

AN

INAUGURAL DISCOURSE

ON

*Presented by
L. B. Peckham*

MEDICAL ECLECTISM.

BY

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1835.

JAMES CONQUEST CROSS, M. D.

Professor of Materia Medica in the Medical College of Ohio.

DEAR SIR: By resolution of the Medical Class, we have been appointed a Committee for the purpose of soliciting a copy of your late introductory lecture, for publication.

This note is respectfully addressed to you in fulfilment of our duty; and, permit us to hope, that our request may meet, from you, a favourable reception. Your inaugural address presented a view so novel, comprehensive, and interesting, of the present state of the medical profession, that, in our opinion, it should be preserved.

HENRY H. RIVES, Tennessee.

J. A. MOORMAN, Tennessee.

F. A. BRECKINRIDGE, Michigan T.

ROBERT H. CRAWFORD, S. Carolina.

S. B. WEST, Ohio.

W. T. T. BUCKNER, Kentucky.

THOMAS J. HOWARD, Louisiana.

*Monday, Nov. 9, 1835. }
Medical College of Ohio. }*

CINCINNATI,

PEARL ST. HOUSE, Nov. 9th, 1835.

GENTLEMEN: I should be unworthy the station I occupy, were I not gratified at the flattering request which you, in behalf of the medical class of the Medical College of Ohio, have made of me. The views expressed in the lecture, to which you have the kindness to allude, though not novel to the medical public of France, will, I am persuaded, be, in a great degree, new to the majority of the profession in this country; and, if true, may serve, in some measure, to advance the interests of medical science.

While I express a willingness to furnish you with the copy you request, permit me to say, through you, to the gentlemen of the medical class, I sincerely regret that it is not more worthy their approbation, or the notice they have been pleased to take of it.

Yours, respectfully.

JAMES C. CROSS.

MESSRS. RIVES,
MOORMAN,
BRECKINRIDGE,
CRAWFORD
WEST,
BUCKNER, AND
HOWARD. } *Committee.*

INAUGURAL ADDRESS.

EPOCHS occasionally arrive in medical science from which the observer may take a more or less commanding survey of its past and of its probable future history. Such is, emphatically, the character of the present busy and enterprising period. In every sense, and under every interpretation, it is strictly, and in an extraordinary degree, eventful. Its checkered annals record no age so full of zeal, so prodigal of industry, or so opulent in the most wonderful and magnificent intellectual achievements, as the one in which we live.

From the days of the Coan sage up to the present interesting moment, the medical mind has been either haunted by the phantoms of superstition, blinded by the prejudices of fanaticism, or misled by the visions of system. During the middle ages, it humbled itself, obsequiously, before the shrine of an absurd monkish idolatry. After the reviviscence of learning in Europe, it clung, in hopeless despondency, to the gratuitous dicta of ancient authority, and, until within a very few years, and even at this moment, except within the boundaries of a single empire in Europe, it is still held in a state of the most abject and humiliating bondage, by the arrogant assumptions of system.

The rise, decline, and downfall of the different systems that have deranged and convulsed the science, from the time of Galen to that of Broussais, constitute, in truth, its literal and comprehensive history. This being the case, the physician, familiar with the events of his profession, must lament, and the heart of the philanthropist must bleed, to reflect upon the thousands of valuable lives that have been madly sacrificed upon system's unhallowed shrine. With a worse than heathenish infatuation, thousands are still throwing their unsuspecting offerings headlong upon the altar already crimsoned in human gore. But we trust the blind and fearful spirit of desolation, abroad in the valley of the Mississippi, will, ere long, be extinguished. How cheering to the spirits, and how animating to the hopes of the friends of humanity, to see the sun of real science rising in a far distant land; and although its feeble rays are yet scarcely felt amidst the deep and palpable darkness by which we are surrounded, it is rapidly mounting to the meridian, and then we shall have the whole vast field of medicine filled with a steady and unflinching blaze of light. I wish not to dissimulate—it is to France I make allusion. There, more ardor, enterprise, and intellect are employed in the cultivation of the profession than in any other country of the world; and there, too, unwearied effort has marked its triumphs by more numerous, important, and wonderful achievements. It is in France alone that the phantoms of superstition have been exorcised, the fetters of prejudice broken asunder, and the long and disastrous reign of systematic medicine has, at last, completely expired. Even that beautiful and magnificent structure denominated *physiologism* has been subverted, broken in pieces, and its shattered and glittering fragments rolled over and ground to dust by the chariot wheels of reason.

The name of Broussais, identified as it undeniably is with the present brilliant condition of pathological science, must live upon the bright and enduring page of history, an object of grateful admiration, as long as disease shall entail its miseries upon man, the science of medicine continue to be an object of liberal and dignified study, or its practice a useful and honourable profession. But his system, imposing as was its aspect, plausible its pretensions, and commanding its influence, has, already, ceased to be a subject of angry and bitter discussion. Assailed, on all sides, with the most unflinching ardor and unwavering resolution, the physiological chief has been defeated in every battle, driven from every position, and forced, at last, to surrender. avowing, in the face of the world, that he looked to posterity for justice, the present untoward and refractory generation being too unprincipled to be reformed by advice, and too stupid to be enlightened by instruction.

Systematic medicine no longer reigns triumphant in France. Those illustrious individuals, who carried forward, so victoriously, the crusade against the doctrine of irritation, and who have led the science forth from the jargon of the schools and the fopperies of the sects, have, to a man, repudiated the systems as the idle and groundless assumptions,

the wild and mystic speculations, of visionary men. Satisfied that medical science, in its present state of imperfection, cannot be comprehended or explained by any systematic principle that has been or can be conceived, they have abandoned the systems, and are now directing their united energies to the cultivation of medical eclecticism. Until all the facts of the science shall be fully evolved, they look upon this as the only rational course that can be pursued, with permanent advantage, to the interests of medical science. It is only by a close, scrutinizing, and methodical observation of disease that the rabble of lawless speculations and wild hypotheses can be routed, which retard and embarrass its progress, and which cripple and oppress its energies.

When physicians determine to study disease, as we have suggested, medical science will not only have entered fairly upon the career of rapid and enduring improvement, but a scheme of successful investigation will have been adopted which will place it, conspicuously, in the midst of those sciences most remarkable for precision and exactitude. Do I see the smile of incredulity playing around your lips? Do I hear the murmur of distrust breathed in this assembly? Are you already prepared to denounce me as a visionary conjecturalist? Do you maintain that the nature and constitution of medical science are such as absolutely to forbid the indulgence of anticipations apparently so extravagant? If, indeed, such be the fact, pause and reflect, I beseech you, before you finally decide; for, difficult and unreasonable as it may seem, this is what I have undertaken to demonstrate to day.

For systematic and eclectic medicine perfectly to agree and harmonize, is utterly impossible. From time immemorial, having waged against each other a war of mutual extermination, they feel for each other a deep and abiding hereditary hatred. In regard, however, to the attention they have attracted, or the respect they have commanded, their success has been strikingly different. While the former has been cherished, admired, and cultivated, the latter has been neglected, despised, and oppressed. Though it has borne the brunt of every war, fought every battle, and gained every victory, medical eclecticism has been treated with arrogant insolence, and shamefully spurned with contempt; while systematic medicine, though defeated in every contest, and driven in disgrace from every position, it has, nevertheless, usurped, and, without a moment's interregnum, wielded the sceptre of empire.

By whom, I ask, were the different systems of medicine defeated and disgraced? By eclectic physicians. They overthrew humoralism, they confounded vitalism, and have recently obtained a signally glorious victory over physiologism. Medical eclecticism has never quailed before the fierce frown of opposition, but, when brought in contact with systematic medicine, under any of its numerous chameleon changes of form, in the fair and open field of controversy, it has uniformly proved triumphant. If it has accomplished so much, won so many victories, is it not surprising that its name is so seldom heard, its character so little known, its reputation so very circumscribed, and its deeds so little honoured? No, I respond, this is not a subject of astonishment. Medical eclecticism, as it has been hitherto cultivated, has, in the language of phrenology, manifested the organ of destruction but not of construction. While its ability to demolish has not been denied, the existence of its capacity to build up was not, until recently, even suspected. Thus its resources and powers being misunderstood, its energies have been misdirected and misapplied.

I do not deny that from the earliest period of recorded medical history, the term eclecticism has been used. It is, at present, occasionally seen in books, and heard in the intercourse of professional men; but its real meaning and legitimate import is not fully or generally understood in this country. Every intelligent physician is able, without difficulty, to define the object of eclecticism, but I do not believe that I venture too far when I assert that no two of them will agree as to the means that should be employed in order to attain it; nor will there be, if any means have been instituted for this purpose, less discrepancy in opinion in regard to the results of any such efforts.

The object of eclecticism, when applied to the study of medicine, is to select from all past and present systems, and from all doctrines, whether in vogue now or prevalent formerly, the truths they contain, and to embody them into a doctrine that shall have experience for its foundation. Has this been done? No one will venture an affirmative response, and for a very obvious reason—the manner of its accomplishment was not, until recently, made known to the public. But few of us are so ignorant as not to be able to indicate objects the attainment of which would prove signally useful, but the difficulty consists in pointing out the means of their sure and certain attainment. The object of eclecticism has always been considered laudable, and its achievement a consum-

mation devoutly to be wished; but, for the want of the means of attaining it, its efforts have hitherto ultimated in nothing more than the defeat of the systems, as they have, in succession, entered the arena of controversy.

Eclectism, as its study has been hitherto prosecuted, has observed no avowed or clearly defined method. This being the case, the unproductiveness of its efforts is not a matter of surprise. By all the sciences it is justly regarded as utterly indispensable to the attainment of truthful conclusions. Without it, our knowledge of facts must always remain imperfect, and the principles of their classification mutable and unscientific.

Though the systems of medicine, without a single exception, have been, upon examination, found indefensible, their authors were sufficiently clear sighted to perceive the importance of attending to method. But a method may be vicious and lead to false conclusions. Unluckily for the systems, this is the rock upon which they have all been wrecked. Having proceeded, a priori, to the establishment of their principles, their method was defective. From assumed data, or from the partial observation of a limited number of facts, principles were deduced that were afterwards applied to the explanation of diseases which had not been previously analyzed, and which, consequently, could not have been understood. The truth of this allegation is fully demonstrated by the fact that every system that has ever prevailed has been found, when subjected to the ordeal of a keen and rigid investigation, not only defective, but absolutely indefensible. Had systematists proceeded, a posteriori, to the establishment of their principles, such a catastrophe could not possibly have happened. The experimental method, which consists in ascending, fact by fact, to a general proposition, would have been adopted, and which, if thoroughly practised and strictly adhered to, would have satisfactorily guaranteed the truth of their conclusions.

A system, to be true, must not only comprehend all the facts of the science, but it must satisfactorily explain them. Destitute of either of these essential attributes, it justly forfeits all right to be thus denominated. It is not sufficient that a system should show itself fully competent to the explanation of a limited number of the facts of the science. An hypothesis may do as much; and, though many facts may tend to prove it to be false, it is, nevertheless, an hypothesis. This, however, is not the case in regard to a system. From the moment it is ascertained that it does not include all the facts of the science, or give a satisfactory explanation of them, it ceases to be a system and becomes an hypothesis.

Having determined the character of a system, deduced from premises indisputably true, let us endeavour to ascertain, if possible, the nature of the efforts made by eclectism to advance medical science. It is useless to repeat that its investigations have not been conducted in accordance with any fixed or determined method. Repudiating the systems as false, and, in a great degree, unfounded, its exertions have been almost exclusively confined to the detection of their errors and the exposition of their absurdities and contradictions. Its criticisms, though unsparingly severe, have, generally, been candid, judicious, and dignified. By facts, the result of close and faithful observation, the systems have been, by eclectism, assailed at every vulnerable point. Unwearied effort and unflinching zeal have succeeded in demonstrating that, if not a tissue of assumptions, they are at least too defective to redeem the pledge which their name clearly implies. But while eclectism has rendered it manifest they are wholly inadequate to answer the ends of a system, and thus to place the science upon a permanent foundation, it has itself made no effort to impart to it a fixed and durable character. Although it has, lately, overwhelmed with terror and dismay the chief, and the disciples, of the physiological doctrine, upon the ruins of that wonderful monument of industry, zeal, and talent, no means have been taken to erect a more beautiful, finished, and substantial structure. Eclectism has not undertaken to cull and select from this, and the other systems, the important practically useful truths with which they, in different degrees, abound, in order to give them a fixed and durable place in the science.

The enemies of systematic medicine have been actuated by a too exclusive spirit of indiscriminate extermination. Because it was found defective as a whole, they have precipitately and erroneously concluded it to be false and untenable in all its parts. For this reason no effort has been made to separate the true from the false. Those principles, which were deduced from undeniable premises, have not been distinguished from those that were gratuitously assumed. Thus, you perceive, that while error has been driven from the field, truth has been so enveloped in the dust and smoke of the

contest as to be rendered invisible. While the fabrics of systematic medicine have crumbled to pieces, the truths which entered into their structure have been buried amidst the ruins.

Little as has been done by critical eclecticism towards the permanent advancement of the science, we shall find that the sphere in which practical eclecticism has been really useful, is quite as circumscribed. Were I asked what clinical principles have been developed, or established, by practical eclecticism, I should be constrained to respond, there are none, as yet, in existence. Why is this department so barren of improvement? Because the operations of eclectic physicians have been irregular, desultory, and unmethodical. Because they have pursued no general and recognized method, and because the truth of their conclusions were not tried by an established standard.

No enlightened or educated physician has, as yet, ever commenced his practical career without being a disciple of some particular system. The tenacity with which he will adhere to it will generally depend upon the extent to which it is founded in truth. Rarely does it happen that a reflecting physician, duly impressed with the solemn responsibilities of his profession, will pass through a life of anxiety and anguish, without apostatizing. Each succeeding disastrous year will bring with it melancholy proofs of the fallacy of the system he has adopted, and of its inability to furnish rational principles for his guidance in the treatment of diseases. He thus becomes, ultimately, an eclectic, and, as his experience renders him familiar with facts, he draws his conclusions and establishes his principles. But in doing this, he is guided by no method, and although he appreciates truth when it is forcibly and palpably obtruded upon his attention, he is entirely destitute of all such means as would direct him to its certain discovery.

Perhaps it may be alleged that practical eclecticism may be safely confided to the sagacity and judgment of the physician. I admit there are many whose enlarged and comprehensive minds, whose powers of deep and penetrating thought, and whose habits of close observation and rigid scrutiny are such as to fully qualify them to master this difficult and complicated subject. With a judgment that seems almost intuitive, they separate truth from error. Vague, delusive, and inconclusive analogies never prevail with them to confound those things that should remain distinct; nor do false appearances of dissimilitude ever induce them to sever those that should not be put asunder. No one, however, will venture to maintain, from such premises, that practical eclecticism can safely dispense with, or be successfully cultivated, without an established and determined method. Although there may be minds of a grasp so comprehensive as to be independent of all such assistance, to the great body of the profession it is altogether indispensable. Without a method to guide them in their investigations, what, I ask, is to protect from the commission of the most absurd mistakes, the most fatal blunders, and the most flagrant enormities, that crowd of physicians who grope their obscure and devious way through life, under the faint glimmering of a light shed by more luminous minds. Destitute of a method settled and defined, as well as universally recognized and understood, it would be utterly impossible for eclecticism to furnish a rational or satisfactory guarantee of the truth of its conclusions. That multitude of physicians who are guided by authority, and to whom precepts as well as principles are altogether indispensable, would never be able to profit by their past experience, nor would their labours prove, in any degree, available towards the advancement of the science. Method is essential to the development of precepts, to the establishment of principles, to the discovery of truth, and, without it, they must continue to blunder on through a life of inexpressible disaster.

Submit the same case of disease to the observation of two eclectic physicians, and let us suppose that they perfectly agree in ascribing it to the same species. Will they have arrived at this conclusion by observing the same process? If so, will they be able to make you understand the different steps of this process? I think not. But if this were possible, would they be able to make a practical application of it to all other cases? Would they be able so clearly to define it and to render it so intelligible, that it would serve to guide others in a similar manner? Assuredly no; and the reason of it I consider obvious. Having been guided more by the dictates of an instinctive judgment than by a close observation of facts and a logical deduction of conclusions, it is perfectly manifest that their process cannot be defined, communicated to, understood, or followed by another.

Laying no just claim to the possession of a specific method, but acting under the influence of isolated and individual impulses, it is quite impossible for the conclusions of eclectic physicians to be otherwise than vague and uncertain. Ask them if they believe in the existence of bilious, typhus, and puerperal fevers? and the answers they

will make you must soon satisfy you of the truth of this allegation. Instead of responding in a clear and definite manner, you will be promptly informed that they are not exclusives, that they are not partisans of any particular theory, doctrine, system, or mode of treatment, but that they select, as if, indeed, the task were of easy performance, those principles and practical precepts, the truth of which has been demonstrated by experience. They will not, however, define or render intelligible this eclectic process, because, being more instinctive than intellectual, it is insusceptible of scientific definition. Should you inquire of them what principles of interpretation, or modes of treatment, have been firmly and immutably established in regard to the diseases just alluded to, you will find them involved in difficulties still more embarrassing. The particular results of the labours of eclectic practitioners cannot be pointed out, because, in fact, they are, except through the eyes of intuition, even invisible to themselves. Though they profess to act upon those principles, the truth of which has been satisfactorily confirmed by reiterated and manifold experience, the eclectic cannot tell you in what they consist, or, precisely, where they are to be found. He pretends to distinguish error from truth, to reject the former and preserve the latter, yet he has not drawn the line of demarcation between them, nor can he draw it. Require of him to prescribe for a case of disease. Will he pursue any particular method, or will he hearken to the wise suggestions of any particular principles? Will he give you a practical illustration of that elective acumen by which he pretends to distinguish truth from error? Most assuredly he will not. Under the idle pretext of not being an exclusive, he will, falteringly, pursue a tame and pusillanimous practice, or a mixed and vacillating medication.

As the processes of eclectics are, in a great measure, instinctive, and altogether undefinable, it follows, irresistibly, that the conclusions of eclecticism, as it has been hitherto cultivated, must be vague, mutable, and controvertible. Imperfect, however, as they evidently are, and, although in their present state, insusceptible of being subjected to any rules of scientific definition, they, nevertheless, contain the elements of a general method, which, when properly arranged, may be defined, understood, and successfully applied to the study of disease. This is the great desideratum in eclecticism, and it is because of the want of this that it has had to contend with so many difficulties, and its labours have, hitherto, proved so very unproductive.

The practicability of introducing a method which shall be as true and unerring in all its details, as in its general results; which, while it shall be applicable to each particular case, will lead to the firm establishment of general conclusions, I consider susceptible of the clearest demonstration. In all the sciences their practice has invariably preceded their philosophy. This must always be the case, for, without a knowledge of the former, no intelligible or rational view could be obtained of the latter. Indeed, the latter can only be deduced from materials accumulated by the former. Practical medicine, therefore, must be well understood before we can, with success, even engage in the study of the philosophy of medicine. The former is the foundation, the latter the superstructure. Those, therefore, who would contribute, materially, to the advancement of the science of medicine, must bend all their energies to the task, completely divested of all systematic bias, uninfluenced by any favourite preconception, unawed by the dicta of authority, and determine fairly, thoroughly, and impartially to study disease, at the bed-side of the sick, as it actually exists.

We have asserted that the introduction of a general method into the study of disease is practicable, and that its elements are to be found in the partial and undefined processes of individual eclectics. In order to furnish an abstract of this method, the results of the researches of critical and practical eclecticism must be properly classified, and strictly but candidly interpreted. Before this is accomplished, it will be impossible for theoretical eclecticism to have an existence.

In the first place, let us ascertain the course of conduct which has been observed by critical eclecticism towards systematic medicine, under the many chameleon changes which it has, at different times, experienced. Systematic medicine has always contended for the truth of some assumed principle. The object of critical eclecticism has always been to test the truth of this principle by close and impartial observation. It, therefore, conducted the abstractions of systematic medicine into the clinical ward, and there confronted them by disease, as it really exists, and not as floats in the excited imagination of the enthusiast. It is there that the eclectic physician has been enabled to determine, with precision, to what extent they are deduced from grounds of unimpeachable probation, and how far they have been hypothetically assumed. While the eclectic investigates

disease in the field of personal observation, the systematist dreams of it in the silence and solitude of his closet. While the former slowly ascends, fact by fact, to a general principle, the latter gratuitously assumes the principle, and then, violently, coerces and distorts facts into a reluctant support of it.

Although it cannot be denied that the course which eclecticism has pursued in the investigation of disease is, in principle, correct, it must be admitted that it has not been fully carried out, nor has it been, with sufficient zeal, persevered in. Indeed, had this been the case, had it been fully and thoroughly applied to the study of all the elements of disease, it would be the experimental method of Bacon. A principle established upon any other foundation must be false, for without it it would be impossible for science to have a single demonstrated truth. The experimental method, when strictly applied, consists in ascending, fact by fact, to the primitive and fundamental truths of science. Without being aware of it, this is the precise course which has been instinctively followed, though in a manner decidedly partial and imperfect, by medical eclecticism.

The reason why eclecticism has made but a partial application of the experimental method to the study of disease, is susceptible of the clearest explanation. Eclecticism has not sought truth for truth's sake, but rather because the systems asserted falsehoods. Its object has been not so much to discover the former, as to detect and expose the latter. Instead, therefore, of arranging and embodying the truths which were, by its efforts, from time to time, developed, it suspended its investigations the moment it had acquired the means of fiercely assailing and successfully exposing the absurdities of the systematic abstractions. Thus, instead of applying the method to which allusion has just been made to all the elements of disease, it was applied to those only which were essential to the active prosecution of hostilities against its ancient hereditary enemy.

While the course which practical eclecticism has pursued in regard to the systems is not fundamentally unlike that of critical eclecticism, its efforts have not been less effectual in exposing their absurdities. By the latter it has been demonstrated that the systems comprehend a limited number only of the facts of the science, while they arrogantly pretend to give a satisfactory explanation of the whole of them. Upon practical eclecticism devolves the duty of showing whether or not diseases do, in truth, present the pathological characters to which they have been ascribed. To accomplish this important object, it subjects not only every species of disease, but every individual abnormal affection, to the strictest scrutiny. Thus, is it his purpose to determine the solidity of the pretensions of the physiological doctrine; and is he called upon to treat inflammation of the stomach, the result of the action of some corrosive substance upon that organ? The fever, epigastric pain, nausea, vomiting, colour of the tongue, and, indeed, the whole cortege of symptoms, manifestly disclose the existence of gastro-enteritis. The physician is plainly admonished that the stomach labours under the type of that organic affection which constitutes the basis of the doctrine of irritation. But, instead of a train of morbid phenomena so easy of interpretation, let us suppose the eclectic called to treat a case of intermittent fever, of bilious fever, of yellow fever, or of Asiatic cholera. Will he find in the several groups of symptoms which these diseases respectively present, exact transcripts of the prototype to which we have just alluded? He will not, most assuredly. The first glance convinces him that they are unlike simple gastritis, and that there must be something radically unsound in the doctrine of physiologism. He, therefore, institutes between the case of genuine gastritis and those affections, the names of which we have just enumerated, a comparison, and, so far from finding them identical with it, or different degrees of it, he is irresistibly led to conclude, from a total want of correspondence between the phenomena they present, that they are separate and distinct diseases. In all this, however, we do not discover that he has followed any certain process, or recognized method; he has simply furnished us with an exemplification of that kind of routinary repetition so common among physicians. He is thus in the daily practice of confirming, or of refuting, a posteriori, principles that had been transmitted to him, a priori. He, indeed, pursues, unconsciously, to a certain extent, the experimental method.

I have said that he follows the experimental method but to a limited extent, and more than this, in the present condition of the science, it is impossible for the critical and practical eclectic to do. It is perfectly evident that the physician can never, with propriety, or success, extend it beyond those facts that fall directly within the sphere of his personal observation. Efforts to do this have only ultimated in the conception and propagation of error. But partially or generally applied, it must be confessed that it is, nevertheless, the experimental method.

Satisfied it is the experimental method only that can lead to the successful discovery of truth, the systematist maintains, with obstinate pertinacity, that he has followed it and strictly complied with all its requisitions. He not only contends that his principles are deduced from facts, but that they rest securely upon a basis into the structure of which all the facts of the science enter. If this were true, their soundness could not be safely impugned. Indeed, resting upon grounds of direct probation, they might bid defiance to the fiercest and most unrelenting opposition. But it is the truth of this allegation that the eclectic denies. He asserts that the systematist has neither frankly pursued it, or faithfully complied with its requisitions.

To decide between the parties involved in this controversy would be a matter of considerable difficulty, were we limited in our resources to one mode of investigating a fact. Were facts perfectly homogeneous and indivisible totalities, the point at issue would constitute a problem of very embarrassing solution. But such is not the character of the facts physicians are called upon to examine. The facts they have to investigate are diseases. What are diseases? Not homogeneous and indivisible totalities, but things that are composed of different and distinct elements. All these elements, however numerous or diversified, must be studied in detail and in the aggregate, or disease can never be fully or satisfactorily understood.

To accomplish this, no other process can be successfully adopted, but the analytic. If analysis be perfect, if it be full and not partial, the disease, however complex, must be resolved into its fundamental elements.

Do you ask how the systematic process is to be rendered perfect? Examine and compare all the partial analyses of the systems, as well as those of eclecticism, and deduce, from them, a complete analytic process. This is practicable:—for, as we have already remarked, they contain its elements.

The examination of a few diseases will not only prove to you that the analyses of the systems are partial, but it will show you how analysis may be rendered perfect. You will, in addition, perceive when the systematist asserts that he has faithfully pursued the experimental method, that he is guilty of giving utterance to an idle and independent assumption.

To show the imperfection of the analyses of the systems, let us select the doctrine of irritation. It is the most recent, the most popular, and the most plausible. M. Broussais admits only one species of disease. All diseases consist in different degrees of irritation, and this irritation, itself, is nothing more than the exaggeration of a physiological property.

It would be absurd dogmatically to affirm that there are no facts which impart some degree of plausibility to physiologism. This no one will deny. But it is not our business to inquire, is it plausible? but rather, is it absolutely true? This is the question that requires a response, and if it is to be in the affirmative, it must be shown that it complies with all the requisitions of a system. It must appear that it embraces and explains all the facts of the science.

Let us now see how disease has been analyzed by the physiological doctrine. It confounds syphilis with common inflammation. How has this been done? By selecting those symptoms only that are decidedly inflammatory, together with the organic lesions by which they are produced, and comparing them with common organic inflammation. This analysis is manifestly defective, but by the eclectic it is rendered complete, by including the cause, which he justly regards specific. The physiologist, though he has studied the symptoms and the organic lesions, has paid no attention to the most important and characteristic element of syphilis. We allude to its cause, which is doubtless specific; for syphilis never results from any of the numerous causes of common organic inflammation. Thus you perceive their pretended analogy is altogether destroyed.

In regard to those fevers considered typhoid, the attention of the physiologist is wholly engrossed with the symptoms characteristic of local reaction, and these he ascribes to organic lesions of the gastro-intestinal mucous membrane. Now the truth is, the symptoms here alluded to have no existence for hours, and even days, after the disease has fairly commenced. Thus the first period of the disease is altogether overlooked in order to make it appear that it commences with those symptoms that denote the existence of a local lesion. This imperfect analysis is rendered complete by the eclectic, who considers the first period of the disease, for the symptoms by which it is characterized, are always present, as one of its most essential elements, and which, instead of

pointing directly to a primitive local lesion, denote the implication of the whole living organism.

With regard to intermittent fever, the physiologist founds his analogies upon the symptoms of the paroxysm, and refers them to organic lesions which are conjectured, for, as yet, their existence has not been satisfactorily demonstrated. Indeed, the researches of pathological anatomy prove that they are mainly hypothetical. The eclectic, however, holds it to be essential to the discovery of truth to study the symptoms of the intermission as well as those of the paroxysm, to point out their genealogical succession, their cessation during the interval, as well as their periodical recurrence. Thus you perceive that the symptoms which characterize local reaction, even when they exist, do not constitute the only element of intermittent fever.

We do not deny that some of the symptoms of inflammation of the stomach are palpably evident in bilious fever. Of this kind are nausea, vomiting, red color of the edges of the tongue, and epigastric pain. But who will contend that this picture comprehends all the conspicuous features of the latter disease? Independently, however, of the entire want of correspondence in regard to the symptoms, characteristic of the two diseases, the eclectic denies their identity, because they do not yield with equal promptitude to the same treatment. While experience has demonstrated that emetics and cathartics are of the utmost utility in the treatment of bilious fever, it has shown that in gastritis they produce the most disastrous results. Thus you perceive that while the physiologist has given you a very defective and partial account of the symptoms, he has altogether overlooked the treatment.

Variola is regarded by the physiologist as merely a cutaneous inflammation, because his whole attention is engrossed with the eruption by which it is accompanied. That this is a false and partial view of the subject is proved by the fact that its cause and nature are specific. The causes of ordinary organic inflammation have never produced it, nor can they.

We might thus proceed through the whole catalogue of human maladies, and, at every step, we should only receive fresh evidence of the fact that where the systematist, either blinded by prejudice, or influenced by preconceived opinions, can discover only analogies, nature has exhibited discrepancies which the eclectic has not failed to detect and expose. From what does this arise? Not from a fundamental difference in the species of process employed, for that of each of them is the analytic, but from the manner in which it has been employed.

By the systematist the analytic process has been employed for individual and selfish purposes; but by the eclectic to discover truth. By the former it has been used partially, but by the latter more thoroughly. By the former a few elements of disease have been developed, but by the latter, many, and, perhaps, all of them.

You have seen, in the diseases to which we have just alluded, that eclecticism has detected elements which physiologism has neither noticed or explained. Its analytic process is, therefore, partial and incomplete. To be valid it must be full and thorough. It must not only comprehend the elements which it has itself developed, but it must comprise, in succession, all of those that have been signalized by eclecticism. Analysis, to be perfect, must not only embrace all the different parts of a disease, but all the circumstances upon which it depends, or which depend upon it.

Seeing so signal a difference in the manner in which the analytic process has been employed by the systematist and the eclectic, you cannot be surprised that the results of their researches should be strikingly unlike. The physiologist confining his attention exclusively to the consideration of the symptoms, and the organic lesions, has, unconsciously, exaggerated their importance. By thus endeavoring to impart to them an interest to which they are not legitimately entitled, he has contributed to advance the science by making them better known. The physiological doctrine, however, has no peculiar claims to respect on this particular account. This has been the result of every plausible system that has hitherto prevailed. They have all, to a greater or less extent, enlarged and enlightened the sphere of medical knowledge. Obstinate maintaining that their principles have been deduced from facts, their partizans confidently demand that the truth of them be put to the test of actual observation. This has been done, and although they have never passed unscathed through the ordeal, we are constrained to admit that certain truths have entered into their structure which merit the greatest respect. Each system embracing the study of one or more elements of disease, they have consequently been fully developed and thoroughly understood. But for vitalism we

should not now be so familiar with the symptoms that belong to the unity of the living organism. To humoralism are we indebted for much that is known of the causes which produce changes in the constitution of the fluids of the body; of the nature of those morbid alterations, as well as of the critical phenomena by which they are followed. In like manner, do we owe a heavy debt of gratitude to the author and partisans of the doctrine of irritation. By their labors, the sphere of pathological anatomy has been enlarged and enriched by the most useful discoveries. Previously it was to the pathologist either a terra incognita, or a confused collection of unintelligible facts; now it is a science in the strictest sense of the term, not only interesting and attractive, but, in an eminent degree, practically useful.

When we think of the many systems that have, at different times, prevailed, and know that no two of them have undertaken the investigation of exactly the same elements, it will not appear unreasonable to assert that the most, if not the whole, of them have been studied. Though this has been the result of the conjoint labours of the friends of systematic medicine, no one will pretend to allege that any system has succeeded in imparting to medical science a distinct or permanent character. They have all been, to a greater or less extent, popular, but the influence which they have exerted has always been transient.

The impression made on the aspect of the science by the systems, as they have, in succession, prevailed, could not have been otherwise than temporary in its nature, as their premises were, in a great degree, hypothetical, and their conclusions more general than the facts which had been previously examined, legitimately authorized. Seeing and appreciating the defective modes of investigation adopted by the systems, it has been the object of eclecticism to introduce into medicine a full and complete analytic method. This is indispensably necessary to the attainment of truthful results; and, from the preceding considerations, you must admit, with me, that it has been accomplished. Thus, you perceive, that by the labours of our predecessors, two signally important objects have been achieved. Systematic medicine has fully evolved most, if not all, the different elements of which disease is composed, and eclecticism has substituted in the place of the defective systematic modes of investigation, a general analytic method.

We must now point out to you how, by a proper use of the materials furnished by the former, and of the instrument of medical inquiry practically applied by the latter, will result a science, though still susceptible of improvement, permanent in its character, and no longer subject to the fluctuations and changes which proceed from party zeal and sectarian influence.

Medicine can be made to possess much of the precision of an exact science. To effect this, three objects must be accomplished. In the first place, diseases must be known before they can be classified; secondly, the laws by which they are governed must be understood before they can be explained; and, lastly, we must be able to cure them.

When I say you must know a disease, I do not mean that you are to know it as does the humoralist, the vitalist, or the physiologist, each of whom looks at it under one only of its numerous phases. You must be able to recognise its existence by its peculiar and characteristic features, by its appreciable and sensible elements. You must know in what respects it differs with other diseases, and to what extent they are analogous. In truth, you must be able to recognise it as you would a plant or an animal. Without such knowledge as will qualify you to do this, you will never be able to group together those that present real affinities, or to separate those that are fundamentally dissimilar.

There are but three modes by which such knowledge as we have just referred to can be acquired. These are the empirical, the systematic, and the eclectic. To imagine or conceive a third is impossible. They all agree on one point. It is admitted by all the world that a knowledge of disease can only be acquired by personal observation.

Although the empiric, the systematist, and the eclectic are equally dependent upon observation, each conducts it in a manner peculiar to himself.

What is empiricism? It is the chaos of medical science; it is a heterogeneous compound of undigested, unarranged, and unintelligible facts. Surely we may spare ourselves the trouble of its formal refutation. Empiricism has been employed, exclusively, in the accumulation of disjointed, unconnected, and isolated facts. It has neither attempted to classify them, to discover their laws, or to explain them. It regards each disease as a separate, distinct, and isolated individuality. It neither studies or understands how they agree or differ with one another. Its observation, therefore, instead of being minute and analytic, is superficial and general.

Setting out from an a priori principle, or, in other words, having drawn a general conclusion before he had analyzed all the facts, the systematist comes to the business of observation with deeply rooted prejudices, and firmly established preconceptions. He thus sees disease not as it actually exists, but as his system has conceived it. His object is not to study and verify the phenomena as they are, in succession, presented, but those only that may be interpreted, or forced to give a reluctant support to the fundamental principle of his reasoning. In proof of the truth of what I have just asserted, submit the same case of disease to the examination of the humoralist, vitalist, physiologist, and I might add, were I disposed to introduce into society so respectable, the vena cavitist, and observe the result. Each of them will be obstinately blind to every phenomenon that does not either directly or indirectly tend to give validity to his particular dogma. Thus the humoralist sees those symptoms only that indicate a morbid state of the fluids; the vitalist those only that denote an alteration in the living organism; the physiologist those only that betray the existence of gastro-enteritis; and the vena cavitist those only, if indeed there be any, that apprise us of the preternatural plenitude of the vena cava.

This subject, however, merits a fuller development. Suppose, for the sake of illustration, a case of typhus fever, in full possession of all those circumstances that render it complete, submitted, in succession, to the observation of the vitalist, humoralist, and physiologist.

In this examination the vitalist will take, comparatively, but little notice of the particular cause to which the disease owes its origin. His whole attention will be engrossed with those phenomena which he supposes indicate a conflict between the morbid and vital principles. He neglects the local, while he studies the general symptoms, the former being subordinate to, and dependent upon, the latter. While he is regardless of those evidences of local reaction that denote the existence of organic lesions, he observes the march of the disease, the correspondence of its periods, as well as its critical phenomena. Do you ask why he is thus exclusive? I answer, because they support his dogma, because they are re-actions of the vital organism, all the efforts of which, directed by an instinctive, if not intelligent principle, conspire to overcome or to expel from the animal economy, the morbid principle. Let us here remark, as we shall have occasion again to refer to it, that the phenomena signalized by the vitalist in typhus have a real bona fide existence. But his picture of the disease is imperfect, having omitted several of its essential elements, while to those which have engaged his special attention, he has endeavored to impart an exaggerated and indefensible importance. It must, however, be confessed, considered apart from his fanciful and gratuitous interpretation, that he has exhibited in bold and striking relief, several elements of the disease, such as the crises, the periodical recurrence of fever, and the consensus of the whole of the vital organism, which can only be regarded as substantial, permanent, and highly important acquisitions of the science.

When the humoralist examines the same disease, he inquires, as the first step in his analysis, as to the particular nature of the cause by which it has been produced. This he must either actually ascertain, or assume, because it is the key to all the subsequent phenomena. In the second place, he observes the symptoms of the disease, its progress, treatment, and termination. All its varied features he considers expressive of the pervading influence of vitiated fluids. The several forms of febrile reaction command his most submissive attention, because they are regarded by him as eminently defeating processes. The number, variety, and peculiar nature of the excretions are, to him, subjects of earnest contemplation, because they are the vehicles in which the morbid principle is to be floated out of the economy. While, however, these circumstances engross his undivided attention, he never dreams of the existence of a phlogistic condition of the brain and intestinal tube, although pathological anatomy has demonstrated this morbid state to be a frequent occurrence in this disease.

By the physiologist, typhus fever is circumscribed within still more narrow limits. What are the objects that concentrate his attention into so small a focus? Symptoms of irritation and inflammation, and the organic lesions observed upon the cadavre. To inquire into the nature of its cause, he considers idle and nugatory, as all causes are alike, in his estimation, and produce identically the same effect, irritation. He denies the existence of specific phenomena as the effects of a specific cause, and looks upon the general reactions of the vital organism, as the offspring of a primitive local lesion. Thus you perceive that he selects, from the apparatus of morbid phenomena which

characterize typhus, but a single element, to which all the others, however numerous or varied, are precipitately referred. He often overlooks, altogether, the first period of the disease, that of general reaction, in order that he may make it appear that the disease has not fairly set in before the development of epigastric pain, which, according to the physiologist, is the first, second, last and only link in the chain of associated, morbid action. In his estimation, the periodical revolutions of fever, and the phenomena that mark the approach, the existence, and the termination of the crises, are unworthy of the slightest attention. The consideration of these elements of the disease is not omitted because he denies, or disbelieves in, their existence, but because it is much safer to remain silent than to attempt their explanation.

Are you not now satisfied that the observation of the various sects of systematists is conducted precisely upon the same plan, though they operate upon different materials? I feel persuaded that you are. I have shown you that the partisans of the systems respectively circumscribe and limit the sphere of their observation to those elements of disease that concur to support a principle of a priori conception. The importance of these elements they have exaggerated. Their alliance with others of equal value and interest, between which there exists an indissoluble natural connexion, they have violently broken up, and have thus rendered it impossible for them to comprehend their real value, to understand their mutual relations toward each other, and also the connexion of the different parts with the whole.

Let us now observe the faithful and enlightened eclectic physician examining the same disease. Having no personal or private interests to subserve, he shrinks with a deep, abiding, and instinctive hatred from the employment of the imperfect and delusive modes of observation to which we have just alluded. The trick and cunning, the paltry manoeuvres of the special pleader, he considers not only unworthy the honest inquirer, but absolutely disgraceful to the pride of the human understanding. Enamoured of no particular theory, he admires, but does not magnify truths; he exposes, but does not exaggerate, errors; he repudiates the passive and inefficient neutrality of the empiric, while he denounces, with unsparing severity, the partial and misguided zeal of the systematist. In a word, he adopts and actively enforces the employment of the experimental method, subjecting each particular fact to a thorough and comprehensive analysis. Thus the eclectic physician, in regard to typhus fever, inquires into the circumstances that conspired to produce it. He must know in what season it has occurred, whether it has resulted from contagion or from the influence of meteorological causes; whether from the crowding of multitudes together in a small, filthy, and imperfectly ventilated apartment, or from the more diffused influence of endemic or epidemic agency. He then follows, with scrupulous care and unwearied assiduity, the chain of morbid phenomena, link by link, from the first eruption of the disease to its final termination. He studies them in their generation and succession, the general as well as the local phenomena; he considers them in detail, and, also, in their totality. Does it exhibit apparently isolated and unconnected complications? He scrutinizes their nature, sees their bearings, and unites them, by natural bands, to an anterior or posterior period of the principal disease. Its stages and periods, its periodical revolutions, its critical efforts, are all subjected to the strictest analysis. He does not confound, but distinguishes, with accuracy, the effects of his remedies from those that arise from the natural evolutions of the disease. Finally, should death, in despite of the wisest treatment, be its termination, he closes his observation by an inquiry into the pathological lesions that have taken place. This, however, he does not do with the view to discover its seat and nature, but that the whole of its elements may be fairly and fully evolved. Not finding, uniformly, the same kind, or the same degree, of organic lesion, in every case, he endeavours to ascertain those that correspond by their antiquity or modernity, with this or that epoch of the disease, and with this or that group of symptoms. He compares these lesions with those observed in other cases, and points out in what respects they agree or differ. He observes the colour and consistence of the blood, the relative plenitude of the arteries and veins, and the condition of the solids. He must determine if there is one or various kinds of organic lesion, and discover if it is circumscribed to a single organ, or if many have been implicated. Thus you perceive that the eclectic applies to the study of disease, not the partial and incomplete analyses of the systems, but an analysis full, complete, and comprehensive.

Are you not now satisfied that observation, admitted by all the world to be the only legitimate and practicable means of acquiring a thorough knowledge of disease, is, as practised by the empiric, superficial and unproductive; by the systematist, partial and

delusive; but by the eclectic, complete and methodical; and that it must prove conducive to the most rapid and triumphant advancement of the science of medicine? But do you ask me if eclecticism is indebted exclusively to the employment of the experimental method for its superiority? To this question I answer you, no; and no one will have the presumption to make the allegation. It is but recently, and only in France, as yet, that it has been brought formally into use. Although but lately practically acknowledged to be indispensable to the certain discovery of truth, it has already acquired great influence, and is now floating, buoyantly and gracefully, upon the tide of popular favor. It now exerts the same empire over the mind of the medical public in France that has been, for so many ages, exerted by systematic medicine, with the most despotic sway. Succeeding, therefore, to the dominion of the systems, eclecticism is called upon to do little more than to employ its energies upon the facts that have been analyzed and elucidated by them. Systematic observation, independently of its theoretical views and objects, has devoted itself, with assiduity and zeal, to the study of certain elements of disease. These it has developed and made known to every enlightened physician. Each system attaching itself to certain elements peculiar to itself, the researches of all the systems, in the aggregate, have ultimately in the evolution of nearly, if not all, the elements, that enter into the constitution of disease. The business of eclecticism now consists in the union, into a distinct group, of all the characters which belong to the same individual, that we may have a precise and comprehensive, but not exaggerated idea of every distinct form of abnormal action. This we have already done in regard to typhus fever. Its numerous elements were subjected to the partial observation of the vitalist, humoralist, and physiologist, in regular succession. They have investigated it under all its different aspects, and have developed all its various elements, and it now remains for eclecticism to associate them together as they naturally exist; to assign to each its proper location, and to exhibit them in the order in which they are successively developed.

This, doubtless, should be a chief object of eclecticism; but its efforts must not be limited to the labors and achievements of systematic medicine. There are, doubtless, elements of disease which have escaped the partial observation of the different systems. These eclecticism must ferret out and signalize. Those lacuna, and they are numerous, that have been left by the partial investigations of the systems, as well as by the irregular and marauding labors of critical eclecticism, must be filled up by the efforts of methodical eclecticism.

Do you require other proofs and illustrations of the practicability of the views I have ventured to express? Then I appeal to the science of botany. The same obstacles with which medicine has had to contend were encountered, and triumphantly overcome, by that beautiful and interesting department of human knowledge, before it assumed its present imposing and permanently fixed character. After struggling, for ages, with the systems, the eclectic method, or, in other words, the full, accurate, and comprehensive observation of individual plants, was introduced. What, I ask, is the natural method of Jussieu, applied to the study of plants, but eclecticism, as we have defined it, applied to the observation and classification of diseases? What was the condition of the science of botany, when Jussieu introduced his natural method? That of the science of medicine at this moment. It was, like medicine, a heterogeneous compound of incongruous, conflicting, and fluctuating systems. One system took the root as its fundamental character, and plants were distributed accordingly; another the leaf; a third the flower; a fourth the corolla, and the last the sexual organs. Here you see illustrated the partial observation so characteristic of systematic medicine, and it terminated in precisely the same results. Each system succeeded in making fully known the part of the plant which was taken as its fundamental character; and the aggregate labors of the different systems, has achieved a full knowledge of all its various parts. It was not, therefore, until after the different systems had completely elucidated the particular character which they, respectively, considered fundamental; it was not until after the absurdity of the systems had been demonstrated by the unnatural grouping together of plants the most dissimilar which they occasioned,—it was not, in fact, until after we were made thoroughly acquainted with the root, stalk, leaf, flower, and sexual organs, that any attempt was made to found a classification upon all these characters at once. It was then, and then only, that botany commenced to be a positive science, based upon the thorough and methodical observation of eclecticism.

You now perceive what has been done for the science of botany by eclectic observation, and we ask, what would have been done for the science of medicine, had it been

applied to the study of disease? All the facts of the science would have been analyzed and the elements of each individual would have been made fully known. After this had been done, each of them, to speak familiarly, would have been properly labeled, and then those would have been grouped together that are analogous, and those separated that are dissimilar. In other words, the synthetic process would have been resorted to. Every definition, like every exact science, to be complete in all its parts, must, after having been analyzed, be submitted to the process of synthesis. In like manner, to ensure absolute certainty, every synthesis should be preceded by analysis. Now, the point to be determined is, whether synthesis can be successfully applied to the study of medicine. Of this I have no doubt, and shall now endeavor to show its practicability.

If the science of medicine were still in its infancy, in making an application of synthesis we should be obliged to rest satisfied with mere probabilities. Invoking the analogy of the other sciences, we should present the synthetic results to which they had been conducted by the experimental method; and from these we should be able to infer, with some degree of certainty, to what extent it could be employed, with success, in medicine. But the science of medicine, so far from being in its infancy, is rapidly advancing to maturity; and, notwithstanding the imperfection of the methods resorted to, numerous important acquisitions have been made to which we may, with confidence, appeal. Systematic medicine, together with the desultory efforts of critical eclecticism, have ultimated in certain results. If the sphere within which these results are true, has been, or can be, prescribed; if they are so firmly established as to be relied upon amidst all changes; and if they are indebted for this to the employment of method, they are so many proofs of the solidity and perfection of which the science is susceptible, when associated with other results of a similar character. Then we have only patiently to submit to the dominion of method all the synthetic results of which the science is already in possession, in order, within proper limits, to circumscribe and successfully and satisfactorily to classify them.

What are intermittent, bilious, adynamic, and typhoid fevers? what rheumatic and arthritic diseases? and what organic inflammations? They are partial syntheses, which have resulted from the various doctrines and systems that have, at different times, prevailed. These syntheses may be considered under two points of view. An example will explain and illustrate my meaning. When I use the phrase intermittent fever, I understand the union of several elements. I embrace the cause, symptoms, treatment, and organic lesions. The whole of these are elements of a morbid individuality. The phrase, intermittent fever, designates an individual synthesis. When I use the same term to express a species, a certain number of diseases that have been defined, circumscribed, and generalised, I perform a synthesis of a species. Keeping in view this definition, you at once perceive, that there are already in the science syntheses of individuals and syntheses of species. In a word, you discover that diseases have received names, and that attempts have been made to classify them. Let us now see to what useful purpose eclecticism will be able to convert the syntheses that already exist.

When we confer a name on any object whatever, we should not only aim at making that object known, but the name itself should remind us of as many of its characters as possible. No name should be imposed upon a disease that rests on hypothetical grounds, or that prejudices any thing of its character that is not already understood and established. Nor should the synthetic designation of individuals express any circumstance that is in opposition to the synthetic denomination of the species, whether this has reference to the symptoms, seat, or particular nature. There are already, in the science, many names of individuals, in conformity to these principles. What course should be pursued by eclecticism, to guarantee their preservation? All those diseases that bear the same name should be, in succession, analyzed in order to discover if any of them exhibit such features of dissimilitude as would destroy the analogy upon which the species is founded; and if there are such, to ascertain if they are so unlike the type the most proximate to the first, as to render it necessary to throw them into a new and distinct division. Thus the denominations intermittent, bilious, and typhoid fevers, and gastro-enteritis, might be simultaneously retained, as they are diseases that not only exist, but differ so widely from each other, as to render it almost impossible to confound them.

We must now inquire, in what manner eclecticism will be able to prove the correctness of the names of the individuals and of the species to which allusion has been made? In other words, how will it demonstrate that the phrase, intermittent fever, when applied to

designate an individual and also a species, expresses a legitimate difference with gastro-enteritis, in this two-fold capacity.

To ascertain if intermittent fever and gastro-enteritis should be regarded as two different and distinct types of diseases, the elements of both must be subjected to a full, methodical analysis. This will show that neither in regard to the respective circumstances in which they are developed; the causes by which they are produced; the symptoms by which they are characterized, whether examined in detail or considered collectively; whether in their manner of generation or mode of succession, nor their treatment, or the organic lesions discovered after death; that there exists between them any identity of character whatever. From the marked difference in the details, you will deduce an irreconcilable difference in the aggregate.

The second point is to know how eclectism will determine if intermittent fever and gastro-enteritis, generalised to a certain extent, may be regarded as types of species. The methodical analysis, just alluded to, will readily and satisfactorily decide this point. By this process we shall be able to ascertain, if the elements that have served as a foundation to the species do exist in a sufficient number of individuals, while their points of dissimilitude are not so numerous as to require other divisions. Thus, the methodical analysis, applied to the different cases of disease that have been denominated intermittent fever, will prove that they may be retained as a synthesis of a species. Simple, intermittent, bilious, gastric, cephalic, and malignant intermittents, should, from the identity of their fundamental elements, be referred to a common type. The same analysis, if applied to the varieties of gastro-enteritis, will show that this phrase should be retained to designate the synthesis of another species. To properly limit and circumscribe these two species of disease, the methodical analysis must be applied to all diseases that have been called intermittent fever and gastro-enteritis, and those retained that are really such, while those are rejected that present too many points of dissimilitude to permit them to be referred to these types, respectively.

Thus you see the synthetic process has been already, partially, applied to the study of disease. The science is, at present, in possession of several, not to say numerous, syntheses, some of which are the offspring of systematic, and others of eclectic medicine. It will not be difficult for eclectism to arrange into suitable orders these syntheses of species; and the day will come when, from a more thorough knowledge of the relations of diseases, these will be united, and then we shall have the great and comprehensive synthesis of the science. Some of these orders will doubtless be created from those apparently anomalous diseases that float, as it were, loosely and disjointedly, in the science, and which no foregoing class will include. There are facts, and in truth they are somewhat numerous, the elements of which have not been fully evolved. When this is done, their classification will not be difficult.

This being done, do you ask, will the science have reached the summit of perfection? I answer you, no. It will still be destitute of a general synthetic unity, from which will spring, in succession, other syntheses, like branches from a common trunk, or like the links of a chain which are in succession suspended from a primitive link. This great synthesis can have no existence, until after all the species have been properly arranged; and species cannot possibly exist until after all the individuals have been analyzed and their characters understood. Botany did not, gratuitously, assume the great synthetic unity from which sprung the beautiful distribution of plants into natural families. It grouped together those individuals that presented the greatest number of features of resemblance, and, afterwards, in accordance with certain systematic divisions and traditional determinations, new sections were established to embrace those plants which the first could not include. Finally, when the vegetable kingdom had been divided into as many departments as the striking differences of plants demanded, the great links which united the whole, were discovered.

When the science of medicine shall have reached this point, do you ask again, will it be complete? I must still respond in the negative. Were we to pause when we have advanced thus far, we should only have an experimental science. We should doubtless have a science positive and certain in its results, immutable in its character, and indestructible in its nature; but a science altogether insufficient to satisfy the restless and lofty aspirations of the human intellect. After we have analyzed and classified diseases; after we have, by advancing from generality to generality, attained the great synthetic unity, we must determine the laws which preside over their production and regulate their action. In a word, the philosophy of medicine must be deduced. It must be

confessed that if this transcendental part of medicine were once legitimately established, it would be, not only to experimental science, but to the art itself, of the greatest conceivable advantage. But the efforts that have been made for its improvement have been premature and misdirected. They have tended only to interrupt and embarrass the progress of the former, while they have seduced the latter from the only direct road that leads to truth. What other results could have been rationally anticipated? You will all admit that to explain, satisfactorily, even the simplest phenomenon, that phenomenon must be known; and, I think, we have already abundantly demonstrated to you that the efforts which have been made to acquire a full knowledge of diseases have been, in a great degree, unproductive. Being unknown in their elements, it was self-evidently impossible to explain them. The theories, doctrines, and systems of medicine, therefore have all wandered into vague and indefensible hypotheses, and their conclusions are little more than wild and lawless conjectures. Their rapid decline, their final and utter overthrow, prove that they were as baseless as the empty fabric of a vision.

If the systems, and the rabble of theories, hypotheses, and idle speculations have not been able to furnish rational and truthful views of the philosophy of medicine, do you ask me if eclecticism, acting under the auspices of the experimental method, will be able to do what they have failed to achieve? Will it be able to discover the pathogenic laws of diseases, to ferret out their nature, in a word, will it be able to explain them? That it will be fully competent to all this I do not hesitate to affirm. Nor can I, with safety, suggest any other plan for its accomplishment than that which I have shown you may be followed with so much success in acquiring a knowledge of disease. All the elements of pathological action being known, the value of each, in succession, must be determined, and this will enable us to form a correct estimate of them in the aggregate. This implies a previous knowledge of all the elements of disease, and this knowledge implies observation. This is the first step in the process, and so closely connected is it with interpretation, that it is with difficulty they are kept asunder. The latter is so completely dependent upon the former that the excellence or defects of the one enable us to judge of the excellence or the defects of the other. Observation, therefore, is indispensable to a truthful interpretation of disease. You have seen that the observation of the systems is partial and imperfect, while that of eclecticism is complete and comprehensive. You have seen that the results of the former are vague, uncertain, and disputable, while those of the latter are precise, correct, and unimpeachable. Inasmuch, therefore, as the observation of the systems is incomplete, their results uncertain, their interpretation must be indefensible; but as the observation of eclecticism is perfect, its results accurate, its interpretation must be true.

You thus perceive that it is the very essence of eclecticism to embrace disease, if I may use the language, in all its length, breadth, height, and depth; to follow it through all its numerous changes; to examine it under all its forms; and to study it under every possible aspect. Its object is not to know one or more of its elements, but the whole of them, and it therefore seeks them with assiduity and zeal. Their multiplicity, so far from frightening or discouraging, only serves to enkindle fresh ardour, and to stimulate to renewed enterprise. All the important elements must be fully developed, or its interpretation of them will be faulty and incomplete. To attempt an explanation, previously, would be to violate the laws it has established for its government, and to overthrow the structure which it has, with so much toil, endeavored to build. The philosophy of disease must be deduced from all its elements, as, otherwise, it would be, of necessity, imperfect, and consequently false. Thus it is that eclecticism unites all the possible chances of reaching the truth. Any thing else is alike unworthy the admiration or the respect of the enlightened physician.

This is precisely the course that has been pursued, with so much success, in the exact sciences. To illustrate and enforce the truth of what we have urged, permit us to select a very simple fact in physics. Suppose a ball to be thrown upon a plane, and after having rolled a certain distance, it stops. How will the philosopher explain this phenomenon? He will consider all that is peculiar to the ball put in motion, as well as all the surrounding circumstances supposed to have exerted an influence upon it. He will consider the degree of force imparted to the ball, as well as the properties of the ball itself, such as its rotundity, polish, and the matter of which it is composed. Then, passing to the external circumstances, he omits to notice neither the force of central attraction which constantly tends to strike it with inertia, nor the resistance of the surrounding medium. He examines the disposition of the plane to determine whether it is horizontal

or inclined, and he observes, with care, the asperities of its surface as well as those of the ball. These circumstances are all elements of the phenomenon, and without assigning to each its due weight and importance, it would be impossible to furnish a satisfactory solution of it. What would you say of the man who should substitute for this full and comprehensive method a partial and imperfect systematic method, and should attempt to explain the phenomenon by referring it, exclusively, to the impulsive force? Of him who should overlook the impulsive force altogether and ascribe it to the conjoint agency of central attraction and the particular disposition of the plane? Of him who, regardless of all the surrounding circumstances, should attribute it to the matter and properties of the body put in motion? Of him, finally, who should undertake to explain the phenomenon by the consideration of any one of its elements, when it is evident that it can only result from the accurate appreciation of the whole of them? This would be too absurd, and yet it is exactly the course which has been pursued by every system that has ever prevailed. It is the course that is still pursued by thousands, who fancy they are rapidly advancing the science. What are the systems of medicine but explanations deduced from the observation of one, or, at most, a few of the elements of disease?

The humoralist explains all the phenomena of disease by referring them exclusively to its cause. The numerous symptoms, however diversified, are so many translations, or versions of this cause. Introduced into the economy, he sees it deposited in the tissues through the signs of local reaction. Circulating with the blood, he sees it pervading every part in those of general reaction. When the process of concoction is completed, he sees it escape from the economy in the critical discharges. This morbid principle is multiplied ad infinitum. It assumes all forms, and explains all the phenomena. Organic lesions are considered effects, and the numerous sympathies the result of its immediate power. We do not maintain that all this is preposterous. On the contrary, there is much truth in it; but it is most incredibly exaggerated. As a system it is false, as is proved by the fact that the humoralist excludes the consideration of all those circumstances peculiar to the individual, all external influences, and the reactions of each particular part of the organism upon the general organism.

The vitalist explains disease by considering man as a system of independent forces. He believes him to be endowed with a peculiar energy that sustains him, though surrounded by agents that continually tend to his destruction. This is undeniable, for it has been deduced from facts that have a real indisputable existence. These facts have been generalized, and the existence of any other lesions or reactions than those of the vital force is denied. All causes of disease, as well as the whole apparatus of symptoms, are to the vitalist of the same signification. In every affection, it is the vital principle that is assailed by the morbid, and between them there is a constant struggle for victory. Every motion, every phenomenon, are so many efforts to repel the assaults of external agents, or to re-establish the disturbed equilibrium. Wherever the humoralist observes effects of the morbid molimen, the vitalist perceives only efforts of the *vis medicatrix natura*.

The physiologist pays no attention whatever to causality, but takes the anatomical character of disease as his point of departure. He thus groups around this character all the symptoms that can be explained by this principle, and excludes altogether those that revolt at so forced a reference. He denies all vitiation of the fluids, and the existence of an independent vital force. He will not admit primitive general symptoms, efforts of the *vis medicatrix natura*, nor crises. Should the crises, however, be too manifest to be overlooked, or disregarded, he refers them to the transference of irritation from one organ to another. External agents, and the modifications they impart to the excitable organism, are the only sources of his reasoning, while the organic lesions, and the sympathies, constitute his only means of explanation. Should, however, the organic lesions be absent, and this is often the case, notwithstanding the presence of symptoms of local reaction; or should the existence of the local lesions be indicated by general symptoms only, he immediately impeaches the competency of the observer; and if he finds this subterfuge useless, he will assert that our diagnosis is not sufficiently perfect to catch all the shades of the symptomatology.

Such are specimens of the kind of interpretation to be expected of systematic medicine. They are faulty, and, in an eminent degree, imperfect. As systematic observation, though partial and incomplete, has made known some facts that have an undeniable existence, so systematic interpretation has furnished some truths that cannot be impugned. From what has been already urged, are you not prepared to anticipate how

eclecticism will be able to make the partial explanations of the systems directly conducive to the permanent advancement of science? If not, I will tell you. It will only be requisite for eclecticism to make a rigid application to systematic interpretation of the same complete analytic process which you have seen may be applied to systematic observation with so much success. The facts of systematic observation, you have seen, may be arranged and classified by eclecticism, and the truths of systematic interpretation may, in the same way, be circumscribed to their particular and appropriate sphere of action. The interpretations of the systems are within certain limits true, because they have been deduced from unimpeachable premises. But as the facts were limited, their interpretation should be also limited. They have, however, been rendered false and untenable by being injudiciously extended to the explanation of facts that had not been previously examined. The functions of eclecticism will, therefore, consist in the circumscription of systematic interpretations to the facts from which they are legitimately deducible. Beyond this they should not be suffered to go. It is not sufficient to show that the explanations of humoralism, of vitalism, and of physiologism are true within certain bounds. This is not denied. It must be shown how eclecticism, availing itself of the materials furnished it by the systems, will be able to explain each particular disease; then each species; and, finally, arrive at the great systematic law that will explain them all. To accomplish this it will be sufficient to adopt the same process that was suggested when speaking of the classification of diseases. The laws that have been considered, by the systems, as general, must be applied to particular facts. They must be analyzed and not allowed to embrace a greater number of facts than they will satisfactorily explain. Should there be those, however, so refractory as not to submit to the laws of any known system, they should not be violently coerced, but allowed to await some other and more satisfactory solution.

Thus, you observe, the interpretations of eclecticism will be posterior, and not, like those of the systems, anterior to the observation of the facts. This circumstance will impart to the whole of those of the former the same degree of precision and certainty that belong to the partial interpretations of the latter. Deduced from data so comprehensive, instead of being subject, like those of the systems, to fluctuation and change from the observation of new facts, their truth will derive from them additional confirmation.

When the truths that have been brought to light by the labors of our predecessors shall have been properly circumscribed, and their appropriate sphere of operation in the science assigned, we shall not be obliged, at each epoch, to review the past; to point out the errors and absurdities, and the perishable nature of the efforts of our predecessors; but what has been done will be sustained by the judgment and good sense of each succeeding generation. A foundation will have been laid, upon which to stand, and upon which to raise the noble superstructure of science. This structure may be enriched and adorned by the labors of coming generations, but it never can again be destroyed. It will remain a monument, to the admiration of posterity, as enduring and imperishable as the science itself.

In contrasting the claims of eclecticism with those of the systems perhaps you may be inclined to suppose that it is my purpose to pronounce a sentence of condemnation on all future systems. If so, you do me great injustice. Such arrogance and presumption I would repudiate with yourselves. Advised of the causes that have led to the adoption of systems absurd and preposterous, and admonished by the evils they have entailed upon mankind, it has been my object to suggest the means of constructing a system that shall rest, for its foundation, upon the full, thorough, and methodical preliminary observation of all the facts. The day will come, though it may be still far distant, when some man will arise, inspired with the genius of Newton, who will discover the great systematic truth that will embrace and explain all the individual truths of the science. Before the great discovery of Newton, astronomical observation had established laws, formulæ of calculations, and species of particular truths, which only awaited the revelation of a truth that would include and explain them all. When Newton had proclaimed this truth, why did the world stand in wonder and amazement? Why was it considered beautiful, admirable, and unimpeachable? Only because it confirmed the admirable calculations of Kepler; only because it found, in the truths of observation that had been announced by Kepler, its full and entire confirmation.

My task is now performed, and, in concluding, let me express the earnest hope that, disenchanted of particular theories, unseduced by the glittering blandishments of system,

you will go forward, with me, in the footsteps of the great apostle of education, which I have endeavoured to point out to you, and do for this beautiful and magnificent country what he, with his disciples, are so nobly achieving for France. Your profession is in the hands of the Philistines; they have crushed its energies; they have blasted its prospects; they have covered it with disgrace; and it is now overspread by the dark and dreary night of desolation. Pause not till you have rescued it from their unhallowed keeping; rest not till you have accomplished its regeneration; sleep not till you have redeemed it from under the yoke of ignominious bondage!

We have been cut off from intercourse with the profession abroad. We have been sent into exile, and there has been erected in the "far west" a medical empire, based upon principles so novel as to be without a parallel on earth; principles, which in their practical bearings, lead to consequences so disastrous in their character that he who can look upon them without feeling his bosom bursting with the most painful emotions, must be dead to the voice of censure and callous to the accents of applause. Between us and our tramontane brethren there is not, nor can there be any community of principle, precept, or practice, so long as we remain under the dominion of that system which now, and has, for years, too triumphantly reigned throughout this valley. While they consider us the dupes of an absurd and dangerous delusion, we are the objects of their merited scorn and withering contempt. Nor is this all. Even their little children are taught to look upon a *Western Doctor* not as a safe, rational, and enlightened man, but as a monster in human shape, waving aloft in stupid triumph the terrific banner of Azrael. Does a family emigrate to our country? They are told to eschew a physician who has been educated in the west as they would the pestiferous effluvia of a charnel house. Shall this be? Shall the phrase "*Western Physician*" still continue the mortifying synonym of "*licensed murderer*?" Must this complimentary cognomen still remain attached to your names? No, I cannot, I will not believe it. You will rise in the majesty of strength—boldly breast the torrent, and roll back to its source the dark deluging flood of desolation.