, in the drunkard and the temperate man, in the stolid and the excitable, in the fool and the wise man—but all, without an exception, who had lived very long, were good and sound sleepers. Brigham, one of the highest authorities on insanity, announces an analogous coincidence of contrasted character here; morbid vigilance being its most frequent precursor, as we know it to be one of its most constant and unmanageable attendants; indeed he pronounces it to be prominent among its immediate causes. Protracted wakefulness, it would seem, must terminate in both bodily prostration and morbidity or abolition of intelligence. It is curious to find so respectable an authority as Laycock making light of the effect of wakefulness. He tells us that he knew a gentleman who never slept a moment for a month, yet with no bad results. Boerhaave is recorded not to have slept for six weeks, in consequence of intense study. Handfield Jones had “a patient from India, who thinks he never slept an hour for five months.” Yet, Winslow, on the same page in which he refers to the case of Boerhaave, gives the story of a Chinese condemned to die by privation of sleep, and states that he survived, enduring this torture, not more than nineteen days.

There must be carelessness, or error here surely. I can much more readily believe that in the strange instances above reported, there was some mistake, some hallucination, some defect of supervision, or even some wilful misstatement by an

imbecile, insane, or vicious patient, than accept as true an assertion so contrary to nature, to our constant experience, and to all seeming probability.

The state of sleep has attracted strongly the attention of the philosopher and the poet, as well as of the physiologist. Some strange misapprehensions have arisen concerning it; one of the loftiest of our poets uses the phrase, "Death's twin brother—sleep." Sir Thomas More would never trust himself to "nature's soft nurse," on account of the alleged family likeness, without a prayer to Heaven for protection. Montaigne, the quaint old Gaul, also dwells on "the resemblance between death and sleep," and "wonders how carelessly we pass from waking to sleep, and with how little anxiety we lose the consciousness of light and of ourselves." Nay, the philosophical Good defines sleep as "the death or torpitude of the voluntary organs, while the involuntary continue their accustomed actions; death is the torpitude of the whole." And Bichat, profound physiologist as he is, affirms similarly, that "death is but a collection of partial sleeps of the several organs and functions."

I protest against these views as full of gross error. Death implies inaction from incapacity: no such condition attends sleep; the vital organs are all in full play—circulation, respiration, nutrition, assimilation, secretion, and excretion go on uninterruptedly. Death is the beginning of disintegration, destructive change; sleep is the period of renovation, restorative change. Death is the correlative and contrast of organic activity; sleep is the correlative and opposite or negation of sensuousness, sensitiveness, psychical activity. Whatever we may conceive *nervous power* to be or to consist in, and whatever source we may suppose it to flow from, and in whatever mode we may imagine it to be generated, this is certain—that it is liable to be diminished by use, or drained away, or exhausted, and thus all our capacities for action may be impaired or abolished. Its resupply is imperatively required; but whence? and how? Of course we must presuppose as fundamentally necessary a sufficiency of food for nutrition and assimilation. For

sensorial purposes this must be elaborated in a peculiar and specific manner, and converted into a proper tissue or substance adapted dynamically, and in a certain sense accumulated to make up for previous subtraction and deficiency. By what organs, what textures is this elaboration effected? Todd affirms, teaching the generally received doctrine, that "the centres, the collected masses of nervous matter, are the great sources of nervous power; they are the laboratories in which the nervous power is generated." I am not satisfied with this position. I am rather inclined to regard the ganglia, great and small, the brain, cerebellum, spinal cord, and the rest, as places of deposit, store-houses of this force, in which it is gathered, concentrated, and kept ready for the purposes of the animal economy. I would agree with the great Italian, Matteucci, who points out a more probable source of the material nervous agent. This fluid, for so he considers it, he maintains to be produced by the nutritive processes everywhere; "it is developed principally in the muscles, enters continually into the nerves, and from them passes into the brain, assuming in these bodies a new state which is no longer that of a free fluid; this state is that of the nervous current which proceeds from the extremities to the brain, and returns in the contrary direction."

Radcliffe, whose speculations on this subject are peculiar, and urged with much ingenuity, confounding, as it seems, the nervous with the electric fluid, has abandoned the idea of the dynamic condition of electricity or its action by currents in the natural or passive state of muscular and other parts, and has arrived at the conclusion that it exists in these parts statically, constituting, as I understand him, an essential portion of their elementary condition. I dissent from his assumption of the identity of the two agents, built, as it is, exclusively upon coincidences occurring only in muscular contraction, one of the functions in which nerve-matter is concerned; we notice none of them in the other movements and changes dependent on the presence of such matter. I readily accept his views as to the static or quiescent condition of whatever agent is engaged in the nervous functions. The currents set in motion in all modes

of action, bear away the important element, which needs therefore to be incessantly resupplied. Thought, passion, emotion, intellection generally, consume it as well as muscular contraction: repose of both mind and body, inaction of voluntary muscle and suspension of reasoning and attention, allow of its restoration; hence the need of sleep.

What is the physical condition of the brain during sleep? This question has interested physiologists the more because of the universal impression—uncontradicted, but we must admit not absolutely proved—that it is to this great centre exclusively we must look for all explanation of the phenomena. I have long contended and still maintain that the concurrence of two circumstances is necessary to the supervention of sleep. These are a subsidence, improperly spoken of as a collapse of the cerebral organs, a suspension of their activity on the one hand, and on the other a coincident vascular fulness, probably venous, perhaps in part capillary, by which a degree of gentle pressure is made upon the cerebral substance, and passively submitted to. The bony skull is an impervious, unyielding case, which must be always full. It contains brain matter and membranes, blood of two kinds, and spino-cerebral fluid. The absolute bulk of all these taken collectively must remain the same, admitting no obvious difference, according to an inexorable physical law. But their relative amounts may vary, and as such variation is both very prompt and quite considerable, it must of necessity take place in the distribution of the two kinds of blood. Erethism, the tense state of action, depends on or implies fulness of one class of vessels, the arterial; the comparative laxity, or so-called collapse, subsidence from inaction or passivity, demands imperatively the filling of the other class, the venous, that the bulk of the intra-cranial mass be preserved, and a vacuum be avoided or prevented. Observers report the brain when laid bare and watched, to be perpetually changing its appearance, even as Holland reminds us, in the condition of comparative repose; showing local fulness and collapse alternately, from moment to moment. Blumenbach witnessed the sinking of the brain in a person whenever he was asleep, and

a swelling when he awoke. Dendy relates the story of a woman who had lost part of the skull. When she was in deep sleep the brain lay almost motionless; when she was dreaming it became elevated; when her dreams were very vivid, and when she was awake, the brain protruded through the cranial aperture. Hammond calls sleep "a function," without sufficient reason, I think. The functions of the brain are numerous and varied; but it is when least active in their performance that the state of sleep is most perfect. The experiments performed by himself and Durham, to whom he strangely refers as the original observer of facts with which the profession is familiar, go merely to confirm the statements of Blumenbach, Dendy, and Combe. But they are satisfied with half the truth, and attribute all the phenomena to the "collapse," upon which they fix their exclusive attention.

What then is this collapse of the brain, observed whenever it is inactive? It is the opposite or contrast of determination to the organ, clearly. "An affinity for, a demand, a rapid assimilation in any part acting through its vessels," says Draper, "constitutes determination of blood to a part; this is a mingled condition of nervous action and chemical change or appropriation." Solly offers us a further step in explanation. "The middle or muscular coat of the arteries in health contracts with every systole of the ventricles, just sufficiently to give solidity to the wall of the pipe, so that the force of the contraction is not lost upon a yielding surface; a much greater force is required to drive water through a leathern hose than through a leaden tube. The middle coat contracts sufficiently to assimilate the artery, physically and temporarily to the leaden tube." It is easy to see that the effect of nervous excitement upon the arterial tissue must then produce a more forcible entrance of blood into the smaller vessels and the parenchyma, and thus we have erethism, or the erectile fulness of activity—the afflux of wakefulness.

"The conditions necessary for the sleep-repose of the brain," says Handfield Jones, "seem to be essentially these two: 1. A non-excited or non-active state of the cerebral tissue. 2. In

most instances a diminished afflux of blood to the organ." But if the afflux be diminished, how is the cranial cavity to be kept in its natural condition of fulness, unless the reflux—through the veins—be proportionally retarded? He ascribes sleep to "cerebral anæmia with contraction of the arteries;" but a page or two after, he says that "drowsiness may be associated with hyperæmia, but this is almost invariably of venous character." I maintain that it is and must be of venous character. The terms anæmia and collapse are too carelessly used to express a condition not morbid; the nutritive vessels must be normally full and active; those only which supply the erethism of intellection become lax; nutrition and nervous restoration go on best—it is scarcely too much to say, only, in sleep. If there were not this suspension of erethism, this comparative arterial sinking or laxity, there could not occur the fulness or comparative congestion in the sinuses and veins which make the pressure necessary to admit of or occasion sleep; and this is the obvious explanation of the wakefulness compelled by involuntary and persistent intellection.

Durham, and, following him, Hammond, give no consideration to the physical law recognized in the old phrase that "nature abhors a vacuum." When the skull is trephined, broken into, and external pressure admitted from the atmosphere, the collapse of which they speak can occur without its violation, as protrusion may occur. But collapse—or shrinking from the entire skull without an opening—how can that take place? Durham ventures to affirm that it does happen. He fastens a watch-glass over the opening—thus restoring the integrity of the globe—makes the closure air-tight, and yet sees the brain sink away from under the glass when the animal sleeps. But if there were thus a vacuum produced between the glass and the brain surface, a great physical law has been violated. I cannot accept this conclusion, and prefer to believe that there must be some error in the record.

There is certainly an analogy between deep sleep on the one hand, and stupor or coma on the other. I do not assert their identity in nature, but their close similarity needs no remark.

Now we know that palpable pressure on the brain, however effected, will generally, if not uniformly, induce stupor. If Solly be right in ascribing the phenomena of delirium tremens to cerebral anæmia, we understand at once the terrible wakefulness of that malady, in which the deficiency of blood—if it exist as he affirms—must be made up by increase of cerebro-spinal fluid, or serous effusion, or some other mode of filling the cranial space; the sinuses and veins being undistended with the amount of fluid requisite to make the physiological compression on which sleep depends. We comprehend too why an apoplectic, if the coats of his blood-vessels have not given way and the pressure on his brain is still intravascular, shall be awakened from his stupor by venesection, or the cold douche, or strong derivatives. Extreme fatigue and long exposure to severe cold will bring on a sleep closely allied to coma in its profound stupor and insensibility, and the difficulty of arousing the subject. An attempt is made, ingenious enough, by Hammond, to discriminate from sleep the condition produced by the administration of opium and other hypnotics. The *modus operandi* of no single article of the materia medica is clearly understood; but the sleep from alcohol and opium, if not from other narcotics, resembles natural sleep as closely as the several varied slumbers under different circumstances resemble each other. One fact is worth remembering—in coma the pupil of the eye is usually dilated; opium causes contraction of the pupil. Now Mayo and Graves both tell us that in healthy sleep the pupil is contracted as it is from opium. We seek the recumbent posture uniformly as favoring sleep, thus promoting the flow of blood to the head and retarding its return. We press these conditions, when we obtain the soporific effect of rocking in a cradle; so act also the oscillations of a hammock; still more the gyrations of a circular swing, and most of all and infallibly as we are told, the motion of the body lying with feet to the centre and head to the circumference of a revolving platform; the blood vehemently sent to the head by centrifugal force.

Cruveilhier tells us that if the dura-mater in a dog is punc-

tured between the atlas and the occipital bone, and the cerebro-spinal fluid allowed to escape, the animal reels about like a drunken man and lies down for hours in a state of stupor, but the day following he is quite recovered. Here the normal and equable pressure being suddenly abstracted, the animal suffers from the derangement; blood is unduly detained in the sinuses and veins, and stupor is occasioned. In a few hours the balance is readjusted by the effusion of the normal amount of fluid.

Pressure upon the carotid produces very striking effects, with which I am personally quite familiar. To Parry we owe the recommendation of this very available method of palliating the tortures of headache. Romberg informs us that he has found this "compressing the carotids an effectual prophylactic in epilepsy, if employed in time in patients who have forewarnings." Hammond says it puts to sleep. I wish to heaven it would; but I have never known that desirable result to follow it. In my own case the pulse becomes slow, after a while intermits, and the heart labors irregularly; then ensues some dyspnœa with panting; there is some dimness of vision and dulness of hearing; intelligence and consciousness are in a degree confused or uncertain; the surface of the cheek and temple grows hot and then cold; the harsh force of the throb in the aching head is diminished, and the pain lessened or suspended; but sleep, or the inclination to sleep, is no part of my often-repeated experience.

Kussmaul and Tenner give us the result of their experiments on six male adults. "When both carotids were compressed there was pallor of the face, with loss of consciousness, dilatation of the pupils, slow, deep, sighing respiration, and in two cases of weak intellect, a choking sensation followed by vomiting and general convulsions." (H. Jones, p. 48.)

Brown-Séquard, speaking of this pressure "as a means employed against headache, congestion and inflammation of the brain," says: "This treatment was employed in the view that by stopping circulation in that blood-vessel the amount of blood in the brain was diminished. But this is far from

being the case. Whatever apparent good effect there may be from pressure in the region of the carotid, it has nothing to do in diminishing the supply of blood to the brain, but it is chiefly a pressure on the cervical sympathetic nerve, which brings about the result; for that nerve being irritated by such pressure, causes a contraction of the blood-vessels of the brain." I need not do more than point out here the self-contradictory statement "that this pressure has nothing to do in diminishing the supply of blood to the brain," when he affirms immediately after that it "causes a contraction of the blood-vessels of the brain." Surely contracted vessels contain a less amount.

But I maintain the facts to be as I have stated above; that pressure on the carotid does impede the flow of blood, the calibre of the artery being lessened, the force of impulse immediately restrained and rendered less grievous and harsh, and the general circulation becoming slower—with intermission of the pulse and of the heart's action. The tender cerebral mass is relieved by the suspension of the throbbing, and a whizzing in the vessel, subjectively perceived, manifests the obstruction. That pressure is often made on the nerve too, is obvious from the panting or sighing and general distress; but these annoyances do not always attend, and I am able sometimes to obtain its benefits entirely without them. Brown-Séquard himself refers to the difference of effects attainable. He enjoins the pressure to be carefully made, so as "to avoid the veins of the neck and trachea." He supposes Jacobi to have compressed them in his cases, thus described: "There is a feeling of burning heat which spreads suddenly over the head and neck, down to the chest—with a feeling of heat in the head in many. The face becomes darker, with frequently the production of a vivid color. An extremely painful sense of compression of the chest, a feeling of tension, weight and pain in the head, giddiness, staggering, sleepiness, sudden sleep, with stertorous breathing; in many, incipient syncope, with uncertainty in the use of the lower extremities, and sometimes sudden and entire, but transient insensibility." These are symptoms of threatening or impending apoplexy, justly ascribable to the

obstruction of return of blood from the head through the jugulars. You may in your experiment press the artery alone, or you may include the nerve, which is probably done in the usual method, or you may coarsely press artery, nerve, and vein all together, and the effects will correspond. From venous pressure will follow stupor, or sleep, or coma, with stertorous breathing and paralysis; compression of the nerve will give panting, intermission of pulse, palpitation, syncope, dilatation of pupils, heat and cold of the face and neck; of the artery, carefully made, as Parry advises, "with the thumb on a level with the superior border of the cricoid cartilage, against the vertebra," slowness of pulse, diminution of impulse, audible whizzing in the head and a sense of sinking.

In dreaming, it is probable that some external sound or touch, or some internal organic movement, stirring up, directly or by association, a definite reminiscence or feeling, arouses a corresponding portion of the brain which undergoes erethism; awaking partially, other thoughts and emotions are excited, the action extending from the intellectional to the volitional, and the dream is put into action; this is somnambulism. Or, the emotional excitement becoming paramount, what is called trance or ecstasy takes place. I have seen a patient lie in this condition for many hours, reciting meanwhile verses of melodious rhythm and deep pathos, her eyes open and highly colored and suffused, and pouring forth ceaseless floods of tears.

A sort of duplicate existence is sometimes developed under these circumstances. Long ago I published the case of Nancy Rector, who lived thus two separate lives by night and day; and have since seen, in consultation, another of the same nature. Both the young women carried on conversations which were connected relatively in the states of sleeping and waking, but did not intermingle; the memories of them being distinct, and their records kept apart fairly.

In the first of these instances there happened some curious physical phenomena, such as spontaneous dislocation of two or three of the joints from muscular action, replaced after some time, and much suffering also by muscular action; reminding

us of the accurate reports of Brodie, who directs our attention to "a peculiar relaxation of the joints in hysterical females, which is apt to give rise to sub-luxation." In the other there was an intensification of the power of vision, approaching the loftiest pretensions of the clairvoyant. The room which she dwelt in could not be darkened so as to render it difficult for her to discern any object, or even to read the most obscure manuscript.

The relations of wakefulness and sleep to diseases generally, demand attention, as of high practical importance to be known and appreciated. As a familiar rule, and with the exception of those maladies of which coma, stupor, somnambulism and convulsions form a part, the state of sleep is hailed, both as a good symptom and as favoring recovery. In many fevers and pyrexia, the desirable crisis or change portending or ushering in convalescence, occurs during sleep. It is our best hope of relief in numerous forms of agonizing headache. Neuralgia is frequently suspended by it during a thrice happy interval; and catarrhal annoyances yield, at least temporarily, to its gentle sway. Malarious fevers are kept at bay, rarely invading the sleeper, even during the day, and very seldom at night; a narcotic which will put the subject profoundly to sleep a little while before the hour of expected attack will usually avert the paroxysm of an intermittent.

It is however equally true, that the state of sleep gives predisposition or ready entrance to certain diseases. We cannot always decide between this apparent tendency on the one hand, and on the other, the nocturnal changes of air and temperature, and also specific periodicity. Croup assails generally soon after nightfall, and asthma often, perhaps from the chilliness of the hour, though I doubt this, for they awaken the subject from his first slumber, even when he has been for hours warmly sheltered and clothed. But it will surprise no one who takes the view I have presented of the analogies which associate sleep with stupor and coma, to observe the frequency of invasion of cerebral disorders at night—apoplexy, paralysis, epilepsy. Thus also we regard the occasional deaths from cardiac affections, and the horrid suffocating spasms of the

victim of hydrothorax. The normal venous fulness of sleep is prompt to become abnormal or inordinate under disturbing contingencies, as an unaccustomed heavy supper, excess in the use of stimulants, depressing fatigue, great mental anxiety or dejection; mere soft distention becomes congestion, sleep merges into stupor, stupor into coma, and coma goes on to paralysis or convulsion. In the histories of insolation—sun-stroke—more frequent in India than anywhere else, we find an almost equal number attacked by night as during the exposure and direct heat of sunshine. The cause probably impresses in the day; the exhaustion of long waking, and perhaps the posture assumed, determine the moment of development. Longmore informs us that “the invasion occurred promiscuously at every hour of the day and night.” Barclay says “the attacks of insolation came on generally when the men were in their tents; in several instances during the night, and only in one instance on the line of march. The patient had generally been lying down, often seemingly asleep, or trying to sleep, when the attention of his comrades would be directed to him by his heavy and hurried breathing, and on attempting to rouse him he would be found insensible.”

I have mentioned the familiar fact of the relief and removal of many headaches by sleep, but every practitioner meets with instances of contrasted character, in which the malady comes on during sleep, and is aggravated by every slumber, going off during the day. This may be called congestive—as the other is probably irritative or quasi inflammatory, febrile; both are neuralgic. In the former the pulse is slow and full; frequent and quick in the second.

Vertigo, which attends on headache often, is sometimes connected with drowsiness, and is capriciously related to sleep and wakefulness. Handfield Jones contends that it is aptest to occur at the moment when pressure on the brain is lessened. Thus we are giddy when we rise suddenly from stooping, or the recumbent posture, in a swing, or at sea, or when we look from a height, or at objects revolving or moving quickly, at night in darkness, and when waking from sleep. “We lean

upon our sight," says Mayo, "as on a crutch." This is prettily fanciful, but not explanatory.

The intimate connection between the condition of the nervous system and the motory function, is familiarly known. We speak of certain states of the locomotive organs as nervous diseases, and scarcely consider an argument necessary to confirm the assumption, that paralysis and spasm and convulsion depend upon morbidity of the sensorial power or the nervous substance, locally or generally. Paralysis, we say, arises from nervous inanition, suspension of nervous action. It is true that if a nerve be cut or ligated, the part it supplies does not move by the impulse of volition, which cannot reach it; but in the paraplegic we often observe, as Marshall Hall first noticed, that there is abundant capacity of motion, and that excitability, irritability, or whatever else we may call it, the *vis insita* is not diminished, nay, perhaps increased. I well remember a negro boy, æt. 16, completely paralyzed from the hips down, incapable of walking, standing, or emptying his bladder. If his lower limbs were subjected for a time to the passage of a galvanic current through them, and then the skin of his foot were touched with a sharp point, or even a drop of water made to fall upon his instep, a series of irregular, vehement, and sweeping movements of both legs would take place. These were entirely unattended with pain or any discomfort; the boy would lie on his back laughing, highly amused with these independent and uncontrollable kickings, continued for a quarter or half an hour at a time.

When the muscles of voluntary motion contract without or against volition, we call such contraction *spasm* and *convulsion*; so also when involuntary muscles contract unduly, either in degree or duration, or under new contingencies of reflex or diastaltic excitement. The two words are unfortunately used promiscuously; the books confound them everywhere, with sometimes a descriptive adjective to indicate a distinction.

Aitken speaks of convulsions as "universal muscular contraction, usually of paroxysmal or temporary duration"—and of "spasm" as "involuntary convulsive actions of less ex-

tent. Of these there are several varieties. (a.) Clonic spasm consists in rapidly alternating contraction and relaxation, as in *subsultus tendinum*. (b.) Tonic spasms, or spastic contractions, consist in contractions having a certain duration; attended with rigidity or hardness of the muscles, as in common cramps and tetanus." But tetanus, his example of tonic spasm, is as universal as any form of convulsion, and some cramps are of short duration. The contraction alone is spoken of in either case, its extent and duration. I think that there are clear and well-marked distinctions between the two phenomena, which I will endeavor to point out. I will not deny their seeming alliance, and it is possible they may occur together, but this is rare. I confess my surprise to find the confusion between them pervading the scientific treatises, both of Romberg and Handfield Jones. Under his order *Hyperci-neses*, Romberg has cardiac and gastric spasm, and spinal spasm as he calls *chorea* hypothetically; spasms from increased reflex excitability, as he calls *trismus*, *tetanus*, and *hydrophobia* also hypothetically; and static spasms from excitement of the brain, *vertigo*; and coördinated spasms, *rotatory*, *climbing* and *salaam* movements; and *psychical*, *imitative* and *epidemic*; and under the same general head of spasms from excitement of the brain, *epilepsy* and *epileptic* conditions.

Handfield Jones, massing together spasms and convulsions of all forms, and dividing them in the stereotyped way into clonic and tonic, attempts a distinction by suggesting that "the latter are more often the result of *æsthenic* excitement than the former." Not so, surely. Convulsions are frequently present in the sinking and exhausted moribund. Tetanus and cramps attend a class of deaths in which *æsthenia* is not, to say the least, the prominent condition; subjects under traumatic irritation and cholera of the briefest duration.

Radcliffe, who ascribes all spasm to "impaired vitality," looks on rigor mortis as the highest type of spasm; but I am not satisfied that rigor or rigidity should be confounded with spasm; nay, I doubt whether either implies muscular contraction. Rigor mortis does not distort the dead body as do the

choleraic cramps. When Dowler struck the biceps, it contracted in the corpse and bent the arm; but the mere rigor of the biceps does not effect this.

The most obvious and prominent distinction between the phenomena is, that spasm is always morbid, always painful, always impairs while it lasts, if it does not totally arrest or abolish the functions of the part which it affects. It implies perverted sensitiveness as well as motility; the smallest muscle, or fibre of a muscle attacked by cramp gives pain. We know not why this should be so, nor what is the essential difference between this intrusive contraction and the contraction of the same fibres under volition. Contrast the familiar enjoyment of ordinary or gymnastic exercise with the terrible sufferings of trismus, tetanus, uterine contraction, and, worst of all, the horrid suffocating struggles of hydrophobia.

Convulsions, on the other hand, though among the most appalling and repulsive symptoms of disease—the rapid alternating contraction and relaxation of opposing muscles—convulsions are not painful. Nay, convulsion is not always morbid; it often aids and protects, rather than impedes the functional actions of parts. We have examples of this in sneezing, coughing, yawning. These surely are not painful; and to the question which I have often asked of patients, I have always received a negative reply, even as to their worst form—what we call epilepsy; they are here also attended with a gracious and happy anæsthesia, passing into stupor and sound and refreshing sleep.

How are convulsions produced? What is it that arouses the tumultuous muscular contraction? Brown-Séquard maintains that the convulsing poisons act by “increasing the reflex faculty”—indirectly therefore. Strychnine may poison fatally, however, without convulsion; an animal may be *still* under its influence, until irritated in some mode, when convulsion or spasm will take place. There are several “nervous centres,” which being wounded, involuntary motion, the phenomena of “turning” often, will occur. Suppose we disturb simultaneously several of these with some poison, acting dynamically, instead of inflicting on them, in our experiments, mechanical injury, and thus

release from control many muscles—or, if the effect be excitant, irritate many at the same moment, will not the effect be necessarily a tumultuous series of muscular contractions—convulsion? Among the varieties of convulsion are sneezing, coughing, yawning already mentioned, vomiting, hiccup, eructation, sobbing, subsultus tendinum, chorea, the rigor of ague, hysterical, æsthetic or emotional, the psychical of Romberg, and the epileptic or epileptiform.

We must not omit to notice an obvious distinction between two classes of these phenomena, all alike as they are, involuntary and irrepressible contractions of muscle. Some of them are coördinated reflex movements which result in a definite and, it may be, desired effect, and are thus widely separated from the category of what we may call emphatically or in a typical sense *true* convulsions, which are disorderly and in the etymological meaning of the word “tumultuous,” awkward, clownish, such as chorea and epilepsy, and ague, and subsultus tendinum, and that uncontrollable twitching of an eyelid or a finger which occasionally besets us for an hour or a day, without obvious cause. Nay, I have seen general convulsion thus ushered in by the shaking of a finger or a limb. Many others are annoyed, like myself, at times by a sudden shock which assails us on the very verge of slumber, when on a hot day, quietly and drowsily disposing ourselves for a “forty-winks, nap.” It almost forces one to acknowledge the analogy of nervous action with electrical excitement, which this phenomenon closely resembles.

Perhaps no other conditions of the organism exhibit in so marked a manner the transition from physiological to pathological, as these. As Virchow has said of certain cells—“you do not know what they are, unless you know where they come from;” you do not know whether a man is sneezing to get rid of a pinch of snuff, or from the tickling of light or catarrh. One of the coördinate movements is uniformly morbid, purposeless and unaccountable,—hiccup. Another is rare—described in books—the salaam convulsion of the East, a regular bowing and swaying of the body. All the other coördinate convulsions are

on the middle line—involuntary, irrepressible, but not altogether purposeless, not necessarily injurious, though they may become so by repetition, fatiguing and exhausting, or under morbid causation.

Sneezing may thus uselessly and injuriously persist; I have counted in a medical friend seventy-four explosions in quick succession, and instances are recorded of more than one hundred, of course exceedingly annoying and prostrating. Coughing is plainly needed and beneficial for expectoration; but it also arises sympathetically or unintelligibly in some obscure circuitous way in which it may do much harm, directly, and indirectly. Hiccup is a topic of doubtful discussion. The diaphragm and other respiratory muscles are at first brought into play, and after a while all the abdominal parietes participate in the convulsive action. Usually arising from gastric irritation, its exciting cause is sometimes undiscoverable. I saw it in one man, otherwise well, last three days; in another seven, preventing sleep and threatening serious evil. I have just heard of a medical friend suffering from laryngeal disease, who has been afflicted for nine days with this tormentor. In 1845 I attended a lady in a miscarriage, with very large loss of blood and troublesome hiccup, supposed to be owing to her depressed and morbid condition; she informs me that she has never passed a day since—now twenty-two years and more—without a vexatious hiccup. Why it should intrude upon and agitate so large a proportion of the moribund from fever, is a problem of difficult solution. It is always an evil omen, as well as a troublesome symptom. Some poisons seem to produce it specifically; every one knows it as a familiar sign of alcoholic intoxication, and I have observed it follow the swallowing of saliva in a tobacco-chewer. Laughing, like sobbing, is a convulsion instinctive, arising from mental emotion, but also connected in some inscrutable manner with the sense of touch, as in tickling. It is as utterly uncontrollable as any other form of morbid muscular contraction. Carpenter, who classes it with sobbing and hiccup, says of it: "The muscles of expiration are in convulsive movement more or less violent, and send out the breath in a

series of jerks, the glottis being open." In excess it becomes painful, inducing cramp and almost unendurable spasm and soreness. Of all human actions it is most remarkably influenced by sympathy as we call it, imitative propensity. A striking instance of this is offered by Southey in his "Life of John Wesley." This great preacher and eloquent orator was of extremely grave and sedate habits and character. "Be serious," was one of his most frequent and earnest injunctions. "He and his brother Charles," says Southey, "when in the first stage of their religious enthusiasm, used to spend part of the Sabbath in walking in the fields and singing psalms. One Sunday, when they were beginning to set their stave, a sense of the ridiculous came upon Charles, and he burst into loud laughter. 'I asked him,' says John, 'if he was distracted, and I began to be angry—and presently after, to laugh as loud as he. Nor could we possibly refrain, though we were ready to tear ourselves in pieces, but were forced to go home, without singing another line.' Hysterical laughter and that laughter," continues Southey, "which is as contagious as the act of yawning, Wesley believed to be the work of the devil."

Chorea, "St. Vitus' Dance," which I would term paralysis vacillans; and tremor, or paralysis agitans, so frequent in old age, in all conditions of great feebleness, sometimes in consequence of certain poisons, sometimes annoying young and strong men and women, as it were spontaneously or causelessly, are examples of a combination between partial loss of power, and irregular, involuntary action of the remaining contractile power—of palsy and convulsion. We may place in the same category subsultus tendinum, which, like hiccup, supervenes in the advanced stages of exhausting disease, affecting chiefly the arms and hands.

All forms of convulsion, spasm, and paralysis, are alike regarded by Radcliffe and Anstie, as the effects of "lowered vitality." "To 1. Paralysis of the brain, belong delirium, coma. 2. To paralysis of the spinal cord, spasms, tonic, tetanic convulsions, impairment of sensation, formication, pain. 3. To paralysis of the medulla oblongata, clonic convulsions,

epilepsy, tremors, shudderings, vomiting." Hence it would seem to follow that a more profound morbidity was expressed by spasm, persistent or continuous contraction, than by convulsion, intermittent or alternating contraction.

"Ague, febrile rigor," says Handfield Jones, "must be regarded as a kind of minor convulsion," and Paget refers to instances in which this sub or minor character disappeared; cases, to use his words, "in which tetanic or epileptiform seizures took the place of rigors." Similar rigor or convulsion sometimes attends the introduction of a catheter into a sensitive urethra; the descent of a renal or biliary calculus along the ureter or ductus choledochus, and the formation of pus or supervention of pyæmia. Jones regards the phenomenon as "an example of the generation of spasm and paralysis by the same cause"—concurrently. This is another instance of the confused promiscuous employment of the word spasm for convulsion. It is easy to discriminate. The rigor is truly convulsive; it is not painful. The sensation and movement are precisely similar to the natural, normal shivering from cold in health. In malarious and other fevers—with the exception of hectic, he supposes, "that the rigor depends on the action of the poison producing them on the spinal cord, throwing it into a state of undue excitability, so that the nerves issuing from it, keep the muscles in a state of clonic contraction." He ascribes "the rigors of pyæmia and of suppurating foci to the action of contaminated blood on the nervous centres with similar results." "On the other hand," he says, "those of hectic fever are probably dependent on nervous exhaustion, the centres falling into a state analogous to that existing in chorea." But the rigors of all fevers, hectic, periodical, and catarrhal, are undistinguishably alike.

Some are disposed, with Billroth, to attribute the rigor of fever to the rapid increase of the temperature of the blood, so much dwelt on of late as its chief characteristic; but we meet with rigors often where no fever exists; we shiver with cold; we tremble with fear, and shake in the same way on the passage of a catheter. Fevers often supervene too, without any

cold stage at all. I confess myself altogether unsatisfied with any of the explanations offered, and very anxious to find better.

Nor have we yet any clear idea of the relation of nervous influence to muscular contraction. Radcliffe's theory, formerly alluded to, obtains more consideration now than it seemed at first entitled to: "Muscle left to itself, is kept in a state of relaxation by its natural electricity; it passes into contraction when the inherent attractive force of the muscular molecules for each other is, for whatever reason, no longer antagonized by that electrical action;" "muscular contraction is ordinarily brought about in this way; the natural electricity which antagonizes contraction, is extinguished for the moment by the instantaneous electrical currents of high tension (analogous to the discharge of the torpedo) which are developed when muscle or motor nerve passes from the state of inaction into that of action." To these positions it is forcibly objected, that he makes no discrimination between healthy and diseased movements; they are all produced—sustained voluntary effort, convulsion, spasm, tremor, pain, and even post-mortem rigor—in the same mode; all depend on vital exhaustion—none on vital stimulation. Denying his postulate of the identity of electric with nervous force, I reject his views.

Convulsions, emphatically so called, epileptic, hysterical, psychical, arise under conditions of the most extensive variety, nay, absolutely contrasted. It is almost universally assumed and admitted, that nervous irritability, excitability, mobility, must be increased by constitutional failure of any kind, and therefore greater of course in weak and debilitated subjects. But I never can regard disease as a mere simple question of plus and minus; there must always be present some special mode of perversion of faculty or action. It is so here. Many strong men are subject to fits; many weak invalids escape them. If mobility be dependent on debility, and results from exhaustion, the methods in which they are brought about tend to determine the results. Hall and Brown-Séquard, and the majority of pathologists, agree that the irritation must ulti-

mately act directly or indirectly upon the spinal cord—that this is the true seat and centre of convulsion. But when an athletic young fellow is seized with an epileptic fit, which soon leaves him apparently as well as ever, and as strong as average subjects of his age and weight and mode of life, he surely does not seem to be a subject of impaired spinal vitality. You may find in the phenomena presented, proofs of mobility and irritability, but it is a *petitio principii* to pronounce their essential dependence on debility.

Is there any means of distinguishing between the several varieties of convulsions—have they been separated and classified satisfactorily? We speak of and define epilepsy, but its nature is obscure; its symptoms greatly diversified, and its seat disputed. Romberg and H. Jones place it under the head of cerebral diseases, Romberg calling it a spasm from excitement of the brain, and Jones maintaining that “the determining cause of the paroxysm and its special features, is anæmia from spasm of the cerebral vessels;” on the other hand, Hall, Brown-Séquard, Anstie, and Radcliffe, refer all the phenomena to spinal derangement. “The whole order of spasmodic and convulsive diseases,” says Marshall Hall, “belong to the true spinal or excito-motory division of the nervous system.” Brown-Séquard fixes it in the spinal cord by every method of expression. Perhaps it is worthy of notice that his language is that of the most unshrinking and frank ontology. In a great many animals, by wounding the spinal marrow, he produced convulsions which he regards and describes as “epileptic.” He tells us that he “cured about a third of the animals thus experimented on,” and then proceeds to say that he “knew when an animal was *cured*, not only by the absence of spontaneous fits, but when I could not produce a fit by giving great pain. So that I am authorized to believe that when a fit was not so produced, it was because epilepsy had ceased to exist.” The tendency, predisposition or liability to be convulsed under pain, which he induced by wounding the spinal cord, he thus personifies or embodies as epilepsy.

As to the characteristic phenomena of epilepsy, it would

indeed be a wide canvas that should contain all the features which are said to characterize it. A fully developed "fit" must present—drawing of the muscles of the neck to one side, sudden insensibility, noisy and struggling respiration, alternate and violent and involuntary contractions of the muscles of voluntary movement, implying necessarily a fall or harsh precipitation to the ground, rolling upward of the eyes, strabismus, lividity of the face, with rapid contortions of the countenance, opening and shutting of the jaws, thrusting out and withdrawing the tongue, foaming at the mouth, relaxation of sphincters and action of ejaculators; these convulsions continuing from two to fifteen minutes, followed by stillness and stupor, passing into a profound sleep, from which the subject wakes unconscious of what has taken place. Not always, nor diagnostically, but often there occur varied premonitory symptoms—headache, vertigo, throbbing of temples, nausea, strange hallucinations, and odd sensations, as of "a cold creeping vapor, the epileptic aura," commencing anywhere and reaching or progressing toward the head, and just at the moment of falling a peculiar shriek. In many there is erection of the male organ and emission of semen; indeed Hippocrates long ago called the act of ejaculation terminating coitus—the "sunousia" of the Greeks, "a little epilepsy." Brown-Séquard reminds us, that "after a fracture or luxation of the vertebral column, and in men hanged, erection and emission are not uncommon." He affirms, after Segalas, having proved it by experiment on male guinea-pigs, that "a transverse section of the spinal cord and its excitement by galvanism, produce the same effects." We may ask, in general reference to the supposed analogies which connect animals of different genera and species, why it is that a decapitated chicken will be convulsed, and as Marshall Hall tells us, a decapitated calf; while a man beheaded or guillotined falls dead without such agitation.

Paroxysms of the kind described, among the most appalling manifestations of disease, may occur at long intervals; once or twice only a lifetime, and may be brought on only by rare

excitements; or, they may come on frequently, and with indefinite repetition; they may also become regularly periodical at longer or shorter intervals. One of my patients was said, on one lamentable occasion, to have had not less than a hundred and twenty-five fits in close succession, separated only by a few moments of quiet sleep or stupor.

Such is full or complete epilepsy, the "grand mal" of the French, not ill named. But there is an affection known under the same title, undoubtedly allied to the above in some obscure way, yet presenting none of its diagnostic symptoms, if any are properly diagnostic. In this, the minor epilepsy, the "petit mal," there is no shriek, no falling sickness, no convulsion, no coma, no sleep, no special lividity, or flush or paleness, no laceration of the tongue, no foaming at the mouth, no torsion of the neck, no gurgling or struggle in the breathing. The most transient reverie or trance, scarce noticeable, passing off instantly; a start as if about to lose the balance; a vacant stare, with a stolid or foolish look, what the Scotch call (we have no English word for it) "a dwam," a flash of light or a dark cloud before the eyes—this may be all. Yet this, persisting, growing more marked and enduring, intensified into vertigo, faltering, going backwards, turning round and round, with or without any peculiar sensation or aura, not unfrequently becomes developed at last into the great calamity.

Something resembling this "petit mal" may be brought on by pressure on the carotid properly managed. Indeed, comparing my own sensations under such pressure with those described by the subjects of that affection, they seem to me nearly or entirely identical.

No malady is oftener feigned by malingerers; in none have more mistakes been made on both sides. It is surely best to "give the subject the benefit of a doubt." The extreme uncertainty concerning its specific character, its nature and pathology must at once be inferred from the breadth of its vague definition and description, and from the infinite number of causes which have been said to occasion it. For my own part, I find it difficult to recognize as the same identical affection,

pathologically considered, a musculo-nervous tumult, arising in a child from the irritation of teething or worms, and that which precedes scarlatina or small-pox; in an adult convulsions from the pressure of a tumor in the brain or a spicula of bone pricking the dura mater, and those from the annoyance of a bit of glass in a nerve on the thumb, or the growth of a sesamoid bone on the great toe; convulsions from anæmic poisoning, and those from mere sympathy or imitation, as in an interesting case related by Prof. Flint, in which he makes a positive diagnosis, and pronounced the attack "a distinct epileptic paroxysm." In the same connection he relates from Prof. Dalton's personal observation, an instance of the same kind. "A dog not previously affected with epilepsy, was in company with another dog who was subject to it; the latter being seized with an epileptic paroxysm, the former immediately had a similar paroxysm."

Of the convulsions of children, we are told that "their non-recurrence affords the only proof that they are not epileptic." What name shall we give them then? What is the pathological difference? The case I have just referred to as diagnosed by Prof. Flint himself, to be "a distinct epileptic paroxysm" was non-recurrent. He tells us "she had never before had an attack of epilepsy, nor did another paroxysm take place, the person remaining within my observation for several years afterwards."

Every theoretical—pathological—explanation of the phenomena hitherto offered, is exceedingly incomplete and unsatisfactory. Take Marshall Hall's for instance. The first is alleged to be a spasm or contraction of the muscles of the neck; this obstructs the return of blood from the head; coincidently or consequently, spasm of the glottis takes place, producing suffocation, and followed by convulsion of the trunk and limbs. Now, there are at least three points unaccounted for: 1. We know not why the muscles of the neck have contracted and drawn the head to one side. We must with Brown-Séquard, presuppose the unknown something or condition, which we call epilepsy, to show any reason why the spasm should happen. 2. Equally

causeless seems the laryngeal spasm. 3. No attempt is made to point out why the suffocation or the venous turgescence in the head should give rise to convulsions. We have them without either, as where there is a tube in the larynx, and in a pale anæmic patient, and in hysteric and psychical attacks. They do not belong to the history of apoplexy or of croup. Next let us consider Todd's views, accepted as at least probable by Flint and so many other recent pathologists. "He attributes the malady to an abnormal development of nervous force, which manifests itself in the epileptic paroxysm, as a Leyden jar when charged with electricity to a certain state of tension gets rid of the disturbance (?) by the disruptive discharge. This undue force he attributes to the accumulation of some material in the blood, which acting on the brain as a poison, excites the disruptive discharge, leaving the nervous system free from disturbance, until a fresh accumulation excites a new paroxysm." (Flint, *in loco*.) This may be admitted as accounting for toxic convulsions in an emphatic sense, those connected with renal disease or paruria, the exanthematous and those following the administration of known poisons, strychnine, datura-stramonium, etc.; but surely it will not apply to Brown-Séquard's traumatic epilepsies, nor those from sexual excitement, sudden emotion, irritation, or sympathy. These humoral doctrines, however plausible, and containing doubtless an element of important truth, are very partial and limited in their application.

Handfield Jones argues, on the other hand, that "the effective causes of epilepsy are mainly dynamic, generating no poisonous matter." Copland indorses Esquirol's statement, that "fits of passion, distress of mind, and venereal excesses" hold the next rank to terror "in exciting the disease." In this connection, Maisonneuve's cases, so often referred to, are highly interesting. "Eighteen sailors, having saved themselves on a rock, by swimming away from the enemy, were for several days exposed to great privation and severe cold; four weeks after they had been received into hospital they were seized with epileptic attacks, before and after which they suffered violent pains in the right hypochondrium. When ten months had

past, six were dead; eighteen months later eight more, only four surviving." (Romberg, *in loco*.) But we are still at a loss to know how and by what modification of their impression any of these causes determine convulsion.

What is the condition of the brain and nervous system just before and at the invasion of the paroxysm—the "*causa sine qua non*?" We shall be driven, I fear, with Brown-Séquard, to the ontological conclusion that unless "epilepsy" be present, materially or metaphorically, virtually or physically, the convulsions will not follow either the fright, or grief, or anger, or pain, or heat, or cold, or pleasure, or food, or drink. "Copland's long list of premonitory symptoms," says Jones, "may be summed up as indicating, all of them, the abnormal irritability or incitability, associated with a varying blood-flow, which may or may not be in excess." The sudden and complete insensibility of the "grand mal," the giddiness and brief unconsciousness of the "petit mal," seem to him capable of being explained only "by a suddenly induced anæmia of the hemispheres, and the convulsions are consequences of the same state of the medulla oblongata and the meso-cephalon. That the cerebral anæmia is not the result of mere syncope, we are sure. There seems then nothing left but to *assume* that a sudden constriction of the cerebral arteries occurs." Thus we have the elephant adroitly placed on the back of the tortoise. But whence the initial movement; what causes or brings about the assumed indispensable spasm of the arteries? Are arteries liable to spasm?

Our author intimates that other forms of convulsion may arise from "direct cerebral irritation, but these are epileptiform, not true epilepsy." And again, "epileptiform convulsions may be brought on also by arterial spasm, acting without the previous abnormal state of the encephalon," of which he gives as example, "the malarious epilepsy" of Mackay, Payne, and Lowe. Ingenious as are these suggestions of Jones, they are by no means satisfactory. I would add to the train of connected contingencies one which he has overlooked. When anæmia takes place, as he points out from spasm of the vessels supply-

ing blood to the encephalon, there must ensue instantly a correlative fulness of the venous sinuses and trunks—from atmospheric pressure and “in horror of a vacuum,” thus inducing a condition resembling that of sleep, and indeed bringing on stupor and coma. “It is a point well worth remarking,” says he himself (933), “that when the arteries are contracted by cold, the capillaries and veins are often filled with venous blood.”

This state of things is surely very different from the syncopic character of the unconsciousness from mere anæmia. Brown-Séquard recognizes this difference distinctly enough. “The loss of consciousness,” he says, “depends altogether, in the beginning of the attack, on the contraction of the blood-vessels of the cerebral lobes, producing in these parts of the brain a state of syncope.” So positive is he on this point, that, like his illustrious predecessor, Marshall Hall, he ventures to recommend therapeutical measures of the most heroic character suggested by his theoretical dogma. As Marshall Hall advised laryngotomy to arrest the morbid movement at an early step, so Brown-Séquard proposes a method of striking out a link in the evil chain farther onward. “The loss of consciousness, which occurs in the *petit mal*,” he assures us, “may be avoided altogether by the extirpation of an inch or an inch and a half of the cervical sympathetic nerve. The teachings of physiology and pathology, and particularly the results of my experiments on animals *rendered epileptic*, conclusively show that there can be no chance for a loss of consciousness after this operation has been performed. If surgery should be bold enough to divide this nerve (which perhaps may be before long), a great advance might be made in the treatment of epilepsy.” We know that tracheotomy has not always succeeded in the prevention of convulsions, and that epileptic paroxysms have taken place when the larynx has been tubed and open. Should Brown-Séquard’s proposed excision of nerve be equally partial in effect, humanity will lose rather than gain by his suggestion; the total unconsciousness of the afflicted patient throughout the horrible tumults which agitate his frame, forms

the only feature of the shocking scene, upon which the mind can bear to dwell.

Where shall we class the convulsions which precede death from hæmorrhage? Hall regarded them as proving the exclusively spinal relations of the muscular agitation because they occurred, equally, when the animal was decapitated. I have twice witnessed strong convulsions following the use of the lancet in subjects not epileptic, bled to relieve the symptoms of violent determination to the head. There was no repetition of the attack in either case; both were adults beyond middle age; one was a man, the other a woman. Where shall we arrange the eclampsia of the parturient woman? Convulsions are not more likely to supervene during the labor of an epileptic patient, we are told, than of a woman not epileptic. On this point, I entertain some doubt; but I agree with those who maintain that an attack of puerperal convulsions does not seem to generate a tendency to recurrence, or lay the foundation for future epileptic seizures. Marshall Hall, who regards this as resembling the epileptic convulsion, offers a vague diagnosis, and tells us that "it is preceded by a hissing respiration. It is attended with great danger; the coma induced by it is very deep, and the cerebrum obviously much affected."

But epilepsy is rarely fatal directly. Now, if we read over the bills of mortality everywhere published, we shall find that they always record large numbers of deaths set down to "convulsions." In a valuable table of mortuary statistics for the city of New York, for thirteen years, "convulsions" stand second on the list of causes of death, "consumption" being of course first—37,038; convulsions 23,063. As it is very rare to see death from typical epilepsy, these convulsions must have been of different character. Indeed, a neat and precise diagnosis of epilepsy would not necessarily include convulsions, whether with Brown-Séquard we regard it as a morbid state underlying and giving rise to the tendency to convulsion, or with pathologists generally look upon the "petit mal" as one of its forms.

On the whole, I am disposed to consider convulsion as a

symptom merely, not a distinct disease in any of its forms—epilepsy, hysteria, or any other. It may be characteristic as in chorea, whose existence we can scarcely imagine without it, but is generally insignificant. It is a forerunner of death, familiarly, but I hardly think it ever the cause of death. Like hiccup and subsultus tendinum, general convulsion is often the immediate precursor of dissolution in a great number and diversity of maladies. As an unmeaning term, then, I would have it struck out of our register and bills of mortality.

I shall not presume to enter into any intimate description of the hysterical convulsion. Nothing can be more obscure, more capricious, more difficult to comprehend, more vague in diagnostic description. If not exclusively a feminine affection, it is misnamed; it certainly betrays almost uniformly its sexual origin and connection. Except the general physical hyperæsthesia and moral mobility and excitability, there are no premonitory conditions. The paroxysm comes on with elation of spirits or depression, with tears or laughter, with anger or tenderness; there is constriction about the throat—globus hystericus, the passion of poor old Lear—yawning, hiccup, pain at the epigastrium, shivering, chattering of the teeth, and general convulsion. This lasts much longer, for the most part, than the epileptic, and is often attended with hissing respiration. The tongue is never, so far as I have seen, protruded or bitten. Tears frequently stream from the eyes, which are red and suffused, and often turned up. After a time—five to fifteen minutes or more—the fit terminates abruptly, sometimes with sobbing, sometimes in sullen silence, rarely in coma or sleep. It has, however, been well ranked as the chief of the mimoses, putting on the appearance of apoplexy, epilepsy, and catalepsy. Both hysteria and catalepsy, like trance, ecstasy, and somnambulism, are conditions of the nervous system, readily brought on by mesmeric manipulations. It would require a volume to discuss them in any detail.

One of the most characteristic phenomena of the hysteric paroxysm, almost, if not absolutely diagnostic when it occurs,

yet not essential—not absolutely uniform, is the flatulent distention, so annoying, both of the stomach and intestines. I have seen the stomach so enormously filled with flatus as to press up the diaphragm and push the heart over diagonally toward the right shoulder with intense dyspnoea and much pain. It is probably dependent upon paralysis of the muscular coat of the great organ of digestion, and of the smaller bowels.

Anæmia and other debilitating causes are alleged to predispose to hysteria; all sexual irregularities excite it. No known alteration in the brain or spinal cord belongs to its history. Yet its frequently repeated occurrence always presents a degree of danger of the supervention of insanity, imbecility, or epilepsy. From the latter it is strongly contradistinguished in typical cases by its marked hyperæsthesia; while epileptics are unconscious from the invasion, in hysteria the perceptive powers are never abolished, even during violent attacks; stimulants and irritants continue to make their impression on the sensitive nerves; the patient in the fit still starts when a loud noise is made, usually hears voices about her, and shrinks from light.

It is not an easy matter to fix the pathological place of catalepsy. It is not properly either a convulsion or a spasm—the muscles seem simply rigid, yet they do not strongly resist efforts upon them to make either flexion or extension. Its alliances with other nervous affections are uncertain and obscure; closer, I think, with hysteria than any other; I once saw it associated apparently with epilepsy. Handfield Jones calls it “a disorder intermediate between epilepsy and tetanus, with both which it has affinities. It resembles much (he says) the variety of epilepsy termed tetanic.” But in tetanus the muscular contraction—a true spasm—is extremely painful. All cramp, the same author tells us, even the cramp of the swimmer, brought on by exertion or cold, is a small tetanus. We know that all cramps are painful. The rigidity of catalepsy is not painful, the consciousness not being always impaired. In cases quoted by him, consciousness was but partially abolished, and pain is not mentioned.

In chorea, I have long taught that we have a mingled condition; the phenomena "partaking as much," to use the language of Jones, "of undue mobility or convulsion, as of paralysis;" and Romberg asserts that "convulsions of epileptic character often complicate the chorea of pregnant women."

In the attempt to locate the direct or proximate cause of these dynamic disorders, Handfield Jones maintains that "much depends on the group of nervous centres most readily affected with morbid action. If it be the spinal cord, we have tetanus; if the medulla oblongata and adjacent superior region, epilepsy; if the mesocephalon alone, we have so-called hysteria; if the hemispheres alone, we have the 'petit-mal,' or apoplectiform cerebral congestion."

I will conclude with a brief notice of what has been called by Romberg "Psychical convulsion," I know no better name for it—emotional, sympathetic, religious, political, the true morbus comitialis. We have historical records of it in ancient times, of exceeding interest. It has been affirmed that the priestesses of the old pagan oracles were intoxicated by the fumes of certain volatile narcotics, and such may have been the fact. But the supposition is by no means necessary for the explanation of the incidents that took place. The mind itself is entirely capable of producing the state of trance described, with or without convulsions, by its own vehement introverted working. We know, concerning a popular writer of this city (Philadelphia), that, after having become familiar, as a very susceptible subject, with the condition brought on by mesmeric manipulation, he could in solitude reproduce in himself the same state, mental and physical, at pleasure. I once saw harsh convulsions occasioned in a healthy and intelligent gentleman, of fully ordinary firmness and strength of character, by an abortive attempt on the part of a mesmerist to put him to sleep; and I am well acquainted with a distinguished authoress, who carried about with her a charmed or magnetized gold pencil-case, by intently gazing on which she could bring on a convulsion of her whole frame, very unpleasant to witness.

It would seem, indeed, that all the forms of convulsions, vaguely described, may be excited by sympathy and imitation; it is the peculiarity of that of which I am now speaking that it has no other source. It is not connected with any defined mode of predisposition. I have not observed that anæmious or hysterical women are specially susceptible of it, nor are the weak more liable than the strong. As in ancient times religion and politics were the chief emotional excitants of the race, so they are now. But now we rarely or never meet with any such results of political excitement; our political faith is not earnest enough; our political zeal, though fiery, is not sufficiently sincere; we agitate chiefly for party predominance and selfish purposes, and our newspapers afford an outlet to the more irrepressible. The *morbus comitialis* of the classical writers—usually, but I think erroneously, referred to as epilepsy—was met with, familiarly we are told, during the heated meetings of political assemblies. Now we have a *morbus comitialis*, but it is emphatically pietistic or religious, and rarely met with outside of two well-known sects, who cultivate and favor emotional devoutness. History records many details of this affection; among them none, however, more strange than the wonderful sufferings and miraculous capacities of endurance of “the convulsionaries of St. Medard.”

I formerly took an especial interest in the manifestations of this nature in our own country, then far more common than they are now, and obtained the opportunity of examining a large number of cases. Under the eloquence of a gifted preacher, I have seen men, women, and children fall suddenly; some collapsing, as in syncope, some passive as in apoplexy, and some thrown down violently, with fierce cries and contortions, bruising themselves, hurting those with whom they came into collision, and exciting a strength surprising to all who attempted to restrain them. “At the sermons of St. Vincente Ferrer,” says his biographer, “the audience were all tears, groans, exclamations, faintings, and strange symptoms; trembling, as in agues, struck speechless and immovable as if they were statues.”

Between thirty and forty years ago, these forms of religious excitement became, so to speak, epidemic, extending widely over the Western and Southern States. "The Kentucky Jerks," as they were called, though probably commencing in East Tennessee, originally rooted in pietistic emotion, came at last to affect great masses by mere attention, and a sort of sympathy, difficult to appreciate; for it did not involve any community of sentiment or interest, but spread, like laughter and yawning, involuntarily and unexpectedly, seizing young and old, males and females, the good and bad, the indifferent and the reluctant alike. One of the noted politicians of the day frequented the meetings where these phenomena were oftenest seen, and took notes for a history of their occurrence. But he was forced to desist, from finding that he was becoming unable to write; his arms and hands being attacked with irregular and unwilling movements beyond his control, and he was glad to escape from the agitated crowd. A venerable pastor of a Presbyterian church, in which all such exhibitions are held in abhorrence, was denouncing in his pulpit with scorn, these "physical exercises" as delusions of the devil, when to the amazement and consternation of his audience, and his own unspeakable horror, he was seized violently with the jerks! One of his elders, a robust and self-willed individual, was profoundly scandalized at this event, and indulged in harsh comments upon the weakness of his unfortunate minister. The very next Sunday, however, he himself was attacked in the most humiliating manner, while walking up the aisle, in full view of the congregation. For many years after, he continued subject to these fits, of which he was so much ashamed, that if he could at any time anticipate their invasion, he would retire to the solitude of the forest, there to suffer without a witness.

I have before me a simple and graphic history from a young and intelligent preacher, of his encounter with this epidemic, and the great embarrassment it caused him. The "exercises" were curiously diversified; by no means confined to jerking or mere muscular convulsion, the subject making strange noises,

barking like a dog or a squirrel, mewling, etc. None, so far as I can learn, were fatally affected, or suffered any greater injury than the annoyance of repetition. Some seemed even to take pleasure in the "exercises," and sought the excitement that renewed them. As in hysteria, there was obviously in many of the female patients a mingled, perhaps unconscious, action of volition. Not a few of the men, and chiefly among those who had previously denounced the weakness, and were afterwards overcome by it, were full of disgust, shame, and self-reproach. I was present when a celebrated black preacher, an *élève* of the missionary Blackburn, fell prostrate in the midst of a sermon, none of his hearers being affected. He had become, by repetition, very liable to such attacks. Throughout the whole strange and painful scene he retained his consciousness, and at intervals was able to speak distinctly. He was violently agitated, and being a large and strong man, it was a difficult task to those who held him to prevent his injuring himself by the convulsive flexion and extension of his powerful limbs; and I could not help being touched with his humble apologies to them for the shocks and hurts he was inflicting upon them.

An amusing account of its irresistible domination comes to us from a gay young fellow, who became its astonished and most unwilling victim. He was the son of an elder, and to avoid going to meeting on a certain Sunday, he feigned to be sick, and was left at home alone. He lay in bed chuckling and congratulating himself, and gradually bent his thoughts on his family and their ride, reached with them in his wandering thought the camp-ground, saw the multitude assemble, "the services" commence, and at last "the exercises," as they were called, in full operation. Of course, one young woman was especially before his mind's eye, and he now fancied her under the complete influence of the familiar excitement, and in the agitation of a paroxysm. "Now!" he exclaimed to himself, with strong sympathetic interest; and in a moment he was himself taken, hurled out of bed, and jerked hither and thither all round the room, up against the wall, and in every

direction. He found himself perfectly unmanageable. He had heard that praying would cause the jerks to cease. He tried it, and the desired effect followed immediately. After panting a little he felt no more effect from it this time "than a person does after hiccup." At his ease he allowed his imagination to run riot again, and again the same scene of tumult and helplessness was acted over, only rather more vehemently. The efficacious remedy was again resorted to with success; and now thoroughly frightened, he rose, dressed himself, and sauntered about the premises. I will not pursue the story further in detail; suffice it to say, that twice more he was assailed with increased force and diminished capacity of endurance, until he was perfectly reduced to humble and prayerful submission.

It is quite clear that the malady did not extend itself exclusively by means of religious excitement. From a respected correspondent, residing in a community where it became familiar, I have the following statement: "The first of our cases originated in a church where there prevailed a vociferous mode of worship, with much devout enthusiasm, and I did not hesitate to attribute all the attacks to religious excitement. But that opinion has been shaken by the fact that the disease has attacked persons who never attended that church, and who feel no interest in the cause of religion, or seem at all concerned for the safety of their souls. Nor does sympathy seem to account satisfactorily for the wide spreading of the affection. I recollect the circumstance of an old man being attacked while under the influence of spirits, and when perfectly alone on his way from a grog-shop. In all the cases, the same muscles are alike affected, chiefly those of the neck. There was usually a paroxysm every ten minutes, during which the head was thrown backwards and forwards with astonishing rapidity." This gentleman pronounces it an epidemic chorea.

Another medical friend, writing to me, says that his patients were chiefly females, only two males being among them. The convulsions began by contraction of the muscles of the neck, were extended to the abdominal muscles, and then became general. The pulse was usually slow—from 55 to 60 in the

minute, and in some intermittent. Embarrassed to account for its broad sway over the country, he declares it "contagious."

A third, who has studied it closely and attentively, informs me that "a very singular feature of it in his neighborhood is, that it most commonly attacks strong, corpulent men, rarely assailing weak women, or indeed women of any constitution. The symptoms which usher it in are as follows: A sense of chilliness is felt throughout the whole frame, with a partial or entire loss of vision, great restlessness, and a sensation of choking or smothering; after a time the muscles begin to contract involuntarily, the subject loses his reason, throws himself often on his hands and knees, in the attitude of a dog; his eyes are turned upward, and he commences to utter the peculiar noise, which has occasioned it to be spoken of as 'religious barking and mewling;' the sounds indeed are in many instances as much like the barking of a dog as the modification of the human voice will admit. Thus he remains, unless removed by friends, until he is exhausted, when he falls to the ground, convulsed in the most violent manner. Many have been attacked who were not at all under the influence of religious excitement, and some assailed while merely listening to accounts of its mode of occurrence. In some it persists for years intermittently."

We have entered here upon the vast field of psychical pathology, in which the landmarks are indistinct, and the course of inquiry difficult and uncertain. We have neither time nor space at present to pursue a discussion of such obscurity and interest, and must postpone it until some future period, when we may hope to attain a clearer insight, to get possession of more available materials, and to have become better prepared to undertake a task so arduous.

PNEUMONIA.

WE have long been familiar with the phrase which designates a certain form of disease as "an opprobrium" to the medical profession, because of its unmanageable, incurable, fatal character. It is not uncommon even for physicians to speak in this way of cancer, phthisis, tetanus, cholera. Such use of terms, however, is irrational, absurd, and inapt. The laws of nature are beyond the control of man, and some of her movements are in themselves irresistible. The earthquake, the typhoon, the volcano, sweep away the ship, the palace, the city, in utter disregard of the skill, strength, and courage of the architect, the engineer, the sailor. No one commits the folly of denouncing them for their defeat in the contest with those gigantic forces. We admit the plea of impotence in full justification, and hold them, in fairness, to a limited and defined responsibility. It is when—as in the instance of steam employed in mechanism—it is only when they profess to have acquired a knowledge of the conduct of any force and the ability to manage, direct, and restrain its energies, that we expect of them regular and uniform results, and call them to account for failures and accidents.

Justice requires an analogous and similarly reasonable estimate of the responsibilities of the physician. Some of the obscure causes which assail the animal economy, derange its healthy action, and destroy its life, are as little within our reach as the contingencies which set in motion the tornado, let loose the lightning, and heave the ocean upon the quaking shore. Epidemics arising far away from us, traverse more or less rapidly whole continents and wide seas, invade the most varied regions, and find victims by hecatombs in all modes of life, savage and civilized. Endemics prevail for ages in particular districts, of origin so occult that the most persevering

inquiries of science are baffled ; of energy so obstinate that all means of arrest and counteraction have been foiled. Individual departures from the normal types of structure and function, develop themselves inexorably, and become marks of hereditary, tribal, and national distinction. Families and races multiply and are spread abroad, intruding themselves as exotics into unfit climates, where the air, the water, the temperature, and its alternations are hopelessly hostile to them.

We must repudiate all responsibility in contingencies such as these ; we do so likewise where men persist to defy laws of nature which we point out to them. They must perish from pestilence when they invite it by filth and crowding ; must languish under scurvy brought on by neglect of proper diet and habits, and groan with gout, the fruit of excesses.

But if our censors specify as bringing disgrace upon us a malady enveloped with no mysterious malignity, arising apparently from obvious sources and under familiar circumstances, which should display from year to year a calamitous influence upon the mortality of numerous communities ; and if, at the same time and in the same places where this disastrous energy manifests itself widely and persistently, many of the most esteemed members and practitioners of the art of healing should declare concerning it that it was of, and in itself, a mild and manageable affection ; that if left to nature it would for the most part subside spontaneously, and pass away without inflicting serious injury upon the subjects of its attacks, and that when properly cared for, very few or none would die of it,—they would unquestionably have made a strong case against us, and would surely have the right to put the whole body of our profession upon trial, and demand to know of them—Why this great mortality ? how explain the melancholy results of its fatal presence ?

It is hardly necessary to say that in the history of pneumonia, the above conditions, inexpressibly humiliating as they seem, are absolutely and precisely fulfilled ; its victims abound everywhere—and everywhere it is habitually spoken of as among our most curable and least dangerous diseases.

The most cursory perusal of our "bills of mortality," our registers of deaths and their causes, will satisfy any one that I do not exaggerate the destructiveness of pneumonia.

During the nine years, 1855-63, the deaths ascribed to this cause in the weekly records of the healthy city of Philadelphia amount to 5,100. During the last three, 1864-66, to 2,614. In this tardy spring and summer of 1867, for the week ending June 1st, to 16; while but one stands higher on the list, consumption 40. Scarlet-fever, epidemic, ranks third—13.

It is not unfair to accept this as an exhibition of the general rate of American cities. Aitken affirms that one-eighth of the deaths in the cities of continental Europe are attributable to it.

A very instructive table of statistics of New York City for 13 years—1851 to 1863—published by Cyrus Ramsay, M.D., Registrar, gives 15,138 as the number dying of "inflammation of the lungs." If we add, as certainly should be done, those set down to "congestion of the lungs," 3,230, and to "inflammation of the chest," clearly a metonymy, 168, we have a total of 18,368. To feel the full force of this statement, we must know that pneumonia stands virtually *second* on this sad catalogue. Consumption, at the head of all such documents, gives 37,038 deaths; "convulsions" are placed next, 22,886: then "marasmus," 18,696; then cholera-infantum, 16,331. But "convulsions" rarely if ever kill, are not properly named as a special disease, being generally a mere moribund symptom, and should be struck out from all such registers by the physicians giving and the officials receiving the formal certificates. "Marasmus" includes usually all the forms of atrophy, scrofulous or otherwise cachectic, and is altogether indefinite. "Cholera-infantum" is applied even more widely, so as to comprise all varieties of gastric and intestinal disorders in young children and infants—cholera-morbus, diarrhœa, dysentery, and enteritis, acute and chronic.

In his "Preliminary Report of the Census of 1860," Mr. Kennedy sums up the deaths recorded, with the causes to

which they are ascribed, during the year "from June, 1859, to May, 1860, inclusive," throughout the United States. The following is a catalogue of those which amount to 10,000 and upward:—

| | |
|----------------------|--------|
| Consumption..... | 48,971 |
| Pneumonia..... | 27,076 |
| Scarlatina..... | 26,393 |
| Typhoid Fever..... | 19,207 |
| Croup..... | 15,188 |
| Dropsy..... | 12,035 |
| Remittent Fever..... | 11,102 |
| Dysentery..... | 10,460 |
| Cephalitis..... | 10,335 |

(The meaning of this last term is not clear.)

The average mortality

| | |
|--|----------|
| | Annually |
| In Paris, from 1837 to 1848, was of pneumonia..... | 2,637. |
| In London, confounded with pleurisy from 1820 to 1831, was of pneumonia..... | 2,077. |

"Circular No. 6," an official document of high value, carefully compiled and skilfully arranged, informs us that the losses from the army of the United States during the two first years of the late war, by pneumonia, amounted to 7,091: being in 1862 at the head of the list of deaths, 2,134; in 1863, it ranked below the camp diseases developed by circumstances, typhoid fever and diarrhœa; yet reaching the large sum of 4,957.

The preserved reports of the Confederate forces state the deaths from the same cause at 5,930.

Thus we have clearly established the destructive and fatal character of the disease under discussion. We have ascertained that among the definite causes of death, recognized and announced on the authentic official registers within our reach, pneumonia, under whatever name designated—as "inflammation of the lungs"—"of the chest"—"congestion of the lungs"—"pneumonia"—"peripneumonia"—"pneumonitis"—holds uniformly and everywhere a most prominent position; carefully considered, it will be found in any long series of years second to consumption, phthisis, only. A prevailing epidemic will take

occasional precedence of it, but transiently and locally—cholera, small-pox, scarlet-fever; in certain places and seasons, yellow-fever and dysentery; in armies, during war, typhoid fever and camp diarrhœa. An apparent but unreal rivalry in numbers may be set forth under the vague phrases, convulsions, marasmus, debility, cholera-infantum—upon which I have already commented.

Yet, when we recite the language in which some of the highest authorities in our profession speak of this destructive malady, we are struck with a painful sense of contrast.

Aitken, whose work has received the unqualified approbation of his countrymen, and is already republished and widely circulated among us, takes for granted, in describing pneumonia, "the tendency to spontaneous favorable termination of uncomplicated cases, and their determinate duration."

Prof. Flint, author of the most recent American "Practice of Medicine," a much-esteemed text-book, whose careful clinical reports have obtained universal regard, and whose monographs upon several subjects have made for him a distinguished reputation, asserts in strong terms, "the intrinsic tendency to recovery, of uncomplicated pneumonitis."

Prof. J. H. Bennett, of Edinburgh, inferior to no living pathologist or practitioner, affirms roundly that all such cases recover, under his observation and care.

Prof. Caldwell, not less confident than the Scotch teacher, anticipated him, by declaring long ago, in a note in his edition of Cullen's Practice, that "it is disgraceful to a physician to suffer pneumonia to terminate unfavorably. Such an event," he quietly adds, "may be easily and certainly prevented."

We cannot contemplate without embarrassment the striking antithesis here presented. It is difficult to explain, hard to comprehend, impossible to reconcile, the discrepancy between the character of the disease thus slightly portrayed, and the stern records of disaster and death before us, in which it figures.

I know of no other malady so often and so particularly tab-

ulated, none which offers such widely different histories, none concerning which the affirmed results of observation and treatment are so contradictory, and admit so little of any form of compromise.

The proportional is not less appalling than the absolute mortality conveyed on the face of these tables, and the differences as wide, and if possible even more capricious and unintelligible. It will appear, I am persuaded, that, calculated on a large scale, the average ratio of deaths among the sick is not less—perhaps considerably more than 20 per cent. Of course it is difficult to procure any extensive number of facts of a character to be relied on. I am well aware of Quetelet's mortifying depreciation of "Medical Statistics" altogether, and I am not disposed to place any blind or implicit confidence in any of them. But we may safely allow them a certain weight, as better than general assertions or mere impressions; and they are entitled to no little consideration in making up comparative estimates. Now Blair does not think yellow-fever, a pestilence so much dreaded, fatal, when properly treated, to more than 13 per cent.; typhoid fever has been rated at 10 per cent.; bilious remittent, of which no one is willing to speak slightly, certainly does not carry off more than 5 or 6 in every hundred. In a table before me presenting 49 specifications of varied time and place and modes of treatment of pneumonia, there are 25 in which the percentage of deaths is set down as 20 per cent. and upward, the highest rate being 38—this was in the Ed. Royal Infirmary; in the King's College Hospital, in London, it was 16; in La Charité and La Pitié, in Paris, it was 29 and 35—in civil conditions and in time of peace. During the late war, it was, with very little difference, as will by and by be shown, between the Northern and Southern armies, about 20 per cent.; in the British army in the Crimea, 27, and in the French even more than this. I doubt whether any form of pestilence, cholera excepted, rises higher; that pandemic stands, we know, at or near 50 per cent. At particular times, and under special contingencies, appreciable and inappreciable, certain endemics and epidemics assume for a while an awful

malignity, impressing with almost irresistible force every subject attacked, as we see in scarlatina, diphtheria, small-pox, spotted-fever or meningitis—but these conditions are local and transient.

Seventeen other specifications run from 10 to 20 per cent., leaving, of the whole series detailed, but seven in which the proportion of deaths was below 10 per cent. Of a *decimation*, no one would speak lightly; how then can any one, unless under some strange prepossession, classify this disease, which doubles at least this array of chances against a patient, among those self-limiting, spontaneously subsiding, and tending to recovery! A strong emphasis is laid on the phrases "simple" and "uncomplicated" by Bennett, Aitken, Flint, and others; Caldwell dwelt on the importance of early treatment; the word "tendency" is employed significantly, and must be well considered. Cullen recognized pneumonia as a mere inflammation of the apparatus of respiration, not separating the investing membrane from the tissues it involved, and affixing to it, as quite sufficient, the name "pleurisy." Flint, with very similar views, calls it pneumonitis, and remarks that it so commonly extends to the pleura, that there would be no impropriety in designating it as "pleuro-pneumonia." Aitken also regards it as "inflammation of the lungs," and this appellation is employed in most of the bills of mortality to which we shall refer.

But Bennett, dissenting from these opinions, gives us an admirable definition of pneumonia as comprising numerous and varied elements: "a lesion, an exudation of liquor sanguinis; blocking up the air-vesicles with a fine molecular matter, with softening of tissue and suppuration." It is not easy to settle the precise meaning of the term "simple" in his vocabulary. He has surely delineated many complicated features in his very truthful picture of this disease. A lesion, a special exudation, a deposition of molecules, a degeneracy or softening, a new formation, as he distinctly contends, of pus-globules; these are not the constituents of a simple inflammation, which is included, on his peculiar notions of the subject, as "exudation."

As to the phrase "tendency," so often used, and with such purposed significance, I hold that it is here made suggestive of a great error in pathological philosophy. No single function, it seems to me, can be disordered in an organism, all whose movements are so indissolubly connected, whose harmonies of action are so nicely adjusted; no single structure can be altered materially without "tending" to produce further and encroaching change in other functions and structures, and so becoming in a greater or less degree complicated.

Whatever may be the primary change in the lung texture, the disturbance of the great function of respiration must interfere with and disorder all others directly connected with and dependent upon it—and what function is not? The circulation is impeded; the heart thwarted and agitated, oppressed or depressed; the blood less arterialized, imperfectly sustains the vital forces, excites insufficiently the brain and spinal cord, and supplies unfit material for nutrition and secretion. Hence an imperfect excretion or elimination of the effete molecules, which should be substituted by new ones everywhere, and thrown off, and the system becomes thus iniquated and loaded, so to speak, by its own offal. The sewerage of the body thus failing, the chlorides, urea, and other superfluous matters are injuriously retained and distributed. Such, it seems to me, is, logically considered, the "tendency" of and in pneumonia. Indeed I think that we must arrive at the conclusion, as unavoidable by any fair process of reasoning, that in the very nature of things all forms of disease must *tend* to spread wider, and strike more and more deeply, proceeding from bad to worse; unless on the one hand the force which causes them is expended and ceases to act; or on the other the morbid processes constituting them are of themselves paradoxically corrective and restorative; or there be in the character of the case, or in the original arrangements of the animal economy, some contingency provided which shall arouse a reactive and curative power—a true, direct, and genuine *vis medicatrix*. I need hardly say that I am not one of those who indulge any trust in either of the latter modes of restraining the ravages

of disease. My hopes of the restoration of a patient lie first in the belief that the energies of the force which in any case has given rise to the perversions of disease, are happily, for the most part, limited in degree and extent. The most virulent poisons may be taken without injury in small doses and well diluted, and with only moderate suffering when not too much concentrated or in too large amount. Besides this, every morbid force or cause of disease is confined in its action, and does not at once affect more than one organ or tissue; upon this its evil influence being for a time concentrated, provided it is not at once fatal, we hope to find measures of palliation and counter-action; meanwhile upholding the nutritive and assimilative function, the exclusive *vis medicatrix* by which all medicable ills are repaired, and health, if attainable, is restored. The most simple and uncomplicated pneumonia will become dangerous by spreading or assailing as double; and if it affect the lung throughout its whole mass, must prove fatal promptly. In proportion, then, to the quantity of lung tissue involved will be the risk; the patient may live upon one lung or half a lung long enough for the series of changes in which all acute disease consists to be gone through. If it be in our power to arrest the extension which always threatens; if we can aid the struggling constitution to maintain the contest for a time, we should not stand neuter. All acute diseases have their vital duration, or period of invasion, culmination, and decline; and in this vague sense all acute diseases, cholera, and yellow-fever, and the plague, manifest a "tendency" to recovery. Several of them are regular or uniform as to the time occupied in their progress, whether to death or recovery; these we properly call "self-limiting;" others vary so greatly, and are so little regular or definite in this matter, that they do not deserve the name. The former terminate obstinately and often abruptly, and among these we have recoveries by *crisis* or sudden and marked change; the latter generally by *lysis*, or slow and gradual subsidence. Pneumonia varies so much in rate of progress and duration that it can scarcely be brought into the category of self-limited diseases; recoveries are by *lysis*, or gradual subsidence,

in most cases; in some they have been considered critical, as attended or preceded by abrupt changes in the general system.

In the discussion of the questions concerning the true nature of pneumonia, whether it is to be regarded as a mere local inflammation, or a zymotic affection, or a malady *sui generis*, there is great and irreconcilable discrepancy as to the import of the actual facts or phenomena, even among those who describe them alike and in the very same terms. *Bennett* speaks of suppuration as a normal step towards health, ignoring or tacitly denying the probability or desirableness of resolution in any case, resorption of whatever exudation, lysis by quiet retrogradation. *Caldwell*, in direct opposition, regards it as disgraceful to a physician who sees his case in the first thirty-six hours, if he allow it to progress into suppuration, which he looks on as purely evil and to be evaded by all means. *Todd* holds that "the tendency is for the lung to become solid, then for pus to be generated, and at last for the pus-infiltrated lung-structure to be broken down and dissolved. Such," he says, "are the unfavorable changes." Recovery, he maintains, takes place "either through the non-completion of the solidifying process, or by the rapid removal, through absorption or a process of solution and discharge, of the new material."

We must inquire into the nature of this new material, this exudation or deposit, and learn, if we can, why and how it takes place. Two hypotheses have been offered to account for the train of events: the first supposes the previous existence of some blood disease,—“of a nature,” says *Parkes*, “not clearly known, but which consists in part of hyperfibrination, as in acute rheumatism.” The lung becoming the seat of determination, perhaps merely as the *pars minoris resistentiæ* of the individual organism, the rapid hyperæmia is productive of transudation into the air-cells, by which the blood is purified. The process is assumed to be analogous to the familiar course of gout; when it is finished, the fever ends. Then the lung-exudation softens down and is got rid of, partly by resorption, but chiefly by expectoration.

The second hypothesis makes the affection of the lung pri-

mary in fact as it is in appearance, the increase of fibrine in the blood being secondary as in other inflammation, a consequence and merely symptomatic. Neither of these views can be accepted as altogether satisfactory or sufficient. There is much to separate the case from mere inflammation of local character, and I decide, with La Roche, that pneumonia is an idiopathic, specific disease: but I see in its history no proof of previous blood-poisoning; no necessity for the elimination of any morbid material from the blood; no reason to suppose that any such excretion takes place.

A few pathologists persist in classing pneumonia among the phlegmasiæ, as Cullen did, though none absolutely with him, interfuse it with pleurisy. Aitken defines it, "inflammation of the true pulmonary tissue"—whatever that may be. Flint designates it as "Pneumonitis,"—"inflammation of the pulmonary parenchyma;" he follows Cullen in affirming its connection with pleurisy, "pleuro-pneumonia."

But the details of its history show that this is a very meagre view of its complex nature. The portraiture which Bennett has offered us of it, is more correct, as well as impressively worded, graphic, comprehensive, and highly instructive. I quote it with pleasure and admiration. "It is a *lesion* consisting of liquor sanguinis poured into the air-vesicles, minute bronchial tubes, and parenchyma of the lungs, blocking up with a fine molecular matter the air-vesicles, which are infiltrated with this exudation." This lesion, which indeed constitutes the very essence of the disease, is regarded by some as a termination, by others, as a consequence, and by others still as merely one of the successive stages, of a local inflammation. Cullen says, § 346, that pulmonic inflammation "has a termination peculiar to itself, an effusion of blood into the cellular texture of the lungs, which soon, interrupting the circulation of the blood through this viscus, produces a fatal suffocation. This indeed seems to be its most common termination when it ends fatally, for upon the dissection of almost every person, it has appeared that such an effusion has happened." In § 259, he has already spoken of "the effusion of a portion of the

entire mass of blood, either by rupture or anastomosis into the cellular texture," as a "termination of inflammation, which happens especially in inflammation of the lungs, and occasions a fatal suffocation by compressing the vessels and stopping the circulation." But no one now confounds true pulmonary hæmorrhage, "pulmonary apoplexy," as the French term it, with the special hyperæmia of pneumonia. Our acute diagnostician, Da Costa, warns us against this error. He tells us that "effusion of blood into the texture of the lung is a rare affection." Distinguishing it clearly from the exudation in pneumonia, he reminds us that "the two may coexist. Blood poured out acts as a foreign body, and around it is lighted up an inflammation of the lung-texture." We would be pleased to know whether in this case there would be true pneumonic infiltration with red and grey softening, or the familiar condition of induration with abscess, such as follows gunshot wounds or other accidental and mechanical injury.

It is important to consider in detail the exact character of the incipient changes undergone by the lung-texture in pneumonia. I must confess myself disposed with Stokes to recognize a stage of what may be called irritation, precursory to hyperæmia or congestion. Why should the vessels, arterial or venous, become distended or enlarge? The heart has no elective power of distribution, and we cannot ascribe the phenomenon to any *vis a tergo*, nor is it supposed that the vessels exert a spontaneous power of dilatation. We must look to the vaso-motor nerves as bearing the first shock of the morbid impulse which determines the whole course of subsequent events. This "paresis" of Handfield Jones, by which he accounts for so many of the symptoms of fever and inflammation, this impairment of contractility, followed of necessity by distension, is the first step. Then comes on an undoubted stage of congestion, whether intra or extra-vascular. Stokes and Andral regard it as mere vascular fulness, even after solidification has taken place; chiefly because of its rapidity of access and subsidence. Stokes holds, indeed, that pneumonia differs from bronchitis, in which we know the hyperæmia to

be simply intra-vascular or without effusion or exosmose, 'merely in the occurrence of phenomena not proceeding from any inherent difference, but from the anatomical structure of the parenchyma—such as solidification, suppuration, and abscess.' But we meet with no similar exudation in other parenchymatous inflammations. Induration—another mode of solidification—is frequent enough, but not the softening, yielding solidification of pneumonia; infiltrations,—effusions occur—but not the peculiar molecular or granular infiltration always seen here; and the rapid conversion of this material into pus is altogether specific and exclusive in this form of disease.

Some have imagined this early stage to resemble the quickly changing condition of the spleen in ague,—Piorry's "spleno-hypertrophy," in which that organ swells largely and subsides in a few minutes, or as he and Corrigan affirm, in a few seconds of time, and hence the term "splenization" employed in contradistinction to the subsequent stage of hepatization. It is not often that an opportunity offers for observing the local appearances thus early; those who have seen them say that they resemble closely, or are identical with the hypostasis of the dependent, generally the posterior portion of the lungs in the cadaver.

I do not doubt that this stage of congestion is fairly divisible into two periods; the first, an intra-vascular hyperæmia, distending the tissues until they yield; then secondly, an exosmose or exudation results, a true hæmal infiltration—the liquor sanguinis escaping; not the entire mass of blood, but a filtered plasma, with more or less fibrine and coloring matter, which pervades the cellular or connective tissue at once and uniformly; almost always the vesicular and fine tubular structure also.

Bennett and Todd differ little or not at all in their description of the stage of hepatization. The first says—in continuance of the paragraph from which I began above to quote—"the liquor sanguinis is poured into the air-vesicles, minute bronchial tubes, and parenchyma of the lungs, blocking up the air-vesicles with a finely molecular matter, occasionally in the form of minute indurations, varying in size from a millet-seed

to that of a pea, often red, sometimes yellow, and liable to be mistaken for tubercle; this molecular and granular exudation with which the above tissues are infiltrated, often forming a complete cast or mould of them, separable by washing or pressure. Sometimes these moulds are expectorated entire, and may be disengaged by teasing out the branched filaments. These, when magnified, present a fibrous exudation, in which are imbedded commencing pus-corpuscles, and more or fewer epithelial cells. Such portions of exudation as remain in the lung are transformed into pus, disintegrated and absorbed into the blood, there chemically changed and excreted, chiefly by the kidneys." It is curious to note here how skilfully he disposes of certain difficult and disputed points by the employment of nicely selected words. How is the exudation "*transformed* into pus?" Vogel makes an effort, as we shall see, to explain this transformation; Bennett gives himself no concern about it. We may ask whether there is not rather a substitution? Vogel denies that pus, as such, is ever absorbed into the blood. Bennett evades the doubt by the use of the term "disintegration," yet leaves it uncertain whether the exudation, or the pus, is thus disintegrated, nor does he define the meaning of the word. Vogel suggests the absorption of the *liquor puris*, and the conversion of what remains into granules, which, he says, are absorbable. Lastly, Bennett does not tell us what "chemical change" takes place in disintegrated pus or exudation, after it is mingled with the blood, nor point out in what form or elements it is excreted by the kidney.

Todd regards pneumonia as "that condition of the lung which leads to the formation of a plastic deposit in the cavities of the air-cells; which plastic material, by filling up the interior of these cells and the finest bronchial tubes, consolidates the previously soft and crepitant pulmonary tissue."

Williams maintains the primary seat of this specific "lesion" or "condition" to be in the capillary ramifications of the arteries and veins; the granular form of exudation or solidification he regards as vesicular; the smooth or non-granular as inter-vesicular, or, as some phrase it, interlobular. Paget, ignoring

the molecular or granular element of the infiltration, tells us: "We must not confound with hæmorrhages the cases in which the inflammatory products are merely blood-stained, *i.e.*, have acquired a more or less deep tinge of blood through the oozing of some of the dissolved coloring matter. In Pneumonia, *extravasated* blood-globules give the sputa their characteristic rusty tinge." He should rather have said, *dissolved* blood-globules. He goes on, indeed, to remark, that "the coloring matter of the corpuscles becomes unnaturally soluble, implying a cachectic, ill-maintained condition of the blood." We find, in fact, that the darker color of the sputa indicates, speaking generally, a more morbid and worse state of the patient. Stokes says that "there are no differences between the local changes in cases with prune-juice sputa and those marked with pus; but the former is met with in asthenic cases, the latter in more robust patients." And Andral says, "The prune-juice sputa (*jus de pruneaux*), usually a sign of the third degree or stage of the disease, may exist even without hepatization," which would clearly import a greater degree of solubility of the coloring matter, with less plasticity or coagulability of the exudation.

Stokes and Copland hold that the incipient change "may take place or originate in the parenchyma, and the interstitial connective tissue, though it is frequently an extension of primary bronchitis—of the small bronchi—to the air-cells and substance of the organ."

Andral describes the lung-parenchyma as consisting of "1st. Cavities, vesicles, cellules closed on all sides; 2d. The walls of these cavities, composed of a delicate membrane, on which ramify vessels and nerves, in a state of extreme fineness; 3d. The interposed cellular or connective tissue." He regards the pulmonary vesicles, not the interstitial tissue, as the true seat of pneumonia. Recognizing three stages, he calls the first "Engouement—engorgement, congestion, crepitant and without softening;" the second, "red softening;" a phrase which he prefers to "hepatization." He says: "The lung does not look like healthy liver; is compact, dense, impermeable to

air, easily lacerated. 3d. Gray softening; the little reddish granulations of the second become white or gray, and pus exudes from the granules; the lung substance is very friable." The rapidity of these changes is remarkable; the whole series may be run through in forty-eight hours. Post-mortem examinations show the transition, exhibiting different portions in the different states of red and gray softening simultaneously. He does not admit the eliminative character ascribed by some to the exudation, having met with pneumonia unattended with expectoration, and catarrhal sputa with both red and gray hepatization. Graves also offers us cases of pneumonia without expectoration "from the commencement of the disease to the period of complete resolution;" and cites an instance of viscid colorless sputa.

Flint, accepting the customary division into three stages, looks on the first as "Inflammatory engorgement, with increased density of lung-tissue, and the presence of a viscid fluid with the air, in the bronchioles and vesicles, and coexisting pleuritis. 2d. Closure by exudation of the vesicles, solidification, incapacity for collapsing in expiration, and increase of bulk and weight. 3d. Persisting solidification, with escape of puriform fluid from the cells into the bronchial tubes."

Aitken seems to recognize contrasted conditions as presenting themselves in the second stage, that of hepatization, which correspond closely with Vogel's views of the varying character of the exudation, sometimes resembling fibrinous dropsy sometimes serous dropsy. "Red softening; having lost its natural toughness, cohesion, and resistant power; gorged, loaded with watery fluid: the exudation can in some cases be readily separated or pressed out of the lung; in the other extreme of this form of inflammation, the exudation forms an integral part of the lung, which now becomes so solid, that if cut it represents with much accuracy a part of the liver or spleen; contains little or no air; does not float or crepitate; is swollen, and of a reddish-brown or deep dull red, to a violet hue. 3. Suppuration *may* follow." Bennett says *must*. He quotes Parker and Wunderlich as concur-

ring in the statement that "the consolidation of the lung is in point of time posterior to the height of the pyrexia; the latter supposes that the exudation commences with the end of the pyrexia, *i. e.*, that the defervescence commences when the lung becomes completely hepatized; whence it is inferred that the lung disease is not a primary but a secondary condition, and that it succeeds and brings to an end, by purifying the blood, a condition of general pyrexia, arising from blood disease."

Kölliker informs us (Dalton, 200) that "the pulmonary vesicles are lined everywhere with a layer of pavement epithelium continuous with that in the ultimate bronchial tubes; Henlé has established their continuity of circulation." Now, if the vesicles and bronchioles are necessarily involved in the infiltration which forms the characteristic, nay, the very diagnostic element of pneumonia, there must always be expectoration, and the sputa must always be of the composition found in the red and gray softening. But cases occur, as we learn from Andral, Graves, and other received authorities, in which there is no expectoration at all, and some in which the sputa are viscid, tenacious, and colorless, as in mere bronchitis. There are then two inferences to be drawn, clearly enough: first, that though bronchial and vesicular inflammation are usually conjoined with and present in pneumonia, yet they are not essential or uniform constituents of an attack: the same may be predicated, *à fortiori*, of pleuritis. Pneumonia may and does exist without their combination, and its primary seat is demonstrated to be in the parenchyma, the cavities and closed cellules of Andral, their walls and the interstitial connective tissue; these are always filled with the exudation, and exhibit the granular or non-granulated liver-colored solidification, diagnostically characteristic, exclusively belonging to it. Secondly, we infer that there is no purifying property in the effusion, no eliminative effort in the process; resolution and recovery take place by simple absorption—by its retrograde action, and often rapidly too, without the escape of any morbid matter by expectoration from the lung, or from any other organ by any observed critical discharge.

The nature of the exudation is variously regarded. It is not a hæmorrhage, an escape from the vessels by "rupture or anastomosis"—Cullen's language—of the entire blood unchanged. "In cases of active inflammatory disease," says Dalton, "occurring in previously healthy subjects, the blood coagulates slowly, and the clot becomes very firm." We know that the hæmal infiltration of pneumonia very promptly coagulates, and in that solid state is very friable, not firm, and very readily soluble and dissolved and resorbed. Yet there is much latitude in these changes. While we learn that all the stages of an attack may be gone through in from forty-eight to seventy-two hours, we have from Graves an instance in which the sputa, rusty from the time of admission, did not take on the appearance of suppuration until a fortnight had elapsed.

Todd considers the deposit or exudation to be "of an albumino-fibrous nature, formed, no doubt, from the liquor sanguinis, mixed with the coloring matter of the blood."

He says nothing of the molecular or granular element.

Vogel, speaking generally of indurations and softenings of organs and tissues, ascribes the first to fibrinous dropsy, in which "the fibrine coagulates and forms a solid substance, penetrating between the histological elements of the tissue; most marked in porous and spongy organs as in the lung, cellular tissue, etc. In some organs it has received special names; thus in the lungs it is termed hepatization, because to a certain degree it communicates to the pulmonary tissue a resemblance to the liver. Softening arises from hyperæmia and serous dropsy, as also from suppuration, deposition of fibrinous exudations which pass into unhealthy pus."

Andral does not regard the tissues in ordinary pneumonia as being actually disintegrated in any degree, or as having lost their normal consistence. Their friability, the apparent loss of their original tenacity, he attributes to the presence in the cells of fluid or semi-solid matter instead of air; the latter evading pressure, averts the laceration of the very delicate membrane, which when filled or half-filled with a granular or smooth half

solid coagulum, cannot either evade or resist the pressure of the finger, however slight. This view of the condition of things appears to me the only one compatible with the quick and complete restoration to health so often occurring. It would indeed seem that if any degree of positive disintegration was inflicted, every attack of pneumonia must involve slow convalescence and repair, and leave some traces, eschars, puckering, more or less loss of substance.

In reference to the relation of the several successive changes to each other, Todd observes as already quoted, that "in pneumonia the tendency is for the lung to become solid, then for pus to be generated, and at last for the pus-infiltrated lung-structure to be broken down and dissolved." "Such," he says pointedly, "such are the unfavorable changes. On the other hand, recovery takes place either through the non-completion of the solidifying process, or by the rapid removal either through absorption or a process of solution and discharge of the new material which had made the lung solid." Bennett maintains, in absolute contrast and opposition to these views, that "so far from the formation of pus being the evidence of an unfavorable course of the disease, it is the normal and necessary transformation of the solid exudation, whereby it is broken up and caused to be absorbed." He has "examined lungs exhibiting all the stages of pneumonia at once; incipient in some places, in others red and solidified, in others gray and purulent, a gradation in the pus-formation being noticeable. In the most solid hepatization young pus-cells may be observed beginning to form. I have never seen any evidence," he goes on to affirm, "that a coagulated exudation is simply disintegrated and absorbed without the development of pus-cells. These statements, easily capable of demonstration, shock certain French pathologists, and M. Grisolles denounces them; but if he had investigated the subject with the microscope, he would have convinced himself by direct proof, 1st, of the molecular exudation into the air-vesicles; 2d, the passage of this by molecular coalescence into pus-cells; and 3d, the formation and subsequent degeneration of such cells. Indeed so constant is the production of pus in

pneumonia, and so clearly can it be seen to form by molecular aggregation, independently of preëxisting cells, as, in itself, to carry with it a complete refutation of Virchow's doctrine, '*omnis cellula e cellulâ*,' or what is commonly called cell-pathology." "Pus-cells, he adds, "must be regarded as living growths, requiring an excess of blood, good nutrition, and exalted vital energy."

Here he differs from Paget, but is strongly supported by Vogel, who has gone into the inquiry more minutely than any other writer whom I have consulted. In his "Surgical Pathology," Paget carelessly, as it seems to me (*pace tanti viri*), exemplifies diffuse suppuration (Andral's gray softening) as similar or identical in erysipelas and in pneumonia. "Lymph"—(what is lymph?)—"lymph is exuded through the areolar tissues with ill-defined boundaries, most rarely with boundary-wall as in abscess. The tissues are thoroughly infiltrated with it; they are softened not only by the mixture of the unorganized inflammatory matter, but through their own degeneration, and very generally large portions of them perish." This may be true as to erysipelas, but surely does not apply to pneumonia. He goes on: "Diffuse suppuration seems due in large measure to original defect of the lymph."

Vogel insists that "the formation of pus must invariably be preceded by the exudation of a modified blood-plasma. The opinion that healthy pus can be produced from the tissues of the body by their decomposition or solution is unworthy of serious refutation. Pus cannot be formed from serous fluid containing no fibrine: only as far as it contains fibrine can extravasated blood be its source. Suppuration consists essentially in the fact that the parts of the exuded plasma, capable of such formation, undergo a peculiar organization. True pus-corpuscles are final results of a definite mode of development; they are capable of no higher stage of completion, no more perfect organism. A pus-corpuscle is never permanent, but is removed by internal resorption or by external rejection. Genuine suppuration is liquefaction with tendency to external rejection; the formation of granular cells, liquefaction with tendency to resorption. Both

may occur together, either may be converted into the other." He alone asks, "*Why* does pus arise from exuded plasma?" a fact admitted by all, discussed by few; its import regarded by Todd and Paget unfavorably; considered by Bennett and Vogel in the light of a necessary and regular sequence; as yet explained by none satisfactorily. "Why does pus arise from exuded plasma?" inquires the German; "naturally from its chemical properties, or is it dependent on external influences? Helbert gave proof of incipient formation of pus in plasma entirely separated from the body: it can be promoted or obstructed by external means, as moist warmth promotes it, cold retards and prevents it." Of its very rapid formation he asserts that "in three or four hours after its effusion the exudation may be converted into pus, though in many cases the process requires three or four days for its completion. Normal suppuration is not destructive to tissue. It is only the serum of pus which can be conveyed into the vessels unchanged, by means of resorption. Pus never occurs in veins or lymphatics, unless generated there, or admitted through openings caused by injury."

He leaves us to account for the disappearance of the diffused suppuration in the gray softening of pneumonia without purulent expectoration—which occasionally happens—by his previous suggestions, that genuine pus and granular cells are readily convertible, and that these latter are absorbed with facility.

We cannot, however, pass by without comment his dogmatic assumption that the formation of pus demands of necessity the previous exudation of blood-plasma, fibrinous, or in any other manner modified. It is not to be doubted that such exudation manifests a tendency to change into, develop, or produce pus-corpuscles. But in common bronchitis, and many other affections of organs and tissues, especially of the mucous membranes, we meet with free and abundant suppuration directly from the surfaces affected, without any such preliminary step as plastic or fibrinous effusion. Helbert's "natural or chemical" method of pus production is inadmissible and unconfirmed, and we must be content here to wait for further research. We gather from the facts, the probability of a paresis of vaso-

motor nerves as a first step; but we know not why of the lung rather than the stomach or the brain; why of the pleura or bronchi sometimes, and sometimes of the parenchyma and closed cells lying between them. We see that the exudation infiltrated becomes solidified, but we know not why or how: it is clearly not mere coagulation, no clot is so rapidly soluble, so quickly removed. We know not the source or nature of the molecular and granular matter, nor why the solidified material, granular or smooth, softens into pus-cells and liquor puris; escaping by exosmosis or otherwise through the air-vesicles and bronchioli, or simply disappearing, as we say, by resorption. On all these interesting points, it behooves us to acknowledge our ignorance, and earnestly press our investigations.

Let us now endeavor to make a practical and therapeutical application of the ascertained phenomenology and pathology of pneumonia. It is at once perceived that the contrast and opposition of the views entertained will lead to discrepant, varied, and even contrasted methods of treatment; and as each practitioner has sought eagerly for light and truth, we have been furnished with a very extensive series of statistical tables, displaying fairly, we cannot doubt, the proportional advantage of every course instituted.

Prof. Bennett, who claims boldly the highest ratio of success, is consistent generally in his reasonings and inferences; yet even he speaks loosely of conditions and crises as influencing his practice, which are not mentioned in his very minute portraiture of the disease. "Pus-cells must be regarded," he tells us, "as living growths, requiring an excess of blood, good nutrition, and exalted vital force." But this hyperæmia must be exclusively local, or his objections to the lancet, which could relieve an excess of blood in the system, are unfounded. The exaltation of vital force also must be limited to the part, for the mass of patients are in a depressed condition.

"I content myself," he continues, "with giving salines to diminish the *viscosity* of the blood." It is incumbent on us to ask what is meant by the term; in what does this morbid condition consist, and how do salines correct it. The salines—the

chlorides usually excreted by the kidney—are affirmed to be retained, and should rather be in excess therefore than deficient. We would wish to be informed whether the objectionable *viscosity* of the blood depends upon the fibrine or the albumen contained in the plasma or liquor sanguinis, or upon some change in the state or relations of the globules, red and white. Both the elements above named are necessary to the solidification of the exuded fluid, and the consequent normal change into pus according to his view ; without fibrine, Vogel tells us, such change cannot take place ; without fibrine, the blood is scarcely plastic—is thin and poor. Why diminish a quality which, to say the least, seems connected with—if it be not itself—the solidifying property or power ?

He proceeds : “ I order as much beef-tea, milk, and other nutrients as can be taken.” The patient is not likely to be nourished by what is taken, unless he is in condition to digest and assimilate it. Perhaps this is the postulate intended by the emphatic mention of “ simple, uncomplicated cases,” from which category any impairment of the digestive function removes them.

If Prof. Bennett finds any large proportion of his patients capable of taking and digesting food during attacks of acute pneumonia, he has been vastly more fortunate than most of us. Gastric disorder attends, as a general rule, in greater or less degree. Parkes dwells upon hepatic derangement also, observing that “ it is well known how frequently the liver is affected in pneumonia.” Aitken reminds us that “ jaundice is not uncommon.” All these interfere with the appetite, and run not rarely into nausea, preventing the reception of food, and prohibiting nutrition for the time.

He adds : “ As soon as the pulse becomes soft, solid food, and from four to eight ounces of wine daily. As the period of crisis approaches, I give a diuretic consisting of nitric ether half a drachm, and sometimes ten minims of wine of colchicum *ter die*, to favor the excretion of urates.”

We are not informed of the symptoms which indicate the approach of crisis, nor in what the crisis consists. The vital

duration of pneumonia has not yet been clearly made out, nor its self-limiting character established. It varies, we are told, from days to weeks. Aitken mentions several days as critical. All acute maladies have, like an epic poem, "a beginning, a middle, and an end." Some are calculable, culminating at a known period and thence regularly declining; these are properly called self-limiting; but pneumonia does not belong to this category. Even of these, not all offer a marked crisis, and if Prof. Bennett has discovered this characteristic in pneumonia, he should have described it with his well-known accuracy, and enabled us to anticipate and profit by it.

"If crisis occurs by sweat or stool, I take care," he tells us, "not to check it in any way." Thus he suggests three modes in which it may manifest itself, and lets us understand that he prefers "to favor the excretion of urates."

In all pulmonary affections, our old masters—of whom Prof. Bennett has sometimes spoken with no great reverence—were fond of diuretics. "*In morbis pectoris ad vias urinæ ducendum est*," pronounces Baglivi. They were shy of cathartics, suspected spontaneous diarrhoea, and were indifferent to diaphoretics generally. It is not rare to see free sweating go on through an entire attack, ending either way, favorably or fatally.

We have no objection to the course of treatment thus detailed by Prof. Bennett. Its uniform success in "simple, uncomplicated," and the high ratio of recoveries in all cases, must obviate all criticism. It is pleasant, too, to read of the good fortune of Barthez, who in attendance on children exclusively, lost but 2 in 212, as we learn. These physicians are entitled to look upon pneumonia as a very curable disease, with a spontaneous proclivity to end happily. Not so however those who, like myself, must acknowledge, that whether from the complex character of our cases, or whatever other unfortunate feature, our experience in this field has not been by any means so gratifying.

The prognosis of pneumonia may be presented in several aspects: 1st. Its "intrinsic tendency," as Prof. Flint has well

phrased it. A man may die of the slightest ailment, a common cold, asthma, mumps, an epileptic fit; but we neither expect nor dread an accident so rare. But is it proper to class pneumonia with attacks such as these? We have anticipated the reply in setting forth its funereal eminence on the official records. Whatever may be said of individual chances of recovery from it under conditions, one of two things is absolutely certain—either its name is wantonly or ignorantly misapplied in a vast number of instances, or it is emphatically one of the widest outlets of human life. Explain upon whatever supposition we may the undeniable facts, there will remain this reflection: pneumonia must be greatly more liable to fatal complication than any other malady, thus becoming malignant and mortal; or greatly more susceptible of being rendered so by unadapted management; especially severe and dangerous in both modes.

2d. Its prognosis compared with other diseases, and relatively to the entire total mortality: in any large mass of statistics, this has been shown to be fearfully high.

3d. Its general prognosis, the proportion of deaths to cases which it inflicts. Our tables, if gotten up in good faith, cannot fail, notwithstanding their familiar and incurable defects, to prove instructive and useful in the ratio of their extent, and the variety of circumstances under which they have been kept. We have in our hands a large abundance; let us take them for what they are worth: *quantum valeant*. I accept them from every available quarter—Bennett, Aitken, Jones, annual reports, etc. I arrive at the following results, which, I confess, have surprised me not a little:

| | CASES. | DEATHS. |
|-----------------|--------|---------|
| Bennett. | 129 | 4 |
| Louis. | 107 | 32 |
| Bouillaud. | 102 | 12 |
| Briquet. | 144 | 29 |
| Grisolle. | 232 | 37 |
| ———. | 154 | 29 |
| Dietl. | 85 | 17 |
| ———. | 106 | 22 |

| | CASES. | DEATHS. |
|--|--------|---------|
| Dietl | 109 | 14 |
| — | 750 | 69 |
| Acerbi | 142 | 16 |
| Hughes | 101 | 24 |
| Wassidlo | 112 | 4 |
| Rasori | 648 | 143 |
| Skoda | 392 | 54 |
| Wurmb | 119 | 8 |
| Tessier | 41 | 3 |
| Lebert | 205 | 15 |
| Jackson | 51 | 8 |
| Tommasini | 115 | 14 |
| Barthez | 212 | 2 |
| Huss | 2,616 | 281 |
| Bamburger | 181 | 21 |
| Flint | 133 | 35 |
| Risler | 119 | 20 |
| Kissel | 112 | 5 |
| Morehead | 103 | 32 |
| Todd | 53 | 6 |
| Mitchell | 5,909 | 1,430 |
| Von Frangue | 874 | 176 |
| Canstatt | 1,212 | 277 |
| — | 3,014 | 748 |
| Chomel | 140 | 40 |
| Wassidlo | 76 | 9 |
| Guenau de Mussy | 90 | 38 |
| Hellis | 47 | 5 |
| Ed. Royal Infirmary { before 1848 | 567 | 205 |
| { to 1856 | 611 | 131 |
| { from 1856 | 548 | 71 |
| | 20,461 | 4,086 |
| Circular No. 6, U. S. Army, 1862-3 | 31,527 | 7,091 |
| Reports (24) from hospitals in the South | | |
| during the late war | 13,246 | 3,431 |
| Hospital Reports from South Carolina, | | |
| Georgia and Florida | 1,862 | 370 |

| | CASES. | DEATHS. |
|---|--------------|--------------|
| Hospital Reports of the Army of the West and Tennessee..... | 2,957 | 542 |
| Field Reports from South Carolina, Georgia, and Florida..... | 2,120 | 127 |
| Field Reports from Gulf of Mexico... | 1,163 | 151 |
| Field Reports of the Army of the West and Tennessee..... | 6,974 | 1,090 |
| Reports for nine years from Pennsyl- vania Hospital..... | 127 | 27 |
| | <hr/> 59,976 | <hr/> 12,829 |
| Add..... | 20,461 | 4,086 |
| | <hr/> | <hr/> |
| Total of cases and deaths..... | 80,437 | 16,915 |
| Giving the proportion of deaths | 1 in 4.8 | |
| * Of the United States army in 1862-3 | 1 in 4.4 | |
| † Of the Southern forces, during the war | 1 in 4.8 | |
| ‡ In the Pennsylvania Hospital..... | 1 in 4.7 | |
| § Hospital reports, as above given | 1 in 4.9 | |
| That of the British Army in the Crimea | 1 in 3.6 | |

It requires some hardihood, I think, to contend in the face of such statistics that pneumonia is a mild disease, not dangerous in itself, not tending to a fatal termination, but disposed to spontaneous subsidence in restored health. If it be so, where shall we look for grave, serious maladies, involving risk and requiring aid?

These tables throw much light, too, on the influence of treatment and surrounding circumstances, both of which seem to me to have been greatly overrated. Observe the wonderful equality of proportional mortality—in peace and all comfort; in hospitals of wealthy communities; in the field of destructive war, and in hospitals and barracks, the emphatic seats of destitution, privation, exposure, and neglect.

*—Circular No. 6.

†—*Southern Med. and Surg. Journal*, Vol. i. No. 1. Collected by Prof. Jos. Jones, M.D.

‡—Annual Reports for 1856-7-8, 1860-1-2, 1864-5-6.

§—Bennett; Aitken; Jones.

||—Circular No. 6.

| | | | |
|--|------------------------|---|-------|
| The universal average in 80,437 cases taken | | | |
| most promiscuously, is..... | 1 in 4.8—Deaths 16,905 | | |
| The average in hospitals in 20,461 cases, in | | | |
| peaceful cities, is..... | 1 in 4.9 | " | 4,086 |
| The average in U. S. Army in 31,527 cases | | | |
| in war, 1862-3..... | 1 in 4.4 | " | 7,091 |
| The average in Southern Army in 28,007 | | | |
| cases during the late war..... | 1 in 4.8 | " | 5,811 |
| The average of nine years in Pennsylvania | | | |
| Hospital in 127 cases under most favora- | | | |
| ble conditions..... | 1 in 4.7 | " | 27 |

From Blockley almshouse I have the published reports of the last three years, previous to which the deaths only were announced, without any statement of the number of cases treated.

CASES. DEATHS.

| | | | |
|---|----|-----------|------|
| In 1864, there occurred in all the departments | | | |
| of the house, of pneumonia (simple)..... | 35 | 3 = 1 in | 11.6 |
| Including the cases set down as pleuro-pneu- | | | |
| monia | 37 | 5 = 1 in | 7.4 |
| Including the cases marked as complicated..... | 38 | 5 = 1 in | 7.6 |
| In 1865 there occurred, of pneumonia (simple).. | 33 | 7 = 1 in | 4.7 |
| Including pleuro-pneumonia..... | 38 | 8 = 1 in | 4.7 |
| Including the complicated..... | 47 | 13 = 1 in | 3.6 |
| In 1866 there occurred, of pneumonia (simple), | 39 | 7 = 1 in | 5.5 |
| Including pleuro-pneumonia..... | 67 | 18 = 1 in | 3.7 |
| Including the complicated..... | 73 | 22 = 1 in | 3.3 |

The "complications" mentioned are the "typhoid, bronchial, tubercular; with erysipelas, peritonitis, tonsillitis, bed-sores;" leaving out which we have 142 cases with 31 deaths = 1 in 4.6.

As to the influence of medical treatment, few of our tables are satisfactory; yet still much may be learned from an impartial examination of them, imperfect as they are. I have collated as fairly as possible those which, indicating however slightly the practice followed, have informed us of the success or failure consequent. Taking the universal average of deaths as nearly 1 in 5—about 20 per cent.—I have placed together

such results as are claimed in the reports, with the remedies and the names of the physicians.

OF FORTUNATE TREATMENT—LESS PROPORTION OF DEATHS.

| | | |
|-------------------------|----------|----------------------------------|
| Barthez, Children only | 1 in 106 | Course unstated. |
| Bennett, 129 cases..... | 1 in 32 | Salines, food & wine, diuretics. |
| Wassidlo, 12 "..... | 1 in 28 | Venesection. |
| Trousseau,..... | 1 in 26 | V. S., tart. antimony. |
| Kissel, 112 cases..... | 1 in 22 | Iron and copper. |
| Wurmb, 119 "..... | 1 in 15 | Homœopathic. |
| Tessier, 41 "..... | 1 in 13½ | " |
| Lebert, 205 "..... | 1 in 13⅔ | V. S. tart. antimony. |
| Dietl, 109 "..... | 1 in 13½ | Diet only. |
| Warrentrap..... | 1 in 13 | Chloroform. |
| Baumgartner..... | 1 in 10 | " |
| Huss, 2616 cases..... | 1 in 9½ | Active "heroic." |
| Todd, 53 "..... | 1 in 9 | Stimulants. |
| Acerbi, 142 "..... | 1 in 9 | V. S., tart. antimony. |
| Bouillaud, 102 "..... | 1 in 8⅔ | V. S., "Coup sur coup." |

OF UNFORTUNATE—WITH LARGE RATIO OF DEATHS.

| | | |
|-------------------------|---------|-----------------|
| Gueneau de Mussy..... | 1 in 2⅔ | Tart. antimony. |
| Morehead of Bombay..... | 1 in 3⅓ | Mixed. |
| Louis..... | 1 in 3½ | " |

From the Charity Hospital, New Orleans, I have reports for six years:

| | CASES. | DEATHS. | |
|-------------------------|--------|---------|------------|
| 1859, of pneumonia..... | 125 | 46 | = 1 in 2.7 |
| 1860..... | 175 | 94 | = 1 in 1.9 |
| 1863..... | 47 | 21 | = 1 in 2.3 |
| 1864..... | 68 | 40 | = 1 in 1.6 |
| 1865..... | 65 | 37 | = 1 in 1.7 |
| 1866..... | 98 | 35 | = 1 in 2.8 |
| Total..... | 578 | 273 | = 1 in 2.1 |

Add 32 fatal cases of typhoid and other complications, we have 1 death in 2.0.

These impartial statistics seem to prove that the result of management by any course, the effect of remedies, is by no

means uniform. Some of those most denounced appear to have been employed with benefit, or, if that be doubted, at least with no appreciable disadvantage.

A large number of our recent authorities, with Prof. Bennett at their head, have directed their special and most vehement denunciations against bloodletting. Both by *à priori* reasoning and assertion of its direct injurious action, they endeavor to establish its inappropriateness. It cannot, they maintain, promote either solidification or absorption, but rather retards them both. The normal processes of suppuration and expectoration are hindered by it, argue those who hold them to be regular and necessary; while Todd, and his stimulating followers, consider it likely to favor the degeneracy of tissue, and its melting down into pus, and to subtract from the strength requisite for the relief of the respiratory tube, by coughing. These objections are, on both sides, unduly pressed.

Venesection, a time-honored remedy for inflammation generally, has been depended on especially in thoracic inflammations. From Cullen down, however, it is admitted to be less prominently efficient in the parenchymatous affections than in those of the serous or mucous tissues. It is most directly beneficial in pleurisy, next in bronchitis and laryngitis, and less relevant in pneumonia, in the early stages of which it is, however, often useful. Few at any time, none probably now, would join with Caldwell, in urging it at all periods of the attack, even conditionally. "So long," he says, "as pain, cough, and hard pulse remain, the physician may continue to bleed. If circumstances call for it, three times in twenty-four hours are quite admissible. Three pints have been drawn in a day, with evident advantage. To prevent syncope, let the patient lie in a horizontal posture, or even with his head lower than his body." (Notes to §§ 363, 335—his edition of Cullen's Practice.)

But if we study the history of the local changes which attend or constitute pneumonia, we must observe that the cases often present simultaneously the mingled contingencies of congestion, exudation, solidification, and suppuration. If

unadapted to relieve the portion of lung softened, it may diminish in other portions the congestion and exudation, and thus check the progress or palliate the severity of diseased action, provided the state of the patient as to general vigor, or force of circulation, does not forbid it. Besides this, pleurisy and bronchitis are apt to be present, both of which are often benefited by it.

Prof. Bennett makes the success of Wassidlo next to his own, and alludes to the heroic, active treatment of Huss in no disparaging terms. Let the figures speak for themselves:

| | |
|---|---------------|
| Wassidlo, employing the lancet, lost but 1 case in 28 | |
| Trousseau " V. S. and | |
| tart. ant. (according to Aitken) | " 1 " in 26 |
| Lebert, employing the lancet, | " 1 " in 13.3 |
| Huss " | " 1 " in 9.3 |
| Acerbi " | " 1 " in 9.0 |
| Bouillaud, coup sur coup, V. S. | " 1 " in 8.6 |

The homœopathic and dietetic methods exhibit very favorable results; and the same may be said of the chloroform practice of Warrentrap and Baumgartner. Kissel boasts a still higher rate of good fortune, under the employment of iron and copper. I regret that I cannot say anything more definite of his course. Todd, whose views led him to rely exclusively upon stimulants, lost but one in nine cases.

The most varied results seem to have been connected with the administration of tartarized antimony, which, in the hands of Laennec, Rasori, and Tommassini, enjoyed for so long a time a high reputation—almost, indeed, as a specific. Lowest on our list, stands the name of Guenau de Mussy, losing one in two and two-fifths cases with this remedy, while—

| | |
|------------------|--------------------------------|
| Rasori..... | lost 1 in $4\frac{1}{2}$ cases |
| Grisolle..... | " 1 in $5\frac{1}{3}$ |
| Laennec | " 1 in 6 or 8 |
| Tommassini | " 1 in 8 |

The use of antimonials probably forms in general an element

of the "mixed treatment" which we find referred to, and, for the most part, not very favorably. Under this system,

Morehead, in the warm climate of Bombay, lost 1 in $3\frac{1}{5}$ cases

Louis, in Paris, lost..... 1 in $3\frac{1}{3}$ "

The early practice of the Ed. Royal Infir-

mary (before 1856), set down at.....1 in 3.8 "

The practice in the Georgia Hospitals during

the war (Jones).....1 in 3.9 "

To constitute this "mixed" course, more easily conjectured than defined, there are many elements, the most familiar of which, in our own country, are venesection, cups wet and dry tart. antimony, calomel, opium, veratrum viride, ipecacuanha, quinia, and counter-irritants, such as mustard, iodine especially, turpentine, cantharides. I am sufficiently catholic in my beliefs, to hold that each and every one of these has done good service, when properly handled; and if remedies, medicines, drugs, ever save life, have saved many lives, and are destined to save many more. Nor do I doubt that recoveries are possible, and have often happened without recourse to any of them. Poultices, as hot as they can be borne, and ice piled upon the chest, have been used with benefit perhaps, certainly without obvious disadvantage. Stimulants on the one hand, and on the other depressants, have received equal eulogy. Veratrum viride has many advocates; I have myself trusted to it exclusively a case which recovered promptly and perfectly. With Ames, of Montgomery, who wrote an able exposure of the capricious uncertainty and the frequent injurious irritation and depression of the antimonials, I long ago abandoned them; in their stead, I have employed in large doses, and as I thought, with excellent effect, ipecacuanha, with or without opium. Mercurials seem to me rarely if ever indicated. It is worthy of notice, that on none of our tables is the administration of quinia alluded to, although it constitutes an almost unfailing portion of the course of treatment of pneumonia, at least in our own country. Its introduction is owing to an opinion, which, originating with our South-western brethren, has become very

generally prevalent, that malaria is among the principal causes of the disease, or that at any rate it modifies its course almost everywhere, giving rise to injurious complications, which require attention and counteraction. I had thought that the excellent and instructive essay on this subject, by my erudite friend Dr. Renè La Roche, had exhausted the discussion; but we have of late had some important additions offered us, to our stock of facts and reasonings, by Prof. Jos. Jones, of Nashville, Tenn., to whose able paper and tables in the "Southern Medical and Surgical Journal," I have more than once acknowledged my indebtedness.

The well-known influences of malaria upon the organism, would lead us to expect considerable additional evil from its combination here. A periodicity is not unfrequently superinduced, which may control even the characteristic symptoms—the cough, dyspnœa, pulmonic oppression, heat of skin, cerebral and gastric disturbance, all taking on a more or less calculable movement of exacerbation and remission. In some instances, the malarial intermittent keeps its separate periods, but does not the less interfere with the well-being of the patient. The blood, impoverished and devitalized, is rendered unfit for the necessary changes which must go on in the loaded and solidified lung. Jones tells us, that the malarious alteration in the blood resembles that produced by venesection—and thus we are warned to be chary as to the employment of the lancet in malarious districts. I have always dreaded attacks of pneumonia, following a Southern intermittent or remittent fever, or assailing a constitution depressed by malarial derangement.

I know not whether we are to attribute to this complication any large share in the great proportional mortality, manifested in some of the Southern hospitals during the war, according to the tables collected by Prof. Jones. The subjects attacked, it is true, labored under infinite and varied disadvantages: fatigue, exposure, bad food, famine, absolute want of remedies in many places, as well as of clothing and shelter; medicines being declared contraband of war. But even with all these

privations, the results on the large scale are striking and consolatory. In the eight hospitals at Atlanta, Ga., the proportion of deaths varies from 1 in 2.8 to 1 in 8.3—the average of the whole being 1 in 3.8. At the Marine Hospital, in Charleston, S. C., there died 1 in 2.77; at the Citadel Square Hospital, 1 in 8.37; and in the Trapman Hospital not more than 1 in 12.5—the average being 1 in 6.8

The field reports in the same region give an extraordinarily favorable statement of recoveries, the deaths amounting to but 1 in 16.7. Men in the field were probably better supplied with food and medicines than in the barracks and hospitals, and all typhic affections seem to do better in tents than in houses.

Still more instructive and impressive is the fact, as little to be anticipated as it is difficult to explain, that the great average of proportional mortality in this disease was not higher among the destitute and necessitous soldiers of the Southern army than in the Pennsylvania Hospital in time of peace; one of the most liberally supplied and best administered institutions in existence. Nor was it as high as that of the army of the United States, well appointed and abundantly furnished with every necessary and comfort, by the greatest and most munificent government the world ever saw. It is little inferior indeed, and by a mere fraction, in comparison with that so largely collated from civil hospitals. It is decidedly better than that of the British and French forces in the Crimea.

Surely there is nothing in all this exhibit to justify Prof. Jones in the employment of the following gloomy and depreciatory language: "The mortality from pneumonia, in a large number of Confederate hospitals, was *far greater* than the mortality in this disease under different modes of treatment in European hospitals."

The very worst showing among them, 1 in 2.8 in Atlanta, and in Charleston 1 in 2.77, is not worse than that of G. de Mussy, 1 in 2.4; and the entire average is a fraction above that of the Pennsylvania Hospital, and a fraction below that of the European hospitals—1 in 4.8 against 1 in 4.7 and 1 in 4.9.

I am not inclined to enter into any farther discussion of the therapeutics of pneumonia, nor to advocate any special mode of treatment. I have known patients recover under what I regarded as the worst possible practice, and have had the pain and mortification of seeing them die under the very best methods, as I judged conscientiously and deliberately, of palliation, relief, and cure.

Nor have I any farther comments to make upon the complications and sequelæ of this grave malady; it suffices here to mention the rarity of circumscribed abscess in acute pneumonia, exclusively the subject of this essay; and the occasional occurrence of gangrene of the lung, too hastily inferred to be present from the intolerable fœtor, which may come on under other circumstances. This is indeed one of those phenomena which arise unaccountably, and embarrass us by their apparent want of connection with concurrent events. An interesting treatise might be written "upon the import of symptoms." Much stress has been laid of late upon the disappearance of the chlorides from the urine in pneumonia, and their return in convalescence, somewhat capriciously, it would seem. In one of Aitken's cited cases, there were no chlorides "in the urine on the 6th day. On the 7th and 8th, eight grammes, 120 grains, of chloride of sodium were taken by the mouth; there was none in the urine on the 8th, 9th and 10th, but on the 11th it began to reappear." It seems, like the *crepitatio redux*, to follow and denote convalescence, but neither to cause, precede, or predict it. What becomes of these salts while they are absent from the excretions, or what effect their retention may produce within the system if they are retained, we do not know. Williams says they are arrested in the inflamed parts; but this is not proved. Nor is it ascertained by analysis that they always come away with the sputa. Even if they did, we should be at a loss to account for them in cases unattended with expectoration. Todd tells us that a herpetic eruption round the mouth is a favorable event in pneumonia, and nurses accept it as such in all fevers; but no one guesses how or why. Thus we know not the value of

the taches rouges in typhoid fever, or the maculæ in typhus ; each is assumed to be diagnostic or, at least, characteristic, but neither of them influences the course of the attack in any known way. Nor have we as yet a plausible conjecture as to the connection between the pigmentary change, the darkening of the complexion in Addison's disease, and the deposit of morbid matter usually attendant upon it, in the supra-renal capsules.

The "Medical Statistics of the United States Army," prepared for the Senate, and published in two large quarto volumes, the first comprising a period of sixteen years, from January, 1839, to 1855—the second carrying on the records from January, 1855, to 1860, are full of interesting and instructive details. I collate and extract here those which refer to our subject:—

CASES. DEATHS.

| | | |
|--|-------|----------------------|
| In the Northern Division, during the entire period, there occurred, of pneumonia.. | 1,135 | 74=1 in 15.3. |
| In the Middle Division, during the entire period, there occurred, of pneumonia.. | 1,138 | 102=1 in 11.1. |
| In the Southern Division, during the entire period, there occurred, of pneumonia... | 468 | 44=1 in 10.5. |
| In Florida, Texas, California, New Mexico, Oregon, Utah and Washington Territories | 687 | 101=1 in 6.8. |
| Total..... | 3,428 | 321=1 in 11, nearly. |

Major Tulloch states the general ratio of deaths to cases in the British army as 1 in 13.

I would ask attention to the increasing proportional mortality as we go from north to south. In two of the reports (*vide* page 13 of the elder volume and page 11 of the later) we have 44 cases set down in the first, and 49 in the second, of pneumonia, without a death; and in two others (*vide* page 35 of the first and page 29 of the second) we have the coincidence of the same number of cases (96) with the same number of deaths (2), an almost unex-

amplified set of results. Both these instances of extreme good fortune or skilful practice, or both, occur at the far northern posts.

From the data before us we may infer that the frequency of occurrence of pneumonia is greatly influenced by the temperature of season, very few cases, comparatively, presenting themselves in June, July, August and September, in general. On the other hand, it seems little affected by the temperature of climate; its proportional mortality, indeed, is rather greater in southern regions, as we see in the army reports above given—those of Prof. Flint, Jones' tables, Morehead's from Bombay, and the sad statistics from New Orleans.

The special treatment, considered on a large scale, I regard as of less importance than Bennett and others would argue. The "mixed method" claims even a higher success than the "restorative course" of the Edinburgh Professor, as is shown in the U. S. Army Reports, in the two striking examples given; neither the homœopathic, expectant, nor dietetic methods offer more gratifying results.

The prudence and skill of the physician, the care and good sense of the nurse, and the proper selection of the fit remedy, decide the patient's fate: Venesection and alcohol, tart. antimon. and quinia, opium, ipecac., calomel, veratrum viride, iron and copper—each of them may do good service, or may inflict serious injury.

SCROFULOSIS AND TUBERCULOSIS.

I SHALL treat of these subjects together; for although I do not believe them to be identical, as is strongly maintained by many prominent pathologists, still at times it is almost impossible, even by abstract reasoning, to separate them. The majority of pathologists regard tuberculosis as the same thing with scrofulosis, under special circumstances and in special localities.

I think we are bound to separate them; and I think we can do this as readily as we can separate purpura from scurvy, or the various forms of typhic fever, if we fix our attention upon the prominent circumstances belonging to their history. Of scrofula, we know its mode of causation; we understand how it has gradually become so extended. Of tubercle, I believe with Williams we do not know its cause; it comes upon us under the most inexplicable conditions, under contingencies which give us no explanation. Scrofulosis is of self-development, always, necessarily; no one becomes scrofulous suddenly, no more than one becomes base suddenly (*nemo repente fit turpissimus*). Tubercles may be developed in any one of us during the progress of a typhoid fever, as reported by Bell of Glasgow and others; and any one of us may fall into consumption at once, without any preliminary warning of changes in condition and constitution, as far as we know.

So slowly is scrofula developed, however, that I do not believe that it is ever developed in one generation; I believe that preparation is made in one generation, and the development is completed in another; the fathers eat the sour grapes, and the teeth of their children are set on edge. Again, tuberculosis is not itself without deposit; we know nothing of it any more than we do of dropsy before we have the exudation or deposit, or cellular metamorphosis; it is not itself without

the tubercle, whose presence is essential. Not so with struma; there is nothing special, no peculiar deposit necessary, in a scrofulous case. Therefore I warn you against adopting too readily the doctrine of the identity of scrofula and tubercle. Examine the subject for yourselves, and decide accordingly. In the admirable work of Prof. Flint, of New York, to which I have often referred you during the present course of lectures, the doctrine of identity is taught. I regret always to differ from him; but I must entirely as to the identity of a scrofulous cervical gland and a tuberculous deposit. He speaks of the cervical gland as showing you the changes which go on in tubercle; but there is no tuberculous deposit in a cervical gland. Lloyd tells you very well the changes that occur; there is hyperæmia of the gland; it is always an inflamed gland. Prof. Flint himself says tubercle is often deposited independently of inflammation. Now no cervical gland ever enlarges without inflammation; it becomes sore, it becomes red, and by and by it suppurates.

Vogel and others tell you that scrofulosis, tuberculosis, and typhosis, are deposits of matter, of an exudation or metamorphosis of cells, whatever they may be, identical with each other. Vogel says they all consist of what he calls an amorphous struma, with earthy salts, a certain amount of fat, and a certain amount of cholesterine. There is no such deposit in many seats of inflammation in scrofulous disease. That such a deposit takes place in the lung, and wherever you find tubercle, I do not doubt. In the vast majority of cases of typhoid fever, no such deposit is clearly seen; and when it occurs the patient may die, and not by tubercle. These are views that are new to myself, views I suggest for your consideration. Is there such a thing as separate typhosis? I once believed there was, but I am now inclined to change my belief, as Woodward did not find it in typho-malarial intestines. You will not find it unless there is tuberculous deposit; and this is tuberculosis itself, as much so as tuberculosis of the lung.

This subject is open for inquiry, and will enrich research. There is no appellate jurisdiction which shall force us to settle

a question; we are in search of truth, and every new fact gives us a little more light. The discovery of the stereoscopic examination of objects is an important one; we see two sides of the object at once. Now we are beginning to look at all subjects stereoscopically as well as microscopically, and see two or three sides of the object at once; we have made a great step in the path of science. We have the opportunity of reading the beliefs of men of science always opposed to each other; thus we are likely to learn what truths cannot be controverted. In investigating this subject, let it be first decided what tuberculosis is. What is a tubercle? There is the greatest difference of opinion on this subject. One writer says that it is a nucleated, another a non-nucleated corpuscle; another says it contains molecular granules, another says it has none. Microscopists of equal ability differ on this point; one tells you it consists first of gray matter which gradually degenerates into the yellow; on the other hand Robin says the gray matter is not properly tuberculous, but a cyto-blast; while Virchow says it is a metamorphosis of a cell, and not a new growth at all! Scrofulosis is identical with what form of tubercle?—the gray? the miliary? the yellow? the fatty? the albuminous? I make these observations to suggest that there is a real difference between the two, closely as they are allied. I have no question that tuberculous deposit is rather more likely to take place in the strumous constitution, but not a great deal more. Flint himself tells you that tubercular consumption is not of more frequent occurrence in those marked by the inflammation of the cervical glands, who have suffered from scrofula in their youth, than in those who have not suffered in this way. This cervical inflammation is one of the clearest marks of scrofulous tendency, diathesis, constitution; and yet he tells you that tubercular consumption does not occur more frequently in them than in others. Nay, you find those who believe it occurs less in subjects who have suffered from inflammation of the cervical glands. I am of this opinion. The late Dr. Parrish of this city hailed the appearance of swelling of the cervical glands as one of the means of added security in such

subjects, from tubercular consumption: given the disposition, it had better take this mode of manifestation than the internal. I have lived long enough, and have had generations enough of patients under my care, to be satisfied that Dr. Parrish was right. Those who have suffered from inflammation of the cervical glands in early youth are rather less liable than those who have not, *cæteris paribus*. I am certain that the ordinary marks of inflammation about the neck, as left in the well-known cicatrices, are far less frequent now than when I began the practice of my profession. I don't speak of light degrees of the disease. I have in my note-books records of many cases, and in my memory a long list of cases of inflammation of the cervical glands of great intensity, great swelling eventuating in suppuration, and which I had to open frequently. How few of you are thus marked! The same number of individuals could not have been collected forty years ago without a considerable portion with manifestations clearly enough of having had such inflammations. Now, while these cases have been diminishing in frequency, tubercular consumption has increased in frequency; and the records I have examined show that, with us at least in this country, it is a disease of progressive mortality. Look at the rates of mortality in this city. In Prof. Wood's book on Practice, in one of the earlier editions, he alleges the influence of cod-liver oil, and states that there had been a diminution of the number of cases of tuberculous consumption upon the bills of mortality, corresponding with the increase in the use of that oil. He spoke the truth. It was a coincidence which is rather remarkable, and is quoted in the last edition of Aitken as a permanent fact. I have taken pains to collate the facts. The use of cod-liver oil has gone on, perhaps increased; it is now used as largely as ever, and yet the number of deaths from consumption has gone on increasing steadily. The annual mortality stated in the edition of Prof. Wood's Practice referred to, was rather less than nine hundred; last year it was more than two thousand; and thus it has been for the last four years. Look at the bills of mortality for yourselves, and you will see that there has been a steady increase of the

disease. I think Bennett and others maintain that it has decreased in Great Britain. I am willing to allow this, and am glad that it has so happened; but is the identity in question to be explained by this subsidence of consumption? Scrofula, they say, has also diminished. Aitken affirms that this is due to the influence of vaccination. Small-pox was a great developer of the scrofulous diathesis; and this being nearly broken down by vaccination, scrofula has become less prominent in a corresponding degree in that country. If this be true, I am very glad of it; and it does appear to be true. It also appears to be true that in the meanwhile there has not been a very great diminution, but still some diminution, in the cases of consumption. I do not think this diminution is really greater than can be accounted for as a result of the nicer discrimination in diagnosis made at the present day with the use of the stethoscope and other careful means of physical exploration. Phthisis is now put under one head, bronchitis under another, and other affections of the lungs under their appropriate heads; different affections all classed formerly under the general head of consumption. So far as I can infer, really looking for the truth, all the difference in favor of our brethren in Great Britain may be accounted for in this way. Nevertheless, what I am saying to you is intended to be suggestive rather than dogmatic. I don't wish to instil opinions in those who hear me. I desire you to think for yourselves; it is responsibility enough for me to bring before you what I think is true, without endeavoring to indoctrinate you with my own views.

Tubercular consumption is the greatest outlet of human life. If connected with scrofula as effect of cause, it is of the greatest importance to remove the cause, to remove the predisposition, which will extend; and among such causes pathologists generally hold scrofula to be. I will not deny that degree of connection, though I deny their identity.

The investigation of scrofula affords us one of those remarkably instructive methods of study in which we take a course opposite to that generally pursued in examining disease; and instead of deriving general or constitutional disease

from some local change of the same kind continuing and extending, we refer the local changes to some general or obscure condition of constitution which we call scrofulous diathesis, the occult state of causation termed struma; when it manifests itself in any particular way, it is usually with some modification of inflammation, and we call this scrofula.

No organ seems to be free from this diathesis; whether it is in the blood or in the harder tissues, is obscure. I do not think we are ready for this question as yet; it is premature to say that the existence of the *materies morbi* in the blood has been proved undoubtedly. Coleman transferred scrofulous blood from a diseased animal to a healthy one, and the healthy animal became scrofulous. He took animals of different orders—the horse and ass—and found the disease transmissible by the blood. Tubercle is equally inoculable. Klenke, and later Lebert of Breslau, and Villemin, have been successful in inoculating with the tubercular matter from consumption to the lower animals, and from one animal to another; the diseases being propagable both with the blood and by the deposit. In all these cases of occult diathesis, as in gout, carcinoma, scrofula, it may be a very important matter to know, and we shall know by and by, whether the original germ is transferred in the solid or the fluid form; whether it is the blood that circulates which conveys the disease, or whether it is the solid tissues which help to form and modify the blood. Probably both are concerned.

In the young child born of parents who have transmitted to it this peculiarity, we find that the very first movement of life will be marked by changes of condition. We have in Mr. Lloyd's Museum in London, two foetuses with tubercles of the lungs. If this be proof of scrofulous deposition, we have the proof of development of scrofula in that form; but we find that children are often born with diseased skin, affections of scalp. Soon the eye becomes affected, the lid especially, which is thickened and roughened. This we call scrofulosis. After a while there is rachitis, mollities ossium; this is called scrofulosis. It occurs in the children of parents who live in cer-

tain circumstances unfavorable to the proper development of animal life. As you go on, you find certain other affections; the heads of the bones swell and inflame—white swelling; the joints are swollen in this way. The child early shows, perhaps, spina bifida. I have seen a child under these circumstances with cleft palate, hare-lip, hydrocephalus, spina bifida, hypospadias, deformity of the genitals, all combined, and still the miserable little infant continued to live. The parents were scrofulous, and probably all these conditions concurred to the production of this universally morbid development of the child. This we call scrofulosis.

Of the three similar cachexiæ, typhosis, tuberculosis, and scrofulosis, we may regard typhosis as the most acute, tuberculosis as intermediate, and scrofulosis as the most chronic.

M. Jean Dollfus, a manufacturer in Paris, observing that the children of his workmen died more rapidly than other children in the proportion of 40 per cent. to 18 per cent., came to the conclusion that it was due to compelling the attendance of the mothers at the manufactory too soon after their confinements; that thereby they were not allowed sufficient time to attend properly to their children during their early existence. He therefore allowed them to desist from labor for six weeks following confinement, instead of four weeks, as had been the rule previously, and this brought the proportionate mortality down to 25 per cent. His example has been imitated extensively throughout France, and the result has been that the mortality among the children of such operatives has been greatly diminished; still it has not been reduced below the proportion of 25 to 18 of the general mortality.

We know that manufacturing communities in England suffer variously from scrofula; we know that their children grow up rickety, scrofulous, deformed, disabled in many ways. Good says there is no difficulty in explaining the existence of scrofula under these conditions: They are not sufficiently well fed; they are not well sheltered, they are not comfortably clothed, and they are set at labor too early. It is not long since the British Parliament enacted a bill preventing the employment of children at too tender an age. Children

of six years of age were employed at labor, and still are, in spite of the bill, in certain branches of industry. We learn this state of things from many sources, for it has been made the theme of poetry, of works of fiction, novels, etc., as well as all medical works; and, indeed, we can often learn much professionally instructive from such works; we learn that they are employed even when in bad health. Read the sad tale of "Simon's Sickly Daughter," sad enough to make any one weep; poor fellow, he takes his "sickly daughter" on his shoulder and carries her to her work, for she, too, must add her little mite toward her own support.

Under such circumstances we can understand how the poorer people shall become scrofulous.

Young says the whole of Great Britain is scrofulous, and he attributes it to the climate; but there is something in race too. The Chinese suffer as much from ochlesis, and, in many parts of their country, as much from imperfect nutrition as the English, but we do not hear of scrofula as prominent among them. The blacks in Africa suffer variously and capriciously; sometimes from famine to a great extent. Famine, we know, pervades all India at the present moment, thousands dying weekly in Hindostan. They also suffer under all the circumstances, except climate, which in Great Britain produce universal scrofula, yet they are not so subject to it. In this country we do not suffer from ochlesis except in the cities; we do not suffer from want of clothing, food, or shelter; our children are not overworked in manufactories; our laborers live well, and are in every way in a better physical condition than the laborers of Great Britain; and yet scrofula is extensively met with among us. I have said that it has diminished much within my own knowledge; it goes on diminishing on account of the increased comforts, the better hygienic conditions which science in the service of health is providing every day; still there is scrofula among us.

Knox ascribes it to our remarkable hybridism; and thus we are led to consider another cause. Unquestionably hybrid races are very generally an improvement on the original races

but it must be congenial hybridism; there is a congenial and an uncongenial miscegenation, and certain races when they mix deteriorate as much as other races improve. We improve our breeds of cattle, horses, etc., by proper crossing, and so the human animal in its various tribes can be made to improve by an appropriate crossing; but there is a certain deterioration in the mixture of certain races which we can trace with ease. The Scotchman, healthy and hardy, perhaps the hardiest specimen of the human species, mixes very readily with the Indian on our frontiers, but it is an absolute fact that their offspring are scrofulous and consumptive. Osceola died of consumption. He was the product of a healthy Indian woman and a Scotchman. The Ridge family had morbus coxarius and other scrofulous disorders, and finally consumption; and thus I could trace numerous instances which have come within my own immediate knowledge. I believe that hybridism between the Scotch and Indian produces almost always a scrofulous race. Not so with the Frenchman and Indian. The French, from the south of France at any rate, mingle with the Indian, and not badly with some of the better races of Africans; hence we have our octoroons, our quadroons, as comparatively healthy; but the mulatto is always liable to scrofula. As Nott says, he can never keep up his numbers; he will deteriorate, and especially by scrofulous degeneration. I speak from large observation, and say it deliberately, that I am ready to indorse all that he has written on this subject. The Portuguese in the island of Madeira mixed with the Indian or Malay, and the negro; the result has been a most miserable race, wretchedly degenerate in every way, and liable to scrofula. We know the degeneracy of the mingled races in Mexico, but whether they are scrofulous or not I am unable to assert; but we know as a fact that they are exceedingly degenerate, and among the common degenerations of the human race scrofula stands foremost.

From what has already been said, we can then account for scrofula very extensively; but there is a certain prevalence of scrofula difficult to account for. It is stated by a recent writer

that there are thirty-nine families in Europe out of which crowned heads are chosen; you may remember it as the number of stripes given the Israelites—forty save one; of these, a large majority are affirmed to be positively scrofulous. Why should they be scrofulous? They feed sumptuously, they are clothed in purple and fine linen, they are magnificently sheltered, and live under as good hygienic rules as are practicable; their children are not badly reared; royal families bring up their children well; they are taught gymnastics, riding, shooting; care is taken to bring them up with a view to their physical development—yet they are scrofulous. We must go far back to the original stock, whence they derive this condition. The house of Hapsburg is famous for its thick lip—and a thick lip is said to be one of the characteristic marks of scrofula. The house of Brunswick, which occupies the throne of Great Britain, is known for many years to have been subject to scrofula. We owe the fashion of wearing high stocks to a Duke of York, who endeavored in this way to hide the glands of his neck and the results of ulceration. There is nothing in the habits of these families which produces scrofula, nor has there been for a generation or two back. Some even go so far as to allow but a single healthy family to exist on a European throne, and this is the superior race in power, the house of Hohenzollern, on the throne of Prussia. As a general rule the crowned heads, the royal families, are at one end of the scale of humanity, the poor artisans in the manufactories at the other; and thus we have an exemplification of the old proverb that extremes meet. Something must be owing to climate. Almost all the crowned heads of Europe are derived from Germany; it were easy to suppose, then, that there is something in temperament and individual constitution, for the individual constitutions of that race present certain peculiarities; they are fair-haired, with blonde complexion and blue eyes; and thus we have imbibed the idea almost universally acknowledged, passing from book to book, with very little doubt or hesitation, expressed with great stress, meeting with no denial, that scrofula especially attaches itself to the light-

haired, the blue-eyed, the blonde complexion. Certainly the rule is not absolute; a crowd of instances to the contrary present themselves to my recollection. Whether from crossing, or otherwise, Holmes observed in this country, and it has been observed, too, in Europe, that this fair race is decreasing in numbers, and the dark complexion, the black-bearded races, are predominating over the entire world. In the various miscegenations taking place, this seems to be the prominent type, which will prevail ultimately everywhere; the other type dying out, perhaps from weakness originally, and therefore the more readily deteriorated. Hybridism, or crossing, has been supposed to be the cause of many evils; but on the other hand, we are warned that the marrying in and in is productive of great evils. Several writers on both sides of the Atlantic—on this side Prof. Bemis, now of New Orleans—ascribe much of tuberculosis and scrofulosis to the marrying of relatives—physical incest, as it is called. I think the truth can be put in a nutshell. I suggest it to you; there is a great deal of exaggeration on this subject, yet there is much reason for the belief that the intermarriage of relatives is dangerous to the offspring, not on account of their mere consanguinity, but because they are likely to have the same predisposition to scrofula if that predisposition exist in that family. The healthiest and hardiest race in the world, and one of the best intellectually, D'Israeli says *the* best intellectually, are the Jews. Brace says they are divided into two races, the light clear-complexioned, and the dark race. They have kept these peculiarities separate from the earliest times; they marry in and in, as we know; breed continually together. Jews seldom intermarry with the races among which they live. Gentiles seldom marry with the Jewish race. There are such exceptions, and the intermarriage does not seem to be uncongenial; they form a good race, generally mingle well together. Here then, we have an instance of long-continued intermarriage, of marriage of relatives, and without any deterioration. With regard to degeneracy in them, scrofulosis and tuberculosis are certainly not necessary results of their intermarriage.

Burton says, it is the same with the Arabs, and the Arabs are likewise divided. The Arabs are Ishmaelites, and they are divisible into two classes, the blue-eyed and the dark-eyed. Abd-el-Kader is blue-eyed and a blonde; yet some of his race are dark, even blacker than the Hindoo. These are closely intermarried—intermarried even among tribes as we learn. Among the smaller tribes of Bedouins, all marry their own cousins—there are no others to marry—yet physically they are a fine race, athletic, energetic, active. What can you desire in physical health and development more than they possess? Consanguinity is not therefore among them necessarily followed by degeneracy. We are told the same thing of the inhabitants of a certain part of France. The Basques and Bretons marry their cousins and kindred habitually, and there are no degeneracies of any kind, physical or mental, among them.

Therefore we come to the conclusion that it is not an essential result of marriage of consanguinity, that there should be scrofulous or other degeneracy. Why then does it often happen, for there is no doubt that it does often happen, that marriages of consanguinity are followed by physical or mental degeneracy? Because, if there is any predisposition to disease in a family, the female will have it as well as the male; if then, under such circumstances, two cousins of similar scrofulous predisposition marry each other, it is certain the offspring of these cousins will be more scrofulous than their parents; but it is not so by the law of consanguinity. For suppose two persons scrofulously predisposed, of the most distant and diverse race, marry, the result will be just the same without the slightest consanguinity. It is due to the predisposition and not to the blood. It is, for this reason, more apt to be encountered among married relatives: but it is not essential, it is not a law. If two cousins are healthy and see fit to marry, there is as much reason to believe that their children would be healthy, as if they were not connected by cousinship or consanguinity at all. If the temperaments be opposite, it will be as favorable a conjunction as if they were not connected. If

we could manage these things as the stock-breeder does with the lower animals, undoubtedly we could improve the human breed to a great degree.

As the scrofulous diathesis discloses itself, we find a universal predisposition to various phenomena, various morbid manifestations on the skin, the eye; the bones themselves becoming soft. Rickets we scarcely know in this country, though frequent in many other parts of the world. Softness of the bones of various kinds occurs, etc., the child being born with the defect. This has led to another suggestion. We cannot readily cure this terrible form of disease in a given individual, but we can prevent it in the offspring by removing the cause. The defect of lime is said to be the cause. Mouries discovered that the milk of women in the city of Paris contained less of earthy salts than the proper proportion. Now in the defects of the kind we are speaking of, the earthy salts are wanting too, and acting on the suggestion resulting from this investigation, he found that by sending the children out of Paris, and by procuring for them the milk of country women, this evil was abated; and the result has been, that at the present day very few children in Paris are nursed by their mothers. We learn that this is really so from works of fiction, from poetry, in sadder numbers than are recorded in our medical books. Stone, of New Orleans, believes that in all scrofulous and tuberculous cases, the phosphate of lime is an excellent remedy. Many others have maintained the same thing. Lime salts are necessary to remedy this defect, and the want of the lime salts in the maternal parent is one of the modes of accounting for the presence of the disease hereditarily.

Have we any mode of treatment for scrofulosis? Are there any means of cure? Any articles of diet that will prevent it? How shall we treat scrofula? Take any of its manifestations. One of the most interesting is the affection of the cervical glands. You find a child beginning to show a ready swelling of the lymphatic glands anywhere, no matter where; you find children very liable to enlarged tonsils, to irregular irritative diarrhoea, to sore throat, to glandular swellings externally, to

swelling of the lymphatic glands everywhere, under the arm in the axilla, along the bend of the arm; you will be able sometimes to trace from a slight blow, a swelling of the duct along the whole arm up to the axilla, inflamed, uneasy, sore to the touch. All this liability depends on a certain hereditary predisposition. Iodine is proposed for these conditions, and the various modes of administering iodine are now among the favorite remedies. I do not believe it to be so generally and universally serviceable as has been supposed. For instance, to a true development of scrofulous degeneracy it does not seem to me to be always applicable. Marasmus I hold to be one of these manifestations. A child thus affected will begin to swell with a sort of general, irregular, knotty hardness of the belly, and at last you will find the mesenteric glands quite enlarged. This goes on to an enormous extent. I have seen in one child an enlargement, a tumor, an immense mass of these glands, agglutinated together as large nearly as his head; you frequently find them as large as the fist. It is a common thing to say there is tuberculous deposit in these cases; I have never seen it. Lloyd describes the manner in which the glands of the neck are thus changed; so it is here; hyperæmia first shows itself; the glands are in a state of determination, a stagnation of blood soon followed by inflammation, then there is exudation, but not of the kind to be described as tubercular; it is a thin curdy fluid, which will sometimes form a cavity—like an abscess; but sometimes the inflammation is of ordinary character, with induration. I have repeatedly opened these glands, when I found pus as laudable in appearance as any surgeon would find in a common phlegmon. At other times it does present a curd or whey-like separation of a fluid, thin stripes of pseudo-plasm with a turbid serum.

Iodine seems very readily to affect the external glands. I don't think it relieves the tonsillitis, or the hardness, redness, and induration of the tonsil, which by and by causes necessity for extirpation; nor does it seem to benefit the subject of mesenteric glandular swelling, which is entirely out of the reach of iodine.

Mercurials, which I object to, and dislike to use under all other conditions of scrofula, which I think rather tend to irritate and disturb a scrofulous constitution otherwise affected, seem to be essentially necessary here; and yet you must avoid with great care the local irritation and inflammation, the buccal and parotid inflammation likely to be produced. Mercury must be used in small quantities. Without it I do not think you will relieve a child from this sort of irritation; marasmus, as it has been called, a wasting away, a very rapid atrophy of the whole body, shows itself, and the child emaciates in proportion to the enlargement of the glands from the injury done in the general disturbance of the assimilative powers of the constitution, which is coincident with the mesenteric swelling. I have opened these swellings after death, and found them in the state which Lloyd describes as existing in the glands of the neck. I have never seen the yellow tubercle in any of these glands; I have seen the white curdy matter which may be, I don't think that it is, identical with the gray matter of tubercle. These pseudo-plasms of scrofula and tubercle connected inextricably, are not, however, the same.

I would rather let alone even inordinately swollen cervical glands in a scrofulous subject. I do believe there is something favorable, on the whole, in their appearance, whether it be metastatical, as Parrish believed, by saving other parts; or whether it shows a not quite so great degeneracy of constitution, attacking external glands instead of internal glands, I am not prepared to say. It is a better condition of things than internal manifestations; frequently they remain indolent at a certain point for a long time. Wait upon them; follow the practice of one of our late physicians, who advises you "if it lets you alone, let it alone." They frequently disappear of themselves, subside completely; and thus is accounted for, the value of the "royal touch," which is one of the earliest superstitions we know of. If we trace its history closely, we find that it was employed among barbarous nations. Vespasian was supposed to have this power of gracious touch, by which he also relieved blindness and paralysis, etc. "*Statim con-*

versa ad usum manus, ac cæco dies reluxit," says Tacitus: "The hand recovered its power and the day shone again on the blind." Laird tells us that King Olaf, one of the earliest of the Norwegian kings, had this singular power. He took a piece of bread, rolled it, made the form of the cross, put it in the mouth of the patient; he then stroked the neck and the glands disappeared, and thus he showed the royal power he derived from heaven. We know that the kings of England employed the royal touch, and the queen down as late as the time of Dr. Johnson. We know of other persons who have enjoyed this power. Valentine Greatrakes has left a name in history on account of the number of cures that occurred under his hands.

Scrofulous persons, after all, grow up very often in good health, get rid of the scrofulous affection of the eye, of the skin, and of the glands.

Internal scrofula is not so readily got rid of; nor are the scrofulous affections of the bones. Toynbee says that the general deafness met with in cities, especially where ophthalmia prevails, is also a scrofulous affection. Iodine is used in all these, and in all the external manifestations it seems to be singularly applicable. Whether goitre is one of these or not, is much disputed. At any rate, it is much under the influence of iodine. How shall the iodine be administered? There are many modes. I prefer Lugol's solution, very weak; a grain to the quart of water, and administered in doses of a wine-glassful two or three times a day. Thus you avoid the irritative absorption of healthy glandular structure which sometimes follows the administration of iodine. The deutiodide of mercury and potash is one of the best preparations and very effective. All the salts of potash are serviceable, upon a particular view which Garrod gives us. He says all the alkaline salts tend to promote the natural metamorphosis of tissue going on in the system; that under the influence of the alkalies there is a more ready metamorphosis of the less vital tissues, less vital deposits, less vital plasma, and of course the deposition forming the glandular and other swellings of scrofula. I have

no doubt he is right. A combination of salts, such as we have in sea-water, I have found excellent. Recent sea-water, taken every day, in small quantities, will improve the health. All the natural impregnations of mineral waters which go to assist the metamorphosis of tissue, are beneficial, and upon that general principle. I know no other to guide us; there is no specific, unless iodine is a specific; but it acts, I think, on this general principle of Garrod.

With regard to the diet much may be done. Bennett suggested long since that such depositions are chiefly albuminous; many books call them fibrinous; but there is very little difference between the two in our view. In either case it will be of great consequence to avoid albuminous food. Bruce also says that they contain too much caseine among the proteine compounds. Beneke confines his patients to a diet of whey; subtracting from the milk the whole of the albumen, leaving the sugar and other ingredients, getting out the curd, he gives the whey. It is said the cures under his hands have been marvellous. Very little bread is allowed, great quantities of whey being drunk. Even the deposit of tubercle is said to disappear under this *régime*.

Tuberculosis differs from scrofulosis in this, that supposing a general constitutional deterioration or cachexia, it has but one manifestation; we know it but one way, and that is by the deposit of a particular matter. It is, as Vogel calls it, a protein-compound consisting of an amorphous stroma (which is at first probably deposited in a fluid state), with some earthy salts, and with a certain amount of fat. There is albumen, as the base, or fibrine, unquestionably. We find it in various conditions, very rarely in the fluid state; rarely in the colloid or jelly-like; yet it is sometimes found so, as is affirmed by Carswell and others. When we first meet with it, generally it is as the gray or miliary tubercle. What is it? Who knows? Microscopists have failed to tell us clearly; the chemists have failed; there is great difference of opinion as to its nature. Addison says it is the white corpuscle of the blood "stagnated and converted;" Virchow says it is "inspissated pus;"

if so, a blood corpuscle, a pus corpuscle, and a tubercular corpuscle are the same. You will find if you trace up the various opinions and conclude that concrete pus and tubercle are the same, you have nothing but the corpuscle to constitute this peculiar morbid matter, the very presence of which strikes every one with dread. At first we find it as gray matter; it soon degenerates into the yellow tubercle, distinguished chemically by a greater amount of adipose matter. Is this a degeneracy? If so, it would seem that we do not follow nature when we give the oily diet to a tuberculous patient. Robin doubts whether this gray matter is tubercle; he thinks it can be distinguished from it, and that tubercle is always deposited as the yellow matter. You find this gray matter in many positions in which the yellow is never found, as, for instance, in tubercular meningitis, to which many ascribe the presence of hydrocephalus. These little points which have been considered tubercular are of the gray tubercle, and never degenerate; the yellow mass is never found there. Virchow speaks of tubercle very contemptuously; he says it is a pitiful birth, a very small cell. The tuber never enlarges; the tubercle enlarges by agglutination, only differing from the cancer cell because that is larger. Virchow says, "If you do not know where it comes from, you do not know what it is;" and that tubercle is little else than concrete pus; and as the cancer cell differs only in size, you have small difference histologically between a pus corpuscle and a cancer cell.

As we meet with tubercle, it is most frequently found in the lung, and upon mucous surfaces. Carswell contends that it is upon the mucous surface that it is usually deposited. It takes the mould of the part on which it is placed. It must be deposited in a fluid state, but goes on to accumulate in masses. Louis says you will never meet with it elsewhere without finding it in the lung also. Whenever you do meet with tubercle, it seems to have a particular tendency to fix itself in one part of an organ. Thus it is much more likely to be found in the apex of the lung than in any other part; so in a particular part of the brain rather than any other; it is

found in the ileum rather than in any other part of the intestine. It is found in the cavity of the uterus at times. There is no organ of the body in which tubercle may not be deposited; but wherever found, it has a particular locality in reference to the structure of that organ where it is usually found. This peculiarity has not yet been explained.

The tubercle is usually contained within its own cell-wall, not in a cyst. It is debated whether it contains nuclei or not. All the changes which go on in the tubercle commence in the centre. Virchow speaks of it as a pitiful product. Carswell says it is unorganizable, but it has been conveyed by inoculation to a healthy animal, and has there germinated. The highest proof of vitality that I can conceive of physiologically is the germinal power. This looks as if Laennec and Baron were right when they ascribe to it a sort of individuality. We find that a tubercle communicates tuberculosis from a diseased to a healthy animal; therefore it is not unorganized, unorganizable; it possesses its own vitality, which must be of high order.

How does a tubercle change from the centre? This fact was a great obstacle to those who believe it unorganizable. The most important change is softening, which begins the process of death in phthisis. A tubercle itself does not seem to be injurious but by softening. If you turn over Aitken's pages you find a remarkable contradiction; he admits on one page that it is often deposited independent of inflammation; read on a little further and he comes to speak of tuberculosis again, and says we have hardly a right to speak of tubercular deposition—it is only a tubercular form of inflammation.

Tubercle is often entirely indolent. On the dissecting-table you will meet with cases where you will find tubercle around which there is no change of condition or of the tissue in which it is deposited, inflammatory or otherwise; perhaps a little thickening or hardening, which may be the result of mere pressure; yet it is often deposited under the excitement of inflammation as a result of the hyperæmia which belongs to the history of tuberculosis.

Lebert says that tuberculosis attacks the internal organs; scrofulosis, the skin and periosteum. We have attempted to show that scrofula does not occur from any one single cause; that a concurrence of circumstances must act upon the subject, and act slowly, to produce this cachexia. I believe that point may be considered as settled. Dr. McConahey, of Ohio, maintains the old belief that the use of the hog as food is the sole cause of scrofula, and that those only who use the hog in various modes as food are subject to it. I have no doubt that this animal, of all others used as food, is the most frequently diseased. All wild animals become tuberculated in confinement; I will not say scrofulous. I examined a lion who died in one of our menageries. I found tubercle in all the organs of the animal's body. Sauvages calls the farcy of the horse a scrofula; monkeys in the zoological gardens die of consumption tuberculated. The horse is a very delicate animal when tampered with by domestication. All domestic animals become delicate except the cat; this animal seems to retain its tenacity for its nine lives whether wild or tame, and is the only animal domesticated without deterioration of its powers of life.

But while we find that scrofula is not hastily manifested primarily on the external surface in those subjects in whom we recognize the affection in the lymphatic glands as separate from tubercle,—while we are clear on this subject, as requiring climate, diet, atmospheric, hygienic, and other influences to produce this cachexia, we know that the separate, modified, or distinguishable cachexia tuberculosis is producible rapidly under morbidly favorable circumstances. If typhosis is tuberculosis—but when there is a deposit I suppose it to be tubercular, for we are told you cannot distinguish it from tubercle—we find in typhus fever, very frequently, sudden tuberculosis. Bell says this is common in Glasgow, and you find it occur occasionally everywhere; both in true typhus, and perhaps more especially in typhoid fever, the patient frequently dies of tuberculosis—of the lung even—when not suspected of having tubercles. How is it produced? By the vitiation, shall we say, of the fluids? of the solids, too, then;

how shall we make a distinction between them? The question has been asked concerning many cachexias and morbid diatheses, where do they arise, in the solids or in the fluids? I once thought it a matter of some consequence to determine this point; I now regard the solids and fluids physiologically as the same—the blood is floating flesh. If we believe the doctrine of “correlation of forces” we must regard heat as a mere dynamic agent, to use a tautology. I do not yield an unqualified assent to this doctrine, yet it has its value and there is a certain amount of truth in it. Take an egg in its natural condition; you say that it is fluid, nothing solid about it but the cell-wall; the yolk itself is composed of cells and is a fluid; but now you expose it to heat—a mode of motion, says Tindall—and what happens? It becomes solid. Is there any difference, then? Can you say of an egg, as the source of all peculiarities as well of health as of disease, that it is the fluids or the solids which are concerned? By subtracting heat you cannot bring the egg back again to a state of fluidity, though you can water, which has been frozen solid, by supplying it. Thus we may argue that the fluids and solids are the same; it is a question of distinction without a difference. We do not know where the evil begins; everything is fluid, and everything is solid; the same atom fixed at one moment is flowing at the next. The blood is not a solution of solids in water, it is an interfusion of solids in water. It is impossible, physiologically or pathologically, to distinguish between fluids and solids.

Disease begins unquestionably, as we see, in the very germ itself. Foetuses are born, dead perhaps, living perhaps, but tuberculated. There are two such instances mentioned in your text-book, of tubercles formed in the lung before birth. The child is often born scrofulous, as we see by the condition of the skin, so that both these forms are transmissible from the earliest periods of existence. The subsequent developments are different and therefore antithetic, and assist in separating them. When we deal with scrofula, everything we see on the surface is of an inflammatory character, modified; on the eye, on the face, little vesicular points containing lymph—I have

watched them scores of times. This dries and a little ulcer forms, which heals for the most part readily; as soon as the ulcer ceases, the redness disappears, and everything is right except a little opaque spot. This will happen sometimes upon the cornea. You find the Meibomian glands exposed to irritation and inflammation; and scrofulous eyelids are very common; sometimes the resulting thickening will last a whole life, the patient being very susceptible to the formation of styas. The various cutaneous affections are all inflammatory; so are the affections of the glands, and the changes in the glands, notwithstanding Prof. Flint, whose book I so often press upon you as worthy your most careful study, says that the condition of the gland is very similar to the condition of the tuberculated swelling. Far from it; there is no tubercular deposit in the gland; it is a change there, corresponding to that going on in the formation of an abscess, and in the centre there will be formed pus; or in some part of it after it thickens and hardens like a common phlegmon, you will find it soften. Pus is formed in that strange way in which blood corpuscles are converted into pus corpuscles; but tubercular deposit does not take place; sometimes it will be laudable pus. Sometimes it breaks down, becomes oily, curdy, etc., but still it is inflammation, and is to be treated as such. You learn to do with it all that is necessary in the case of an abscess; you poultice it, you open it so that it shall not cause an unsightly scar by opening of itself; but in tuberculosis this does not happen.

Every living thing increases and undergoes changes from the centre. It has been a great puzzle to many pathologists to know why tubercles soften, undergo their change, in the centre first, as a general rule; but so it does in the glands, though it is a different change. What is the change taking place in a tubercle? There are several such changes; one is the softening with which you have much to do in phthisis. This is a very important change; it is always attended, as far as we know, with inflammation around it; it excites inflammation near it, breaks and softens. It undergoes other changes, which are contrasted with the softening, in that they tend toward

the healing condition. Obsolescence is one; the tubercle seems to lose its normal form. Virchow insists that tubercle never masses itself; that it is an agglutinated condition; it always consists of gray clusters of little tubers, which do not coalesce in their ordinary condition; they are always tubercles, therefore the appropriateness of the diminutive. Beale says that these changes in the centre are a peculiarity of life itself. When the obsolescence commences in the centre it becomes like old cheese, often as hard as horn, and in this state it is only mechanically an irritant for a little time, and becomes afterward entirely indolent and does no harm. It undergoes another change. An absorption takes place of the animal matters, the albuminous and fatty, etc., leaving the earthy matters, and you have a cretaceous substance left there, which is sometimes spit up, and the patient gets well, with a puckering of the lung. If it is not spit out, it is in an indolent condition, and is nothing more than a mechanical irritant. It is the softening of the tubercle that is a specific and dangerous element. Again, osseous matter is sometimes deposited strangely enough. Under what peculiarity of circumstances the tubercle becomes osseous we do not know; but when it does become so, it is only a mechanical annoyance, and does not do any harm. Now these changes do not occur in the scrofulous affections. They are marked in their difference in this respect, and thus we separate the two diseases to a certain extent. It has been suggested too that scrofulosis is a curable, and tuberculosis an incurable condition; but these are vague and uncertain phrases.

Le Grand maintains without reason, that in scrofula the blood is impoverished, and the inflammatory element is taken away; on the other hand that tubercle implies the retention, at least, of that portion of the blood which is liable to inflammation. Ringer and Dr. Parkes have been making some careful experiments with the thermometer, and they state that in all cases while tubercular deposition is going on, the heat of the body rises above the average rate—rises up to 102° F., perhaps—and this for a length of time. They do not give us any experiments

upon the condition of the body while a scrofulous gland is undergoing inflammation. In many of these cases, the inflammation is enough to produce fever, and the thermometer would show the increase then. Sometimes they go on without affecting the constitution with fever. It is certain that when the tubercle begins to soften, this is the fact; hectic fever is fully developed, and the different parts around the tubercle undergo a decided change. Not so with the scrofulous gland, which does not necessarily inflame the parts about it to any great degree, though there is, of course, some irritation of the parts around. To get rid of scrofula, it is important to understand the nature of the hygienic conditions which favor it, and the hygienic conditions opposed to it.

We do not know entirely the condition of savages, yet I do not believe that they are free altogether from tuberculosis; this can be built up suddenly. I do not know that there is any proof of scrofulous affections attacking the savage or half-barbarous man, living in a wild state, and in a good climate. Our navigators tell us nothing of scrofula affecting the natives in cold countries; and in warm climates, the frequent exposure to the open air prevents the occurrence of scrofula pretty effectually. We suffer less from scrofula in warm climates than in temperate and colder ones; not on account of the temperature, but because of the housing up and avoidance of fresh air to protect from the cold.

THERAPEUTICS.

THERE is no function or office allotted to and performed by man, more elevated and important than that of relieving the suffering and promoting the health of his fellow-men. I except none. The objects and purposes of the religious teacher, the most worthy object of comparison, and those of the patriotic ruler and lawgiver, are truly noble—of the first, indeed, little less than divine; but they are only aims impracticable, visionary, unsuccessful. History shows, and experience daily illustrates, the inefficacy of their laudable efforts and the unavailing feebleness of their influence, even when exerted, as is not always the case, in the right direction. Crime is as rampant and irrepressible as in the days that are past, and all forms of piety are found as superficial as ever, and as ready to yield under temptation and opportunity.

Meanwhile a steady and unquestionable progress of improvement has been and is still going on in regard to the physical well-being of our race; and if civilization does bring in her train certain new modes of infliction and increased susceptibilities, she does not fail to offer, sooner or later, from the resources of her ever-advancing science, consolatory methods of correction, palliation, and compensation.

It is truly gratifying to record the concurrence in these views, of one of the profoundest thinkers of our day and generation. We find Mr. Carlyle expressing his opinion of "the profession of the human healer" as "being radically a sacred one, and connected with the higher priesthood, or rather being itself the outcome and acme of all priesthods, and divinest conquests of intellect here below—as will appear one day."

The ultimate object of all our studies is the prevention and cure of diseases. In the beneficent labors of our modern

hygienic institutions, philosophers, legislators, and philanthropists share largely with us; our exclusive duty lies in the persevering pursuit of therapeutics—the art of healing. It is incumbent on us, from time to time, to engage in an extensive survey of the field which spreads itself before us; to review the results that have been arrived at; to lay down for ourselves farther definite aims, and to inaugurate properly the exertions necessary to their attainment, to choose the correct course of proceeding, and understand clearly as well what is feasible, as what is to be desired. All things are not possible in themselves, however much they may seem within our reach; many things are impossible now which we may hope to grasp at some future time; many things are only possible to those who enjoy special opportunities for their attainment. These self-evident truths should be brought to the notice of the zealous young aspirant, for his benefit and warning; not to damp his ardor but to set limits to the scope of his ambition, that he may not waste his strength and fall into despondency from disappointment. He must not be encouraged to indulge unreasonable expectations. As it is notoriously in meteorology, so also in our biological studies, the elements are numerous and diversified to such a degree, and subject to such infinite variety of combinations, as to defy every attempt to arrive at uniform or precise conclusions. Every movement, doubtless, follows exact rules, but of these our cognizance is partial, and as we have not yet acquired clear or available knowledge, we shall often meet with unexpected and seemingly capricious incidents. But there is no caprice in nature; all is governed by inexorable law; we have to deal with neither miracle nor chaos; and herein lies our hope of progress, our inducement to exertion.

We have been engaged in the consideration of the nature and history of disease, the causation of diseases, and of some of their most striking characteristic phenomena. Let us now reflect, and inquire whether in all that we know upon these points, we can find any satisfactory deduction, any relevant suggestion as to the modes of relief from the sufferings we have been contemplating. We may pretermit here all discussion of the

various theoretical and hypothetical doctrines of the so-called "Schools of Medicine;" their solidism and humoralism, empiricism and scientific methodism, their allopathy and homœopathy. There underlies all these superficial debates a more profound and catholic philosophy, which, if we can make it our own, we shall find a safe guide; but it will not lead us, be sure, into any exclusive views. The faithful physician will avail himself, in his interminable contest with physical evil, of every weapon in the medical armory, indifferent whether it acts by similar or opposite impression; nor will he concern himself even with the question of quantity, farther than to be certain that he is not on the one hand drawn into dangerous excess, nor on the other held back by timidity from the employment of whatever may be requisite for obtaining the effect sought.

When he chooses entirely to abstain from interference, for whatever reason, he will simply wait or "expect," without holding up before him the false mask of an imbecile trust in mythic "potencies," or infinitesimal divisions of dose. Nor will he be deterred from the use of any means fitted to his purpose, by the fact that charlatans or impostors have adopted it, and endeavored at its special appropriation.

Our first and most ready division of the causes from which arise the host of maladies that afflict us, separates the incidental from the specific. This seems to me to indicate, at once, a corresponding distinction in our therapeutics, as in diseases themselves. In the first class, produced by the impression of those agents which sustain our lives—air, heat, food, etc., as their influence is both indispensable and inevitable, we must content ourselves with the effort to regulate it. We direct this effort against their excess or deficiency, or unadaptedness, or abnormal variations; or we bring appropriate measures to bear upon the condition of the subject, his strength or weakness, his susceptibility. Many of the most serviceable advances of modern medicine have been made in this field, and many abuses corrected. It has become less common to apply and misapply drugs of the most active force and the most specific character in the management of the most simply incidental

maladies. We are not so familiar with the promiscuous administration of antimonials and mercurials for every casual disorder of the digestive organs, "bilious," they are styled, and every catarrhal attack. We have been taught some sharp lessons as to our share, active and acquiescent, in this absurd and reckless malpractice. The "medicine expectante" school, and its caricature homœopathy, well described as being "not anything, so much as a nothing which looks like a something," have furnished us with irrefragable proofs that there was here a frequent if not habitual infliction of useless additional annoyance at least, not rarely perhaps of positive injury, to the unfortunate invalid.

Yet we must not submit passively to the censures heaped on us, nor receive without examination the statements brought forward to our discredit. Allowance must be made for almost inevitable exaggeration; we must endeavor to discriminate between the actual and the seeming, the real and the apparent—discrimination often so difficult as to justify fully Cullen's severe and paradoxical dogma, that "we have in medicine more false facts than false theories."

The spontaneous faith in the healing art, which has grown up so widely among all peoples and in all times, civilized, semi-civilized, and barbarous, the universal and instinctive resort to varied methods of medication, often fantastic, often harsh and painful, can scarcely be imagined to be absolutely without foundation in the nature of things. Far be it from me to defend, or even excuse, a promiscuous or indifferent employment of agents, possessed of whatever powers. It is the very essence of our art, the very purpose and object of our science, to distinguish and appreciate the circumstances and occasions for our interposition; to enjoin abstinence from all blind and indeterminate action; to enforce the restraints of prudence, and ensure the guidance of reason and experience. But I will not shrink from saying that reason as well as instinct, and the highest prudence seem to me to justify, or rather to demand an unhesitating interference—even if simply tentative—where our knowledge and experience fail us, in preference to an inactive,

stolid submission. We must examine such contingencies on every side, and surrender ourselves to the dictation of some hopeful analogy, or some plausible, even if conjectural *rationale*. It is plain, indeed, that without this instinctive, nay, we may call it involuntary, effort and cry for help, there never would have originated an art of healing, never existed a divine science of medicine, such as we are hopefully engaged in establishing and building up. As a universal rule, in the obscurest ignorance, the most misty doubt, I would maintain that perturbation is better than inaction; in the former, there is hope, even if we incur unavoidable dangers; in the latter, nothing but blank despair—unpardonable acquiescence in suffering and injury.

With regard to the critical examination and cross-examination, which I have insisted on, previously to the reception of the documents presented us for instruction and reproof, none can deny our right to make it thoroughly searching. Yet it is a very delicate matter to say what is necessary to be said on this point. We must take into the account, as far as they can be ascertained, not only the intellectual, but the moral status of the witness; not only his competency but his bias, his opportunities, his surroundings. Tables of statistics, it is well known, can be produced to prove almost anything; and as no two truths can really conflict, or contradict each other, we shall often be forced to hold our belief suspended between opposite statements, from authorities of equal or unknown weight. Nay, we are sometimes compelled to the disbelief of alleged facts by an irresistible course of reasoning, as we reject and throw aside—in many instances by common consent—the apparently strong evidence of an impossibility, a modern miracle, a ghost, a clairvoyance.

Sometimes we receive the bare assertion, but repel and refuse the inferences linked with it, and demanding to be also accepted. For example: Prof. J. H. Bennett, of Edinburgh, claims to have achieved uniform success in the treatment of simple pneumonia. His personal authority is sufficient, and we will not deny or dispute his asserted claim. But we are

not prepared to yield our assent to the corollaries deduced therefrom, which may be set down thus: that pneumonia is not in itself a dangerous or fatal malady; and that Prof. Bennett, having ascertained this fact, has indicated a forbearing and cautious method of management of patients laboring under it, which is incomparably preferable to all others, if not absolutely infallible. I would by no means impute to Prof. B. the alternative inferences which he has been charged with suggesting, viz.: that pneumonia is a serious disease, and that he alone has found the proper method of controlling it. I say that while we must not reject his statements, we may stop short with the mere acceptance of them, declining to be led on from them into any hasty generalizations, any dogmatic conclusions. In looking over the bills of mortality everywhere published, pneumonia will be found to occupy a conspicuous place on the list of the assigned causes of death. In cold and temperate climates indeed, it holds a position inferior only to that of the great destroyer—phthisis. Let us see. I have before me, unselected, the mortuary statistics of the city of Philadelphia for the years 1863–4–5. In 1863 there are 743 deaths put down to pneumonia; in 1864 the number is 929, and in 1865, 776; making in those three years the large total of 2,448. Only two other named maladies compete with it in amount of destructiveness—consumption, its congener and sometimes its sequel, 6,064; and cholera-infantum (a wide word), 2,455. “Circular No. 6,” a document the preparation of which does great credit to the compiler and to the War Department, informs us that in the first two years of the late war, the deaths assigned in the Army Reports to pneumonia, amounted to 8,090, a mortality of about one in every $4\frac{1}{2}$ cases—appalling enough, but less than that of the British troops in the Crimea, one in every 3.6. In Ramsey’s valuable tables of the mortality of the city of New York for thirteen years, we find 15,138 ascribed to inflammation of the lungs, while consumption claims 37,000; convulsions, marasmus, and cholera-infantum, vague designations, are the only other named maladies that rank above it.

Now, one of two things must be true on a large scale; either

pneumonia is in itself a dangerous and often fatal disease, or the great mortality attributed to it, must be the result of the malpractice of the physicians who report the deaths: *The patients must die, whether directly or indirectly, because of the treatment followed.* It is impossible for me to assent for a moment to the humiliating and offensive conclusion, thus distinctly suggested. I have been so unfortunate as to have lost more than one case of pneumonia in my busy life; and I protect my brethren against such a sweeping declaration of incompetency and error, which I fully believe to be altogether unmerited and misapplied.

This is a strong illustration of the dilemma in which we are often involved. My mind finds rest *in this position*; to which, indeed, I have been so frequently driven that it is now habitual and fixed. I question no man's veracity, as I will not submit my own to question. I take the facts as presented by the reporter, for what, in good faith, they seem to him. But I am looking upon them from a different stand-point, and see them in a different light. In my analysis I judge for myself of his competency to observe, to distinguish, to record. I note critically the manner and spirit of his communication; I weigh cautiously, and make allowance for any detected bias from preconceived opinions; the warpings of his vanity, his interest, his ambition; and after all, I decide according to reason and my best judgment. For, as no amount of evidence can establish or prove what is impossible, so, it is not in me to believe what is incredible.

We have heard, before a great medical association, of hundreds of cases of scarlatina treated by one country practitioner in his special method, without a single death. Our Journals mention the presentation to a distinguished physician not long deceased, of a silver vase, on which was engraved an inscription, purporting that more than three hundred cases of Asiatic cholera—a large proportion of which were negroes, notoriously bad subjects of this dreaded pestilence—had been treated by him without any fatal accident. These histories may be collated with Lefoulon's, Nagle's, and Hastings' account of their success with yellow-fever, as mentioned in La Roche's

great work. It would be idle, if not impertinent, to investigate the details of such reports as these. We would not, if we could, prove a negative here, and lawyers and logicians warn us of the difficulty of the attempt. There is a gross caricature, yet not without some likeness to such statistics, in the advertisement of an irregular practitioner, who, after ascribing prodigious powers to his formulæ, and reciting their varied and marvellous triumphs, wound off with the formal challenge—"If these things be not true, let the contrary be proved!"

Exaggeration of the efficiency of medicines and medical treatment is generally presented in the style of *eulogy* by some enthusiast, self-deluded or deceived by favorable coincidences; for I will not speak of the mischievous falsehoods emanating from ignorant pretenders, with base motives of selfish advantage. I have before me a work from the hands of a distinguished physician of "the modern Athens," recently issued, out of the open page of which I copy the following pithy and dogmatic phrases: "Many a child have I seen die from nothing but the mother's not liking to make her swallow a powder, or put on a blister; many a life is lost from this." He tells us a story of his being sent for to a village where "malignant cholera had broken out with great fury." Summoning the people together, he explained the mode of invasion by preliminary or premonitory stages, and asked them all whether they had any of the symptoms. "They all denied having any; I knew," he says, "they were not telling the truth, and I made every one take some of this medicine—producing a bottle of simple and cheap physic, only one woman going away without taking a dose; she was the only one of all of them who died." I do not know in what language a positive faith in drugs can be more strongly expressed. Yet, we shall find, looking back a little way in the same volume, a pleasant anecdote of a rustic to whom he had given a written prescription. The stupid fellow "*took this*" literally, swallowing the paper, ink and all, and was promptly cured.

We must not forget, however, that there is another and contrasted form of exaggeration, which deals in menace and invec-

tive. The degree to which this is carried, is so absurd that it would be absolutely ludicrous, if it did not concern matters so serious as human life and health. Mercury and the lancet, blindly worshipped so long, are not only dethroned, but angrily expelled by the modern iconoclasts, whose denunciation of these time-honored remedies is loud and violent. One of our English brethren, in his humorous wrath, characterizes the old and dangerous proceedings, so long familiar, in the following terse and forcible phrase: "A. B. caught fever—took calomel, bled, blistered, died." Our friend quoted a few lines above, would perhaps have written in a different spirit: "A. B. caught fever—refused to swallow a powder, or be bled, or blistered—died on the spot."

Everybody knows that thousands, with reason or against reason, have gone through this dreaded routine, in its most stringent severity, unharmed; recovering, whether because of it, or in spite of it—who shall decide? I do not recommend; or even defend, this *methodus medendi*, which is by no means my favorite course; I simply state the fact. Happily for us and for themselves, patients are not so easily "done to death." They survive, in large proportion, the loss of blood and other heroic practice, even including ptyalism. And when they die at last, it is not, certainly in the vast majority of instances, of the remedies, whether well or ill chosen, but of the disease.

Let me not for a single moment be suspected of indifferentism in this matter. Short of absolutely putting an end to life, injudicious practice may be extremely injurious. There is a vast difference between the right and the wrong method. How much harm may be done by an unskilful, unwise physician, it is impossible to compute; but it results not so much from his selection of medicaments, as his incompetency to employ them properly. We say truly, *omnia sana sanis*; we may say with equal truth: "Everything is safe in the hands of the prudent man."

Some have maintained the impropriety, as a general rule, of interfering with the class of simply incidental diseases, now under consideration, because they will, in all probability, subside and disappear with the removal of their usually transient

causes. I would not go so far as this. It is our duty, as it is almost always in our power, to diminish the violence even of these maladies, to shorten their duration, and to obviate such tendencies to more permanent evil, as they may essentially or occasionally be found to develop. Yet, if it be the observed course of any disorder, that it is likely to pass away after a known period, without inflicting any ultimate harm, we may watch its progress quietly, interposing only such palliatives as may be called for to relieve some symptom or abate some annoyance. And it should be noted that this passive method may be followed with propriety, if with careful discrimination, in certain cases of the gravest forms of pestilence, in which the cause, whatever it may be, has acted with so little force, or has met with such constitutional insusceptibility, or resistance, as to have made but slight impression. It is a wise and true saying of Schöenlein, that "it often happens to good physicians to find no indication for treatment—to bad ones, never."

On the other hand, the most evanescent of incidental causes, such as degrees or alternations of temperature, excess in eating or drinking, or venery, or muscular exercise; all or any of these contingencies may, from vehemence of impulse, or casually enhanced susceptibility, or temporary defect of reactive energy, impress deeply and dangerously the system assailed, and require for its relief or protection, the promptest, most vigorous measures.

Our ancient and established therapeutic has been, *ex necessitate*, to aim at the diminution of the particular force of morbid impression, upon whatever part directed, by subtracting from the general force of the functional movements of the entire organism. This was effected by abstinence, rest, cold, local and general bloodletting, cathartics, sudorifics, nauseants, and diaphoretics. Revulsive measures came to be added very reasonably, however obscure may have been the first suggestion; and emetics, blisters, issues, moxas, were resorted to. And what better means than these have we now within our reach? Many instances present themselves familiarly indeed, in which no others are available; and our only course is this

retardation, by such depressing and derivative agents, of the rapidity of the changes going on under morbid excitement of whatever varied character.

In this proceeding we follow the promptings of nature. Anorexia is one of the earliest and most uniform symptoms of acute maladies in general—we abstract food; there is thirst, with heat—we dilute the fluids and cool the body by ice and iced drinks; languor oppresses the sick—we enjoin repose of mind and body; light and sound annoy and irritate—we darken the chamber and order silence and stillness. Hæmorrhage suddenly bursting forth, has been productive of instant relief from previous symptoms; diarrhoea, emesis, or a profuse sweat breaking out, has been likewise beneficial, and thus we learned venesection, and built up gradually a natural system of therapeutics, which no hypothesis or theory, no system or school, scientific or empirical, will ever overthrow.

We have been meanwhile seeking earnestly to obtain the power of substituting for these means of universal impression—often superfluous and at least in part irrelevant—agents of more direct, more relevantly local and circumscribed influence, manifesting their force and efficacy only where and when they were wanted and applicable. It is greatly to be regretted, that hitherto we have not been in any gratifying extent successful. For the relief of a local hyperæmia, we are still often compelled to subtract from the whole amount of the blood in circulation; for a local pain or spasm we must reduce the entire organism into a condition of anæsthesia or relaxation; disturbing at more or less risk functions and organs of parts which might otherwise have escaped intact. Yet we are not without hopeful results of experiment in this direction, and find much to encourage us in the atomizing application of remedies, the bold injections and insertion of instruments into the respiratory tube, begun by Green, and in the increasing resort to hypodermic medication and the local anæsthetics, such as rhigolene, both in surgery and in medical practice, strictly so called.

I take the occasion to remark here, that the active general remedies, above spoken of, require to be employed at the

present day, with more reserve than in times past. I am one of those who believe and maintain that great and decided changes have taken place during the last half century in the general type of diseases, both incidental and special, or in the condition of the human constitution, or, most probably, in both. The sthenic, formerly paramount, has progressively yielded to the asthenic type. Not that either ever prevailed to the exclusion of the other; our predecessors recognized both, but wrote and acted on the assumed general predominance of the inflammatory over the adynamic, a state of things which we find in a certain sense and degree modified, if not reversed. If my observation and experience, enduring through the above specified period, have clearly satisfied me of any one point in the natural history of diseases, it is this.

We are often tauntingly called upon by those doubters of the wisdom and common-sense of our ancestors, who differ from us and deny that any such change has taken place, to fix the date of its occurrence. It began to be manifested certainly more than forty years ago. The time of invasion of typhoid fever has been ascertained, when, in this country at least, it forced itself upon the attention of physicians, and substituted progressively, and rather gradually than rapidly, the adynamic for the inflammatory character of our pyretic affections, mingling with, rather than abruptly displacing, where they had been prominent or exclusive, our malarial fevers.

As a student of medicine at that period, I will avow that I was not unobservant, as my earliest writings will show, of the necessity which had come upon us, of modifying the energy of the antiphlogistic or depletory practice then in vogue. My most intimate professional friend, educated under Rush, rarely omitted the use of the lancet in the treatment of the phlegmasiæ or other febrile disorders. Comparing the result of my more forbearing course with his, I became fully persuaded that even if I could not boast of fewer deaths among my patients, they suffered somewhat less in constitution and recovered more readily.

All who are old enough to recall the history of that bygone

time, will remember that constipation was far more uniformly an annoying element in fever than it is now. Cathartics were invariably administered among the first remedies, and it was often difficult to obtain their proper effect. At present, and indeed for a full generation looking back, a very different condition prevails. To the question when this change manifested itself, two replies have been given. Many of my medical associates are unwilling to go beyond the invasion of cholera, which, coming among us in 1832, left, they affirm, an impression which has been permanent upon the general character of our diseases in this respect; exerting an obscure and indefinable influence, either upon the constitutions of our people, or by mingling and combining with the causes of our other maladies, indigenous or endemic. I am inclined to agree with them in the positive clause of the opinion thus expressed, and admit the influence of Asiatic cholera; but I believe that the change was apparent long before, being coincident with the introduction and spread of the typhoid, enteric, or abdominal form of fever.

My preceptor, honored be his memory! a zealous pupil of Wistar and Physick, and Rush, a most sagacious and successful practitioner, who, through a long series of years, enjoyed the highest reputation, and carried on the largest and most lucrative professional employment that ever fell to the lot of any private physician in this, or perhaps any other country, made familiar and free use of the lancet, as all doctors then did. But his therapeutical specialty lay in his dependence upon purgatives. All cases that fell under his care—with exceptions so few as not to be worth noting—were indulged with some cathartic formula at the very beginning, and great numbers were carried through a febrile or inflammatory attack into convalescence, upon the same method, modified *pro re nata*. We, the inmates of his office, serving each in his turn, passed whole days and evenings, not seldom far into the night, in the preparation of prescriptions, nine in every ten of which were cathartic formulæ. It may be well to say here, that at the period I write of, and in that section of our country, it was the custom to make up the prescriptions of physicians at their

own offices, and that it formed a very valuable portion of medical education, the student thus becoming familiar with drugs and formulæ.

I have said that the gentleman spoken of was successful in every sense of the term; eminently so, as must be inferred from the lofty and long-enduring estimation which he enjoyed, both among his brethren and his wide circle of clients. Yet, had he lived but a very few years more, he would have been forced, nay, he would have of himself promptly recognized the necessity, to make a great change in his accustomed methods of treatment. I had not, I well remember, more than barely entered upon the responsibilities of practice, before I became aware that the class of remedies which I had been so assiduously engaged in preparing and administering, were not demanded so generally, were not to be used so freely, were not unfrequently ill-borne and injurious, and that it was incumbent on me to resort to them with a certain degree of reticency and reserve. Yet they have held, and will always continue to retain, a prominent place in the early management of fevers generally, as well as of many other acute affections.

A similar transition of views was going on coincidently as regards venesection, and even more rapidly. It was the more marked, because under the teachings of the illustrious Professor of Practice in the University of Pennsylvania, then the centre of medical instruction in our country, lavish and almost promiscuous bloodletting had become the "order of the day." The very equivocal, but indispensable argument, cutting with double edge, and convenient alike to sustain truth and error—"post hoc, propter hoc"—so fearlessly wielded by the ignorant pretender, so casuistically repelled by the indiscriminating sceptic, soon came to be absurdly applied here, as indeed it is still; and as all recoveries after venesection were formerly recorded as triumphs of the lancet, so now, all deaths preceded by it were set down to its charge. I have stated my unequivocal opinion, that a change had taken place, both in the prevailing types of disease generally, and in the constitutions of the subjects—both of them having become comparatively

asthenic or adynamic; and of course the loss of blood, like other methods of depletion, was less required, and not borne so well. Hence, venesection fell for a time into almost absolute disuse, and physicians soon ceased to carry lancets about them; nay, I know several who have attained middle age without ever having handled such an instrument.

Farther, I will remark that I look upon this specified constitution of diseases and subjects as having culminated some years back, and to be passing, or to have passed away in some measure. I do not know—I scarcely suppose—that cathartics are more demanded; but I am satisfied that depletory measures of every other character, venesection especially among them, are not only coming again more into fashion, but are really more frequently called for and better adapted to the general requirements of ordinary practice. We have ceased to be burdened with the recently prevailing timidity as to the lancet. Even if we are mistaken in our belief of the revolution of an adynamic cycle, and the advent of a new phase less asthenic, surely the experience of our late cruel war has proved that, under the most depressing contingencies of imperfect nutrition, shelter, ventilation, clothing, the loss of blood is far less impressive for evil than has been supposed; and we will hereafter bleed a patient in a doubtful case with vastly less misgiving, and offer him much more readily the chance of a reasonable experiment, the *anceps remedium*, which may require courage, but which, as I have maintained, we are bound to prefer to abstinence or inaction.

During the period of which I have been speaking, professional orthodoxy, ever ready to swing from one extreme to the opposite, repudiating the lancet of Rush, the leeches and gum-water of Broussais, and the purgatives of Hamilton and Piedagnel, progressively attained the comfortable confidence in sustaining and stimulant medication shadowed forth by Brown, and carried to its height by Todd. The reign of this system has been short, however, and the vibrating pendulum seems to have settled down for the moment at what we call, after Chambers, "the restorative point," a little above the

medicine expectante. It is well represented by Chambers and Bennett: A little very careful depletion, sometimes by a mild laxative, it may be an early emetic; a wet cup occasionally, or two or three leeches; a revulsive plaster, and in a severe attack a blister perhaps; an emollient poultice certainly; cod-liver oil, quinia, and iodide or chlorate or bromide of potassium. This is all very well, and entirely unobjectionable; presents no appreciable risk to the sick man, and is very safe for the attending physician, who by this course avoids all chance of being accused of the death of his patient, if he does not recover. Following some such method in our habitual routine of business, as the great majority will, and prudently too, let the young physician be warned not to fall into any relaxation meanwhile of due attention to the varying conditions of the cases under his care. Some one among them may suddenly grow worse; aggravations of symptoms may happen, which will not bear neglect, which it will ruin him not to have observed or appreciated duly, and which may demand prompt and energetic action.

Medicine, it should never be forgotten, is both a science and an art, and it is difficult, if not impossible, to offer to its votaries, eager to learn and to know, a satisfactory detail of the principles, rules, and reasons by which they are to be guided in practising it. Arnould says truly, that "the whole of every science may be made the subject of teaching; not so with art, much of which is not teachable." One of our most distinguished brethren has described physic as "one of those departments of knowledge in which there is frequent necessity for the exercise of an incommunicable faculty of judgment, and a sagacity which may be called transcendental, as extending beyond the combination of all that can be taught by precept." It is so, indeed, with all arts which, like ours, are based upon what Whewell calls "the mixed and applied sciences." The painter may thoroughly inform his pupil in the rules of perspective and the harmony of colors; but all this will not enable him to produce a picture. Of two men, one may be master of the principles of gunnery, the other

comparatively ignorant of them; and yet the latter shall make the best shot. Harvey was emphatically the scientific physician of his day; his contemporary, the "rebel Captain" Sydenham, the great practitioner; and a late antithesis of the same kind, has been suggested as between Sir Charles Bell and Abercrombie, the modern Sydenham. Coleridge, referring to medicine as a "tentative art," observes that "there is always a great deal that is mysterious in whatever is practical." The Hindoo smith extracts from his ores by his scanty, coarse, and blind methods of working, an iron superior in itself and fit for making a better steel than the most skilful metallurgists of scientific Europe can produce.

The class of *specific diseases*, our endemics, epidemics, our pestilential and malignant maladies, arise, or are assumed to arise, each of them under the influence of some definite poison; some morbid agent of characteristic properties. Such are plague and the extensive group of typhic and pernicious fevers, the exanthemata, Asiatic cholera, diphtheria, and our familiar intermittent and remittent. The list may with propriety be largely extended. Careful investigation has led us to a more or less probable conclusion as to the nature of some of these poisons; we have not attained demonstrative certainty concerning any one. It is alleged concerning a few of them, that they are parasitic; vegetable, as Salisbury maintains ague and measles to be; animalcular—with or without Vogel's semi-individuality—psora, trichiniasis, and Virchow's cells; others neither cellular nor parasitic, as the aërial contagions of scarlatina and pertussis. I am aware that such knowledge as we possess on these points has thus far been barren; "luciferous" merely in the phrase of Lord Bacon, "not fructiferous." I know also, that our profession, while admitting the specific character of the poisons which generate so many diseases, have been reluctant to make that admission the basis of any extensive therapeutical aims or purposes. Nevertheless, it is in this direction that I am disposed to look with the most hopeful anticipations of the future usefulness and glory of the divine art of healing.

I am not one of those who refuse to acknowledge specific, or, in the strictest sense, antidotal powers, as belonging to the articles of our materia medica, and who represent "rational medicine" as regarding only the physiological properties of the tissues and organs, and their varying conditions as to excitement and irritation, anæmia and hyperæmia, tension, spasm, relaxation, etc. I recognize one at least of our drugs, cinchona, with entire confidence as a specific in the truly antidotal sense. Since there is one such article in our hands, we may reasonably hope to discover others. Indeed, I think that we might class here one or two in addition, but they are all disputed, and I am not inclined to enter into the controversy. Salisbury denies the quality of antidotal specificity, even to quinia, our most prominent and strongest example; while he admits its direct efficacy in controlling the morbid influence of the malarial poison. "Quinia is not, strictly speaking," he argues, "a curative or specific agent, but simply acts beneficially by controlling cryptogamic development, and imparting such tonicities to the organism as enables it to resist the paroxysms until aided nature can cure the disease by eliminating the cause." And again: "It braces up the system, and controls cryptogamic growth, till nature can effect a cure by eliminating the material cause through the skin, the mucous surfaces, and kidneys." I am very reluctant to yield up the claim of this, our most admirable medicine, to be considered positively, strictly, and emphatically antidotal. In fact, the point seems to me to be virtually conceded by Prof. Salisbury himself. He more than once employs the phrase, "controls the growth of the cryptogam"—"controls the cryptogamic development." How can it effect this? If directly by a property peculiar to itself and independent of its asserted tonic qualities, which seem unadapted to the purpose, surely this is being specifically an antidote. "It braces up the system," he tells us, "and imparts such tonicities to the organism, as to enable it to resist the paroxysms," but we are not told how this is to affect the palpable and present cryptogam. And I object to the vagueness of the language—a charge very rarely to be brought with justice

against Prof. Salisbury, as expressive of indefinite conceptions: "bracing up the system; imparting tonicity to the organism;" I confess that I have not a clear idea of what these phrases mean. If quinia be a direct tonic, which I seriously doubt, it is inferior in that property to others which are not imagined to be appropriate here. Its unequalled promptness of efficiency, its striking action of apparent arrest in the worst "congestive" cases, deserve to be particularly dwelt on. Its virtues are equally displayed when administered hypodermically as when received into the stomach. No other drug is employed thus, and in such small quantities, to relieve debility or prostration; nor is any other tonic, mineral or vegetable, gifted with its most beneficent prophylactic influence.

Observe too, that its *modus operandi*, assumed here to be "bracing and tonic," is much disputed and as yet unsettled. We may ask, indeed, what do we know of the *modus operandi* of any one of our drugs? There are as many who pronounce it sedative, as there are who regard it in the light of a stimulant or tonic. "Calmant" is the term applied to it by Drake. Experiments made to determine its properties are instructive chiefly in a negative sense. A single grain, two grains, three or five, will suffice to arrest the course of an intermittent, which would resist or evade the energies of all our other most potent medicines. Yet, the same quantity, or four times as much, will stir up no disturbance in a healthy system; will not derange the pulse, nor the breathing, nor the temperature, nor disorder the mind in any way.

It is but the other day that accident afforded me an opportunity of repeating this observation. A young girl, æt. 13, being constipated, but in her usual health, took from the hand of her mother a heaped-up teaspoonful of sulph. quiniæ instead of Husband's magnesia. The mistake being at once discovered, I was applied to. Having quieted their alarm, I placed the child in a recumbent posture, and sat by her, watching her attentively for more than four hours. At short intervals, I examined her breathing, pulse, and temperature, making

exact notes of her condition in every perceptible respect. There occurred nothing whatever, absolutely nothing to report, beyond the well-known subjective phenomenon of buzzing in the ears.

We hear sceptics frequently demur as to the alleged remedial influence of some article in some specified case of disease, that it can be administered in the same amount to a person in health without appreciable effect. This is true of many medicaments besides quinia, as of bismuth, zinc, and of arsenic in moderate dose; but the practical physician finds here no ground for objection or doubt of their value as relevantly curative.

Prof. Salisbury, not content with impugning the antidotal virtue of quinia, goes to the paradoxical extreme of asserting, in plain terms, that "in many instances it really aggravates the paroxysms." Now, if the paroxysms depend, as he has taught us, upon cryptogamic development, and are favored by debility, it is difficult to comprehend how they can be aggravated by an agent which controls "cryptogamic development," and at the same time "imparts tonicity to the organism."

Latham, on the contrary, dwells with a rational enthusiasm upon the unparalleled specificity of cinchona. "With it alone we *cure*," he exclaims, "fevers of malarious type; we *treat* all other diseases." We ask no further question concerning such cases than as to their nature, history, and probable cause; and then without delay, without reference to surrounding conditions, abandoning of late even the once familiar ceremony of "preparing the patient for the bark," we administer our drug. We expect it to resolve congestion, to subdue inflammation, to tranquillize irritation; and it does all this and more. Not infallibly, however; no infallible remedy exists—no infallible course of treatment. Men die every day in great numbers of curable, as well as of incurable maladies; of pneumonia and diarrhoea, as well as of cancer and tetanus. We can as little explain its occasional failure, however, as its almost uniform success.

I must not omit to notice the striking views on the subject

opened to us of late, by the discovery reported to the Royal Institution of Great Britain, of a substance closely resembling quinia in the textures of animals, hence called "quinoidine," by its discoverers, Drs. Bence Jones and Dupre. They dwell on its analogy to the alkaloid in its chemical properties, and in its remarkable fluorescence under electrical light—the emission of light, as by fluorspar when treated. It is strange that they altogether ignore Headland's previous assertion of the existence of such an element in the blood. "Among the natural constituents of the blood," he says, "there is a substance which chemically resembles a true alkaloid like quinia; and this similarity admits of direct application." "Quinia and other vegetable principles," he goes on, "resemble a certain principle in the bile; they tend to cure certain diseases which depend on deranged hepatic functions." He even anticipates their therapeutical inferences, and asks if these agents may not supply in the system the deficiency of the bile products, and thus prevent the action of the ague-poison." Their course of deductive reasoning is as follows: "Assume that a substance like quinia exists in health in the textures; can its rapid destruction and removal, through the action of the marsh miasm, give rise to ague? Does quinia cure ague by furnishing a substance which retards the changes going on in the textures? The incoming quinia probably causes—thus, and by arresting the oxidation of the natural fluorescent substance—an actual excess in the textures"—whence the quinism, deafness, etc., etc. They go on to offer some pregnant suggestions in marked conformity with views which I have long been engaged in teaching. "If the circulation can carry alkaloids, as experiment proves with quinia, everywhere through the textures, even pervading the non-vascular tissues, such as the crystalline lens, is it not reasonable to suppose that medicines pass through the blood and act on the textures? And may we not expect that among the multitude of new substances which synthetic chemistry is now forming constantly, some medicines may be discovered which may not only have power to control the excessive changes of the tex-

tures in fevers and inflammations, but may be able to remove the products of insufficient chemical action, even in those diseases which affect the non-vascular textures, as, for example, in cataract and in gout?" Let us indulge these hopes, which, if ever carried out into fruition, will present us with an available armory of antidotal specifics. Why should we not succeed in finding antidotes for the poisons causative of disease, miasmatic, ochletic, and contagious, as well of the poisons familiar to us in the *materia medica* and in toxicology? In the tolerance of energetic drugs under particular contingencies, do we not trace by analogy the antidotal antagonism, whether static or dynamic?

A man bitten by a rattlesnake will drink without intoxication an incredible quantity of alcohol, as also in some fevers of low type. Tetanus enables a patient to bear, alas! how often uselessly, an immense amount of opium, with not even an approach to narcotism. It can scarcely be doubted that much knowledge on this subject has been attained and lost, or allowed to be laid aside unimproved. Dr. Ellenberger, Aide Naturaliste of the Museum of Natural History at Prague, directed his researches to this department of inquiry, and had made remarkable progress. "He was in the habit," says Dr. Miniere, who visited him in company with the distinguished toxicologist and Dean of the Faculty of Paris, Orfila, "he was in the habit of swallowing in the presence of professional men, large quantities of the most virulent poisons, after having taken some counter-poison previously. We saw him take nearly twenty-five grains of the acetate of morphine. After the interval of more than a minute," which Dr. Miniere assures us, seemed very long to him, "Dr. Ellenberger deliberately took from his pocket a paper containing a whitish powder, in amount nearly equal to the morphia, offered it to Orfila to taste (who found it very sweet), and then swallowed it, continuing to walk about the museum and converse with them." Among other deadly poisons, he has been known to take with impunity, we learn on the same authority, from three to three and a half grains of pure strychnine. Every friend of humanity must

regret the untimely end of this heroic experimenter, who was one day found dead, doubtless of the effect of some poison, whose antidote had failed of effect. Still more must we regret that he has left behind him no record of the researches made with such heroic zeal, such dauntless courage.

We know, however, at present so little of the intimate nature of the several specific poisons which produce our diseases, that we are not prepared for any other than merely tentative or empirical efforts at the employment of counter-poisons. There is one, however, which we confide in. Armed with cinchona and its salts, we venture as fearlessly to expose ourselves to malaria as did Dr. Ellenberger to his strychnine and morphine. Clothed with the panoply of vaccination, we bid defiance to small-pox. Did the ancient *Psylli*, do the modern snake-charmers of India, possess any prophylactic against the venom of the cobra and the viper? The old notion of the existence of any general counter-agents of this protective character has been abandoned altogether, and bezoars and mithridates, once objects of such blind faith, are now no longer sought for. But the search for special safeguards and remedies continues and must go on. Scientific pathologists are disposed to regard many of the causes of disease—miasmatic and ochletic, and all the contagions—as possessing an individual life, cellular, animalcular, or fungous. Why may not this life be extinguished? We know many parasiticide agents. The algæ and fungi of the microscopists must be exterminated, the cryptogamous sources to which Mitchell and Cowdell and Salisbury ascribe ague and cholera. We must find means to destroy animalcula, whence we have trichiniasis and psora, and according to the suggestions of the sagacious Holland and Pacini and Thompson, the fearful Asiatic pestilence. Are we not effecting something in this way by the application of our disinfectants, chlorine, the mineral acids, high temperature, and ozone?

But as the evil agencies which originate our endemics and epidemics may not be parasitic, or endowed with vitality individual, or to use a word of Vogel's, "semi-individual," as

many of them would not seem to be impressive in their appreciable characters, chemical or mechanical, affecting us dynamically, not statically, we must search also most assiduously for dynamical counter-agents. I would illustrate my meaning by a reference to the employment of belladonna to relieve the narcotism produced by opium. Corrigan, dwelling on the fact that one of them causes the pupil to contract and the other dilates it, conceived the idea that their general influences upon the organism, and especially the nervous system, might be similarly in contrast and opposition. It is now widely believed that we may place a reasonable confidence in either of them, as against the other. For like reasons, it has been inferred that chloroform is antidotal to strichnia. Most of the non-parasitic poisons are obviously and profoundly sedative; and the analogy which has led us, from the usefulness of alcohol in snake-bites, to the large employment of stimulants tentatively, has not been altogether unfruitful. But even Todd was not satisfied with the results, and was led to seek for still more precise guidance in the conditions of opposite electric polarity.

"Confidence is a plant of slow growth" proverbially, and the evidence in favor of any alleged antidote must be cautiously admitted and most critically scrutinized. Yet, from whatever source such evidence is offered, let it not be peremptorily rejected without inquiry. Scherzer informs us that not only vulgar belief in Brazil ascribes to the vegetable drug *assacu*, a healing power over the hitherto incurable and loathsome Elephantiasis Græcorum, but that a committee of French medical *savans* had taken the matter into consideration, and had made a highly favorable report to the Emperor. Our profession should take warning from the record of its obstinate incredulity as to Harvey's and Jenner's discoveries, and its opposition to inoculation and the administration of "the Jesuit's bark." One after another, so many of our hopes have been disappointed, so many vaunted specifics have been exploded, that we have naturally fallen into apathy and distrust, from which it is not easy to arouse us, even sufficiently to procure

the hearing to which each new promise of benefit to suffering humanity is fairly entitled.

In reasoning of the *modus operandi* of medicines, we must recognize the wonderful and unintelligible affinity which attracts the several articles to the organs and tissues which are fitted to receive their impression, and on which they are adapted to act: some of them by mere presence or catalysis; others by entering into and modifying the composition of the fluids and textures; some by direct and others by reflex or diastaltic influence. We are also aware of the fact, by empirical observation, as we know all that is known on these obscure points, that no two articles act upon the same organ or tissue in precisely the same manner. It is not, for instance, a matter of indifference what emetic, cathartic, or diuretic we shall employ in any serious case requiring one of these drugs. From this trite axiom we may, however, I think, logically draw an important inference. Homœopathy, whatever there is of truth or practical value in it, derives it all from denounced and dreaded allopathy. It is not the *similarity* which exists between the poison that has caused vomiting or diarrhœa, and that which Hahnemann administers to cure it, but that deeper, nicer, and more essential *difference* in their mode of action which makes it beneficial. A careless reader might be surprised to read in a letter from Sydenham to his friend Gould, directions for procuring and encouraging a mercurial ptyalism in confluent small-pox. I do not defend the practice, nor would I repeat the advice; but I find the great physician here, whether right or wrong, consistent with himself and following the best light of medical science in his day, in his effort to substitute a "laudable salivation," as he calls it, for a morbid and variolous one.

This twilight state of our knowledge, and the uncertainties which it involves, call upon us for untiring reserve and prudence, which it is not always easy to keep from degenerating, on the one hand, into timidity and scepticism; or hardening, on the other, into a mere indifferent routinism. An assiduous study of the varying indications presented from time to time

—a watchful attention to the progressive course of the malady and condition of the patient—the “prognosis” so emphatically dwelt on by Hippocrates—so as to be early aware of the “tendencies to death” and on the alert to obviate them, are our urgent duties.

Above and beyond all, it must never be for a moment forgotten that we have under our care, in a state of absolute submission and dependence, a sensitive and suffering fellow-creature. The anguish both of mind and body connected with the great mass of diseases, is apt, I fear, to be most inadequately appreciated, not from want of humanity or sympathy on the part of the physician, but from a habit of mechanical abstraction too ready to grow upon us, or from absorption in the consideration of the case professionally, or from an overwhelming sense of personal responsibility. In certain instances, the indications of suffering are too obvious to be overlooked or neglected; agonies too intense to be endured, forming an unexpressed element of the attack—sometimes significant of urgent danger, at others utterly disproportioned to the actual or impending risk. A distinct and active recognition of the patient's claim upon us for the promptest and largest relief from pain, consistently with his ultimate well-being, is an indispensable portion of every therapeutic. Any indifference to this matter is unpardonable.

In the present state of our science, a sceptic may be permitted to doubt whether we have the power to arrest a forming malady, or shorten its duration, or determine its final result; but no one denies or can doubt our ability greatly to modify the attendant suffering, avoidable and unavoidable. This beneficent potency of our divine art has been augmented very happily of late, and our resources in this kind are every day increasing. Our list of anæsthetics has been enlarged, and new modes of applying them brought into very satisfactory use. The employment of ether and chloroform, the two most familiar and powerful of our anæsthetics, has been extended into the treatment of many additional maladies by successful and hardy experiments. The hypodermic exhibition of our nar-

cotics has enabled us to circumscribe, in some degree, their too promiscuous influences, as well as to evade some of their unpleasant effects. The processes of atomization for internal as well as external application of our concentrated remedies, have made them vastly more available. We introduce them into the open orifices by inhalation or intrusion, with strikingly good results in the immediate diminution of pain, which in its external seats we at once extinguish by the ether spray of Richardson or the rhigolene of Bigelow. We may indeed exult when we succeed, as we not seldom do, in thus putting an end to, or suspending for a blessed interval, the tortures of neuralgia, and the burning, lancinating agonies of cancer.

It is a melancholy reflection that forces itself upon us, whenever we touch on this gloomy subject of pain, that even in the ordinary circumstances of humanity everywhere, life is found to be rather a condition of endurance than enjoyment to the mass, and that in all ages, and among all peoples, the means of procuring sleep and insensibility, or stupefaction, have been eagerly sought for, highly prized, and deeply indulged in. They are, of course, poisons in the true sense, and may be made available in our therapeutics. Tobacco is by far the most extensively used, and must, indeed, possess some property which adapts it peculiarly to the instinctive requirements of our race. It differs from all other narcotics and stimulants, in this, that so far as we know, it is not convertible, like every other among them, into food and nutriment. In concentration its effect is terribly and quickly fatal; but as usually prepared and employed, does so little obvious harm, that all denunciation of it is futile. We are told that two tribes of men can swallow it in substance with impunity. This seems to require confirmation, but the authorities are highly respectable. Wilkes saw it thus consumed by the Drummond Islanders, and Col. Shakespeare affirms that the Indians of the Nylgherries eat it. Not to dwell on the several varieties of alcohol, which perhaps commands the worship of an equal number of votaries with tobacco, we find opium affording to many millions the solace of temporary tranquillity and freedom from care and suffering.

With it the Turk attains his paradise of "Kief," a dreamy state of quietude and torpor; the coarser Chinese plunges into the annihilation of a comatose stupidity; and the reflective Hindoo seeks the cataleptic ecstasy of trance. Tea and coffee, invaluable discoveries of the Arab and Asiatic, are happily spreading over the world their cordial blessings, innocent delights of the domestic circle. Wine, perhaps the earliest known, is now among the most widely diffused, as it is among the least injurious of anæsthetics; I exult in the hope that our country will, ere long, become a land of vineyards, and that the well-concocted and delicious juices of the grape will supersede ultimately all the rest of their rival beverages—the muddling fermentations of hops and malt liquors, and the hot and pernicious products of the devilish still.

Kawa or Kava, the piper methysticum, as yet a stranger to the higher civilizations, is a necessary of life in Polynesia. Its pulp, prepared in Puynipet, by crushing between two stones, elsewhere in most disgusting fashion, by chewing and ejection with the saliva, is said to produce the least offensive of all the modes of narcotism or intoxication, and if the reports be accurate, well deserves to be introduced into our materia medica. A complete muscular relaxation, with drowsiness and soft repose, follow its administration, while consciousness is never entirely lost. The cannabis indica, bang, or haschisch, is much written and talked of. A gentleman who resided long in Egypt, told me that it was not uncommon for individuals to bring it with them into convivial parties, and take it instead of wine, or punch, or other strong drinks, each being permitted to select his own method of getting tipsy; and that he observed that the haschisch-takers were uniformly in better condition the next day than the rest of the company. The amanita muscaria, which retains its intoxicating power in passing through the body, and thus may serve in succession to stupefy the master of a household and all his retainers; the areeca, the palm, and the aloe, are made subsidiary to this instinctive desire of forgetfulness for millions of Tartars, Mongols, Africans, and Americans. Millions more chew the Betel-nut, of

which no European has yet given us his personal experience. Scherzer quotes from a Sanscrit poem, the following eulogy of it: Betel is pungent, bitter, aromatic, sweet, alkaline, astringent; a dispeller of phlegm, carminative, a sweetener of the breath, an ornament of the mouth, a remover of impurities, a kindler of the flame of love. O friend! these thirteen properties of betel are hard to be met with, even in Heaven." If it possesses one-half of them, we are much to blame for our neglect of it in Western practice.

The Caffres and Hottentots exhilarate and stultify themselves with a narcotic plant, dagga, and often refuse to hire themselves as laborers on farms unless a portion of land be set apart for its cultivation. It must resemble somewhat the whiskey-root or peioke of our vast south-western plains, of which we find this brief notice in Burton's *City of the Saints*. "It is a kind of cactus, which grows in southern Texas on the sand-hills of the Rio Grande. The Indians eat it for its exhilarating effect on the system, producing the same excitement as alcoholic drinks. It is sliced as you would a cucumber, the small pieces chewed and swallowed; and in about the same time as comfortably tight cock-tails would 'stir the divinity within you,' this indicates its effect, only giving a somewhat wilder scope to the imagination and actions."

Palgrave, in his travels in Eastern Arabia, met with "a narcotic plant gifted with curious properties. Its seeds, pounded and taken in a small dose, produce effects much like those of Sir Humphrey Davy's laughing-gas. The patient dances, sings, and performs a thousand extravagancies, till after an hour of great excitement, he falls asleep, and on waking, has lost all memory of what he did or said while under the influence of the drug. To put a pinch of this powder in the coffee of some unsuspecting individual is a not uncommon joke, nor did I hear that it was ever followed by serious consequences. I tried it on two persons and witnessed its operation, laughable enough, but very harmless."

The Peruvian delights in his coca. This most admirable narcotic soothes but does not intoxicate, in this respect resem-

bling tobacco, though differing in all others. It is either a food, as Anstie and Smith contend of so many stimulants, or it serves wonderfully as a substitute for food. Campbell, who tells us this, sent an Indian two hundred and forty-nine miles, in April, 1859, from La Paz to Tacna, which he walked in four days; resting but one day, he returned, crossing a mountain pass 13,000 feet high. Through this enormous pedestrian journey, he took no other sustenance than a little roasted maize and his coca. An Aymara Indian travelled with Mr. C. ninety miles in one day, keeping up easily with his mule; eating a few grains of roasted maize, and incessantly chewing coca leaves with chalk. At the end of the journey, he *stood on his head* a few minutes, "a practice of the Aymaras under the circumstances," drank a glass of brandy, and started homeward again. Scherzer says that this "standing on the head after long and severe marches, a custom not less universal than extraordinary, is resorted to, to mitigate the severe pressure of the blood." How does it effect this purpose?

Tschudy and Markham speak in highest eulogy of coca. The latter tells us that "an Indian consumes two or three ounces daily, carrying it in a bag with the ashes of the quinoa plant and lime. It enables fatigue to be borne well and with less nourishment, and makes the breathing easier in ascending mountains. Tea made from coca keeps people awake; applied externally, cures headache and moderates rheumatic pains; of all narcotics, is the least injurious and the most soothing and invigorating." Its active principle, cocaine, has been extracted by Niemann; "other elements, hygrin and ecgonin, are found in it."

I conclude here this incomplete reference to our long and happily increasing list of anæsthetics, narcotics, and anodynes-stimulants—almost all of them nutritious generally as it seems clear enough to me, and in a certain sense tonic also—with the expression of a hope that, as each one of them possesses some serviceable property peculiar to itself, and adapted in some special manner to the promotion of human comfort, and the relief of human suffering, they will, all of them, at no distant period, be added to the resources of our *materia medica*, and made available in our improving and beneficent therapeutic.

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