

**Elements of medical logick : illustrated by practical proofs and examples ; including a statement of the evidence respecting the contagious nature of the yellow fever / by Sir Gilbert Blane. --.**

**Contributors**

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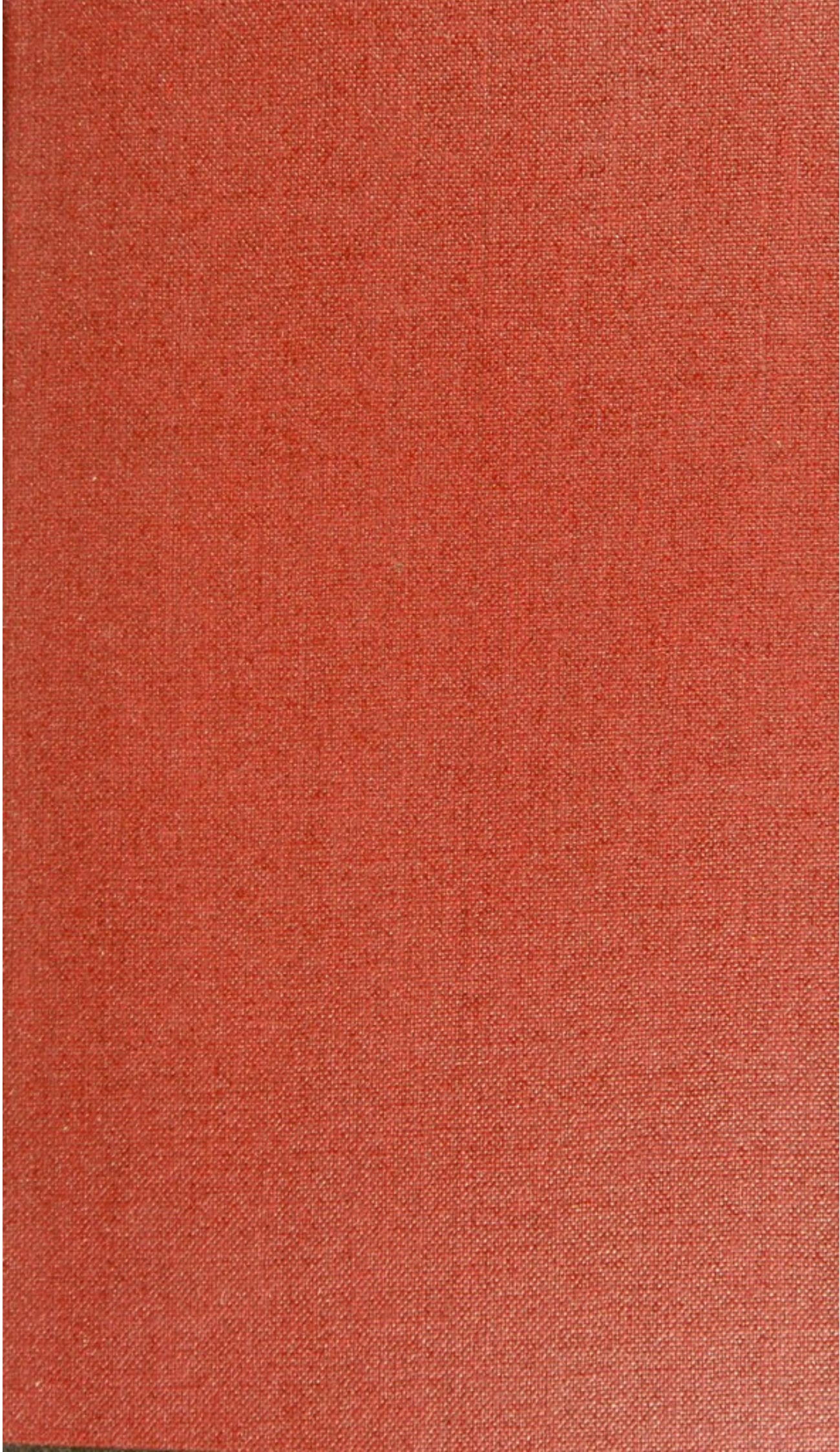
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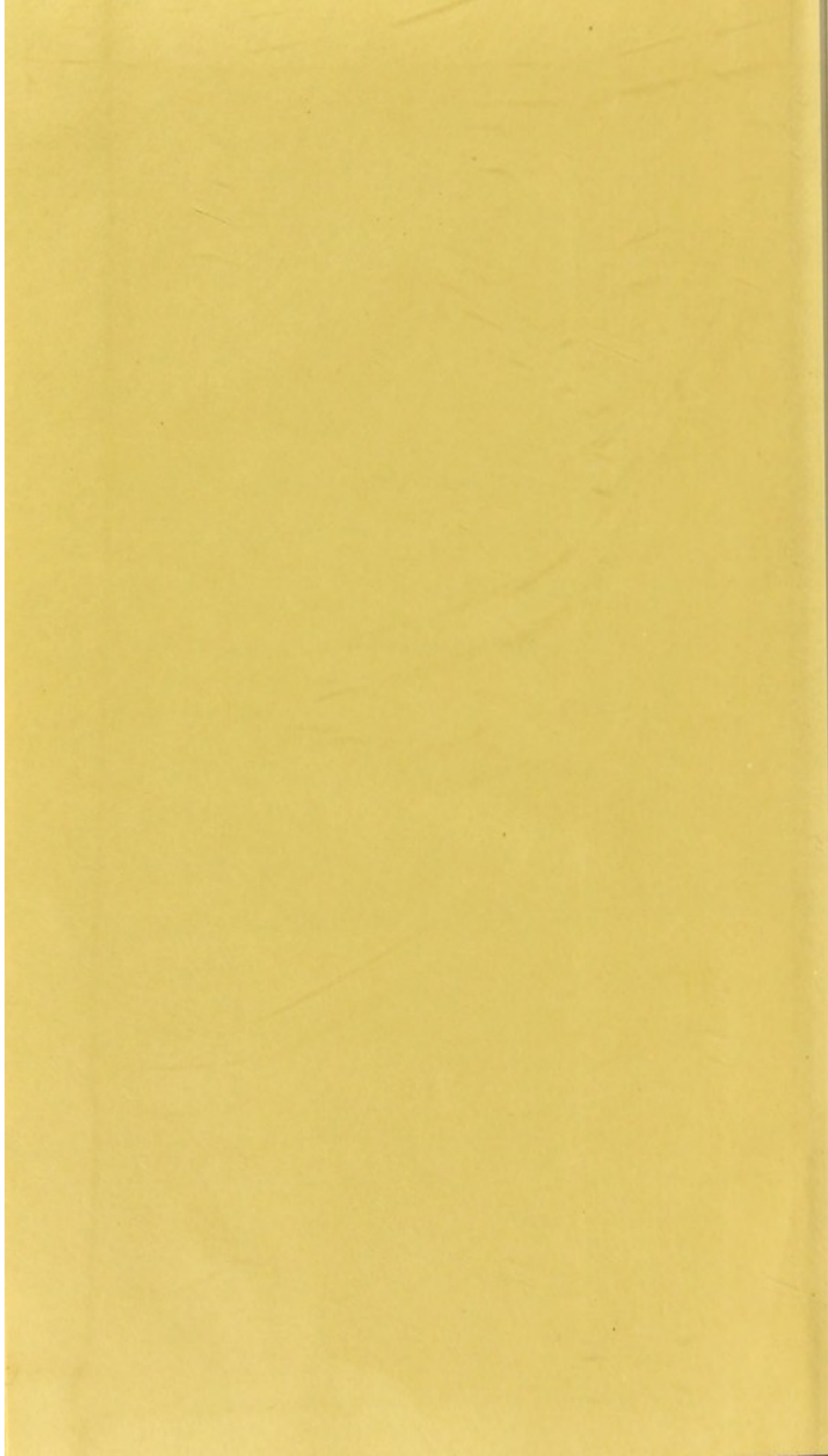
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**ELEMENTS**

OF

**MEDICAL LOGICK,**

ILLUSTRATED BY

**PRACTICAL PROOFS AND EXAMPLES;**

INCLUDING A STATEMENT OF THE EVIDENCE RESPECTING THE  
CONTAGIOUS NATURE OF THE YELLOW-FEVER.

~~~~~  
BY SIR GILBERT BLANE, BART. *MED*

FELLOW OF THE ROYAL SOCIETIES OF LONDON, EDINBURGH,  
AND GOTTINGEN, MEMBER OF THE IMPERIAL ACADEMY  
OF SCIENCES OF ST. PETERSBURGH, AND PHYSICIAN  
TO THE PRINCE REGENT.

~~~~~  
**London :**

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ELEMENTS

OF

MEDICAL LOGIC

ILLUSTRATED BY

PRACTICAL PROOFS AND EXAMPLES;

IN FORMING A STATEMENT OF THE EVIDENCE RESPECTING THE  
CONTINUITY OF THE LIFE OF THE

BY SIR CHARLES BLAKE, BART.

MEMBER OF THE ROYAL SOCIETY OF LONDON, F.R.S.  
AND OF THE SOCIETY OF MEDICAL JURISTS, LONDON.  
OF THE MEDICAL JURISPRUDENCE, AND OF THE  
TO THE MEDICAL JURISPRUDENCE.

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TO

SIR WALTER FARQUHAR, BART.

PHYSICIAN TO THE PRINCE REGENT,

AND

DUGALD STEWART, Esq.

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IN TESTIMONY OF THE

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**ELEMENTS**  
**OF**  
**MEDICAL LOGICK,**  
**&c.**

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**INTRODUCTION.**

As medicine has for its object the preservation and restoration of health, it comes under the definition of an ART, a term, the import of which consists in the adaptation of means to ends. These means must be derived from the previous knowledge of the changes producible by them, whether as corporeal agents constituting physical causes, or as affections of the mind constituting moral causes.

The most precise criterion that can be fixed upon for distinguishing rational beings



from brutes, is the faculty of adapting means to ends, and there is perhaps no operation to which the term *reason* is so appropriately applicable. To contemplate an end, and to devise and apply the means of attaining it, is a process strictly mental, and an attribute which does not belong to mere animal nature. Language has very commonly been assigned as the distinctive character of rational nature ; but it requires little reflexion to perceive, that, under this definition of art, language itself is an art, for it consists in the contrivance and employment of the *means*, articulate sounds, for accomplishing the *end*, the interchange of thought between one intellectual being and another ; and the logical process of thought by which children first catch the import of words, is an example of the same sort of induction, by which cause and effect are ascertained. The same principle explains the improvable capacities of man as contrasted with brute animals.

It follows from this, that as it is physical influences with which we have chiefly to do in medicine, the main and ultimate object



in cultivating this art, must consist in ascertaining the agency of external objects, whether salutary or noxious, on the living body, and in applying or avoiding them so as to obtain the desired result, either of preventing the occurrence of disease, or in converting the state of disease into that of health. It is in the extent and correctness of our knowledge of these agencies, that the perfection of the art of physick must consist.

This knowledge has to some persons of a sceptical turn of mind, appeared so unattainable, as not to be worth prosecuting, and they raise the previous question, *an datur ars medicinæ?* They allege that the powers and resources of nature in the human, as in the brute creation, are all-sufficient; that we do not possess such a power over the agencies of nature, nor such a knowledge of their application, as to constitute an art; that the history of this pretended art in all ages, so teems with the fanciful influence of superstitious observances, the imaginary virtues of medicines, with nugatory, delusive, inefficient and ca-



pricious practices, fallacious and sophistical reasonings, as to render it little more than a chaos of error, a tissue of deceit unworthy of admission among the useful arts and liberal pursuits of man.

As these allegations strike not only at the dignity and importance, but at the very existence of the art of physick, an answer to them seems to be called for.

The argument from the brute creation seems plausible enough, and it might have been added, that whole tribes of the human race pass through life without the benefit of any regular art of physick. But though animals are very little subject to disease compared to the human species, it is not strictly true that they use none, for they are observed to swallow certain simples to relieve themselves from disorders. Neither are the untutored tribes of mankind without their valuable remedies discovered by natural sagacity, and some of their greatest sources of misery consist in their having so scanty a stock of remedies, and the want of those means of relief when under suffering and



danger, known and practised in civilized life. It is also remarkable, that savages seldom attain to old age, though their disorders are much fewer, and the mechanical injuries to which they are liable, are much less frequent than among nations cultivating the arts and sciences; and it is incontrovertible, that if the powers of unassisted nature were all-sufficient, whether in the rude or civilized state of society, all mankind, or a very large majority of them, would die of old age, which is far enough from being matter of fact.

The maladies and casualties incident to the human species are more numerous and complicated than those of other animals, in consequence of the artificial habits and practices peculiar to rational beings, and they are still more multiplied by that superior cultivation of reason which distinguishes civilized from savage life. The exercise of reason has also a tendency to obliterate, or at least to weaken such suggestions of instinct as animals possess, and those dictates of natural sagacity, such as are found among savages, which nature has implanted for



the protection of both. From these considerations, it is obviously in accordance with the wise and beneficent arrangements of Providence in other departments of creation, that compensation should be made by reason's proving a corrector of evils which reason had induced, so that artificial ills should be counteracted by artificial remedies. This is presumable *a priori* from the analogy of nature; but it is not only presumable, but incontestably true in fact, that most diseases are more or less under the controul of art; and one could be named of such potent malignity as would have gone far towards the extinction of the human race, by striking at the very source of life, had it not been resisted by the power of art. This example is drawn from curative medicine, but instances equally important and convincing might be quoted from the history of preventive medicine, in the various means which have been devised and practised for obviating and extinguishing infections, engendered by the artificial habits of human life, in the more or less cultivated stages of society.



The unbounded number and variety of powerful agents under the command of art, by which the living human frame can be acted upon, afford further ground for admitting that physick is a real and efficient art. These consist not only in the regulation of diet, of the temperature and purity of the air, and the abstraction of blood, but there is such an ample and beneficent provision made by nature in the productions of the vegetable, mineral, and animal kingdoms, so diversified in their peculiar and respective virtues, as to be co-ordinate with a parallel diversity of human suffering, so as to leave no hesitation in interpreting their meaning, as a portion of that system of final causes, so expressively displayed in the whole works of creation. Is there an organ, or function in the animal economy, which cannot be either incited, or restrained by some natural agent discovered by man; and is there a malady, which, even in the present imperfect state of the medical art, does not admit either of cure, or palliation by some specific remedy, or mode of treatment already found out? It is also highly worthy of remark in this place, that as the diseases



and casualties of life are multiplied in the progress of civilization, so are the remedies multiplied by the improved energies of reason, in conformity to that scheme of consistency and harmony which pervades the universe, particularly in all that relates to organic beings.

Would it not also be an anomaly and blot in the beneficent adjustments of the creation, that human beings, under the extremity of suffering, should, by the impulse of nature, call aloud for relief, without any means being afforded for furnishing it? These calls for relief are indeed so imperious, that inefficient and even pernicious means will be caught at, if those that are efficient and salutary, cannot be procured; so that the dearest and tenderest interests of mankind would be left at the mercy of ignorance, selfishness and fraud, unless society were protected by the light of experience and knowledge. It is too true, that medical practice has been perverted by fallacious reasoning, and by the misapplication of the powerful resources discovered by superior intelligence; and it has been sarcastically



said, that there is a wide difference between a good physician and a bad one, but a small difference between a good physician and no physician at all ; by which it is meant to insinuate, that the mischievous officiousness of art does commonly more than counterbalance any benefit derivable from it. This view of the matter takes it for granted, that there is a greater risk of nature being thwarted by professional interference, than by the sick being left in the hands of the uninstructed. But is the uninstructed person more likely to humour or imitate nature, than the educated one ? From all I have been able to observe, the former is more apt to counteract nature by pernicious interference with her, than the wildest professional theorist ; nay, ten to one he is the greatest theorist of the two, for every old woman has her theory generally drawn from the humoral pathology. The faculty of clearly comprehending and fairly interpreting the ways and aims of nature, is one of the highest efforts of reason, and is attainable only by attentive study, and a happy turn for observation. If it were further necessary seriously to repel by argument this



depreciating sarcasm, it might be urged, that no argument is more weak than that which avails itself of *abuse*; for there is no maxim more true, and few more practically important, than that the best things are the most liable to abuse: *Nil prodest quod non potest lædere idem.* And the powers with which medicine is armed, forms no exception to this. It is not meant to deny, that great mischief ~~does not~~ occasionally arise from the unseasonable interference of art with the operation of nature, but as this consists in abuse, it affords an additional reason for the more intense cultivation of the art, in order to save mankind from the misapplication of it. And this is more particularly called for in the present state of natural knowledge, for the many new and potent tools which have been put into the hands of the profession, by chemical and botanical research, may prove swords and firebrands in the hands of the ill-educated and unskilful.

To conclude, it is the really enlightened physician alone, who can discern, in each particular case, to what extent art is avail-



ing, or if it is at all availing. But at all events he is bound not to desert his patient, and while he himself forbears from doing harm, it is his duty to protect those who are so eminently exposed to the over-active officiousness of others. He will also (if I mistake not) feel it incumbent on him in such moments, to administer the moral remedies of consolation and sympathy, and to assuage the anguish of despair, the most acute of all mental sufferings, ~~unless~~<sup>if</sup> we except the pangs of remorse. Those who conceive the whole art of medicine to consist in wielding the powers of the *Materia Medica*, entertain a narrow and unworthy conception of their own duty, and of the value and dignity of their profession.

The scepticism which has here been combated is a disease of the mind, which, like some of those of the body, is the offspring of over-refinement. But the great mass of error with which medicine has been encumbered, or as it were overlaid, has been engendered by an opposite malady of the mind, credulity. This has generally been held to be the peculiar reproach of rude



ages, and of the vulgar and illiterate in more refined communities; but if we are to include under it, the hasty adoption of crude and fallacious theories, derived from real or pretended principles of science, it will be found, that in the history of physick, there is equal room for the imputation of credulity in the learned and cultivated ages, as in the rude and illiterate.

It seems evident from all this, that physick being an art beset with every species of fallacy, it is of the utmost importance that those who engage in it, should be fully aware of this, and that they should so discipline their minds, by a knowledge of the laws of evidence, and the rules of investigation, as not to fall into either of the extremes of credulity or scepticism, to both of which the human mind, in different circumstances, is so prone.

It is the author's intention, with unfeigned diffidence and humility, to endeavour to point out, in what medical truth consists; what are the difficulties that have obstructed its progress, and what the means of obviat-



ing them; in other words (if he may be allowed to adopt professional technology) to expound the physiology, pathology, and therapeutics of the medical mind.





## SECTION I.

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THE sound state of the mind in physick, as in all the other practical pursuits of life, must consist in conceiving clearly and correctly, the reciprocal relations of cause and effect; for it is upon such knowledge alone, that the adaptation of means to ends in which we have defined art to consist, can be founded; for it is by the ascertained agencies of nature, and the just application of such as we can command, constituting skill and judgment, that the cure and relief of disease, like all the other purposes of human life, are brought about.

These agencies are ascertained by observation and experiment: by the former we may be said to listen to nature, by the latter to interrogate her.



Every reflecting mind must be struck with the admirable correspondence of the structure of the living body as a whole, and of the senses and functions in detail, in relation to external nature, such as the adaptation of the whole frame to the laws of gravitation, and of the eye and ear to the properties of light and air.

In looking still deeper, we perceive a like relation subsisting between the constitution of the mind and the laws of nature. The most essential attribute of these laws, is the *constancy* of their operation, as exemplified in the accuracy and precision of the revolution of the heavenly bodies, the unvaried universal and incessant action of gravitation, and every other circumstance, constituting what is meant by the course of nature in the inanimate world. Now the human mind has as evident a relation to this constancy of the laws of nature, as the senses have to their respective elements; for from the earliest period of life, there is, previous to all experience, a most unbounded confidence in the present and future constancy of events, manifested in all the actions and



attainments of practical life. The belief that the sun will continue to rise every morning; that all bodies will continue to gravitate to the earth; that the human beings around us exist, feel, and think as we do, may be quoted as examples of this untaught knowledge.

But this is not all, nor the most important co-incidence of the frame of the mind with the established course of nature. In all the effects, produced by the action of external bodies on each other, and on our own bodies, there is a rapid and instinctive connexion established between cause and effect, in virtue of that part of the structure of the mind by which it is made susceptible of habit and association, particularly in early life. This may literally, and without a figure, be termed the mental organ; for it corresponds to the constancy of nature, just as the eye or ear corresponds to light and air; so that not only every organ and function of the body, but every faculty of the mind, is co-relative with, or represents and reflects as it were, the elements and laws of



universal nature \*. The final cause of all this is evident, for if these rudiments of knowledge had not been thus rapidly, spontaneously and intuitively acquired, but had depended on the slow processes of induction, the ends of self-preservation, and the acquisition of language, could not have been effected. Moreover, these confident expectations of the future, could never have been discovered by reasonings *a priori*; inasmuch as we know nothing of the tie which connects cause and effect; nor can we form any anticipations of future events, but from the past experience of what may be called simple *sequence*. We become assured of the reality of such agencies, by finding, that in imitating the sequences of nature, we can adapt means to ends, so as to bring about certain proposed results. It seems to be in this way that we acquire our first idea of POWER. And those arts, which

\* See this sentiment more fully illustrated in a Lecture on Muscular Motion, read before the Royal Society by Gilbert Blane, M.D. page 40. Lond. 1789. It is also most ingeniously and appositely alluded to in Madame De Stael's account of the German poetry in her work, entitled, *De l'Allemagne*. Page 334, Volume 1st. Paris, 1815.



were before matter of blind instinct, become acts of reason, as man advances to mature and adult age\*. But the same physical constitution of mind, which in infancy, and very early youth, led us instinctively to ascribe causation to the connected events of nature, and which established our habits and associations, becomes in the same manner the parent of error, by ascribing causation to any events closely succeeding each other, whether casually, or by being common effects of the same cause. This

\* This train of reasoning is taken from a discourse read by the author before a literary society at Edinburgh (The Speculative) in the year 1771, in which he endeavoured to refute Mr. Hume's doctrines, particularly that of custom being the only source of our ideas of cause and effect. And he has been in the habit of meditating on such subjects, during the course of a pretty long life, in the intervals of his active pursuits and duties. In the course of this discussion, the reader may possibly meet with some original remarks; but what they are, the author himself cannot point out. Observations have occurred to him, which have seemed at the moment new to him, but which he found either to have been obscure reminiscences of his own, or derived from the stores of others, particularly Bacon, Locke, Reid, but, above all, Professor Dugald Stewart, the most profound Metaphysician, as well as one of the most elegant writers of this age. *!! profounds! the characteristic of*

*Dugald Stewart. Philosophy is any thing but profundity.*



it is which has given rise to those mischievous errors and inveterate prejudices ; those numberless fallacies, those nugatory and superstitious practices with which the history of the world abounds ; and which have proved sources of vice and misery, embittering and deforming human life and conduct.

It is the great business of cultivated reason, that is, of genuine philosophy, to unravel (a literal translation of the Greek word to analyse) and tear asunder those fallacious and imaginary catenations of cause and effect, from those that are correctly referable to the physical and moral agencies of nature : and in this consists the process of inductive reasoning, the only clue to useful and available truth.

In prosecuting researches into the several departments of nature, there is a great difference in the degree of precision and certainty, of which they are respectively susceptible, and in the degree of difficulty in obtaining satisfactory results. It is evident, that those departments, in which the subjects of enquiry are most simple and in-



variable, will be the most easy of investigation ; for they will be the least embarrassed by complicated and collateral influences. Mechanics and chemistry may be adduced as examples of this. In mechanics, the effects of gravitation, cohesion, impulse, pressure, and friction, are subjects admitting of precise calculation ; and in chemistry, the affinities are so constant and exact, that a single experiment is sufficient to establish a general truth. But the living body, besides it's being endowed with all these properties of inanimate matter, possesses such a number of attributes peculiar to itself, and of such a various and fluctuating nature, as to put their influences and combinations beyond the reach of all calculation ; and present the most formidable and discouraging obstacles to those who may propose, *a priori*, to controul and predict its operations, whether in health or disease. But it is incumbent on those, who allege that there are obstacles to physiological investigations, seemingly so unsurmountable, to specify what they are.

The author, therefore, submits to the profession the following enumeration of the



properties peculiar to animated nature; meaning under it to describe all the ultimate facts, or primary elements, which form the groundwork of physiological and pathological science.

These energies may be arranged as follows:

1. The Generative.
2. The Conservative.
3. The Temperative.
4. The Assimilative.
5. The Formative.
6. The Restorative.
7. The Motive.
8. The Sensitive.
9. The Sympathetic.

This arrangement differs from any with which the author is acquainted, inasmuch as it is not founded on an enumeration of functions and organs, but on elements pervading and belonging to the whole animal system. It is meant to comprehend all the properties in which the essence of life consists, and which characterize and distinguish



it from inanimate matter on the one hand, and from moral and intellectual nature on the other. If the enumeration is complete, it ought to embrace all the principles of disease as well as health, which can be brought under consideration ; for if it includes the whole powers of life, all disease must consist in the disorder of one or more of them. In this case, it might be made the basis both for a scheme of nosological arrangement, and for a plan of physiological instruction. But this does not at all fall within the present scope of the author, whose intention it is merely to state and define these principles as matters of fact in the natural history of life, with a view to render the medical enquirer fully aware of the difficulties which he has to encounter, in deducing practical results from such complicated data.

1. The **GENERATIVE**. It will not be disputed, that this primary energy of nature belongs purely and peculiarly to animal and vegetable life. Being emphatically named the mystery of nature, and being now admitted, by all correct physiologists, to be inexplicable, it only re-



quires in this place to be barely enunciated. It may not, however, be without use, here to hold out as a beacon to those who may still be disposed to waste their time and labour, in attempting to overleap the stated boundaries of nature, the fruitless and absurd results they are likely to attain ; for what can be so extravagant and irrational, as that hypothesis, which professes to explain generation, by supposing an infinite involution of embryos : *Obscura obscurioribus*. The doctrine of that most respectable physiologist, Dr. Blumenbach, who refers generation and growth to what he calls the *formative nisus*, is perfectly consistent with reason ; inasmuch as it is to be considered rather as an exposition of facts than as a theory \*.

The morbid deviations of this energy of nature, consisting in mal-conformations, monstrosities, and extra-uterine fœtations taking place in the act of conception,

\* See D. I. F. Blumenbach de Nisu Formativo Göttingen, 1787 ; and Abhandlung über die Nutritionskraft. St. Petersburg, 1780.



can hardly be regarded as objects of practice.

2. The CONSERVATIVE. By this is meant that power by which the living body is prevented from running into putrefaction. According to the experiments of Dr. Alexander\*, the range of temperature most favourable to the putrefaction of dead animal matter, being between 86° and 100° Fahrenheit, includes the usual standard of animal heat. There must therefore be some powerful energy in life itself which counteracts this tendency to spontaneous decomposition. It was alleged by Dr. Alexander, and some of the other physiologists of that day, that putrefaction was averted by constant motion of the fluids and solids during life, together with perpetual removal of effete matter, and the fresh supplies from food. These are evidently quite inadequate to account for this striking phenomenon, and that there is an antiseptic power in life independent of motion, and the change of

\* See experimental Enquiry on the Causes of putrid Diseases, London, 1771.



matter is proved by the same principle of self-preservation being found in the quiescent state ; for instance, in impregnated eggs and torpid animals \*. It pervades also the living organic bodies of a lower order, as is manifest in vegetable substances, which though not endowed with the same degree of heat as animals, are yet in such a state of succulence, as would lead to immediate destruction, unless they were sustained by this principle. It was considered by Mr. Hunter as so important an element in the existence of all organized beings, that he deemed it the most essential constituent of what he

\* This subject was first set in that clear and interesting view which is due to it, by Mr. John Hunter. (See *Observations on certain Parts of the animal Economy*, Lond. 1786 ; also *Treatise on the Blood*, 1794.) These works are rare and valuable specimens of true inductive research, and for logical precision and vigorous originality of mind, have never been surpassed, nor perhaps equalled, in the history of Physiology ; and it is not easy to be accounted for in the present times, that neither the import of his doctrines is clearly understood, nor their importance duly appreciated. We meet with works on Physiology, some of them even professing to be complete systems, in which this fundamental law of life is not once adverted to !



called emphatically, the *Living Principle*. This principle maintains a constant and arduous struggle against the septic tendency incident to the matter in which it is inherent, and it is more or less equal to this struggle, according to the constitution of individuals, and the operation of morbid causes. The circumstances in which it shews itself in too low a state to maintain life, are in those local affections in which gangrene takes place, and in those affections of the system in which the whole constitution sinks under them, as in acute disorders, generally produced by a morbid poison, such as small pox, typhous fever, and plague. The propriety of calling these disorders putrid has been questioned, because they do not produce actual putridity before death. This is certainly true, for actual putridity is incompatible with life; but the rapidity with which the dead bodies of those who perish by those disorders, is so much greater than that of those in whom life had been extinguished by other diseases, or by external injury, that there can be no doubt, that the conservative principle had been maintaining, before death, an unequal



combat with the principle of spontaneous decomposition.

As there is a comparative deficiency of this conservative energy in some constitutions, so there is an exuberance of it in others. The proofs of this consist in the resistance which some individuals oppose to the causes of gangrene and putrid fevers, either by maintaining an exemption from them, or by restoring themselves more readily than others, when under their action. The superiority of this principle is probably also one of the main causes of longevity.

3. THE TEMPERATIVE. By this is meant that steady degree of heat with which all animals are endowed, and which, in the *mammalia*, is higher than that of the atmosphere in any climate or season in the ordinary course of nature. It is extremely uniform in the same species, and in man it is found at 98° Fahrenheit, with less deviation in different individuals, than in most other points of the animal economy. There is a considerable latitude in the variety of the stature and form of individual men,



and in the natural frequency of their pulse ; but hardly any in the \* healthy standard of their temperature. This seems a presumption of its being an essential constituent of life, and combined with the conservative energy, may be deemed the main basis, or *stamina* of simple vital existence.

Some physiologists have attempted to prove, that animal heat is generated and kept up by chemical causes, and that respiration is the function by which this effect is produced. It cannot be denied that animals are warm in proportion to their intercourse with the atmosphere through respiration, as is evident in the gradation of mammalia, amphibia, and fishes, so that breathing probably contributes in some manner to the supplying or exciting of heat. But on weighing the whole evidence, it seems clear, that the heat of the living body is chiefly generated by the vital energies. Whoever will consult the writings

\* See Essay on the Thermometer, by Dr. George Martin, p. 145, Edinb. 1792.



of Cullen \*, of Hunter, of Currie †, and Brodie ‡, and above all, the experiments on a heated room, drawn up by Dr. Blagden §, can hardly fail to be convinced of this. But what appears decisive on this question, is, that the living system could not exercise the faculty of maintaining an uniform temperature in itself, in spite of the external impressions of temperature, whether exceeding or falling under that of 98°, unless it possessed the power of generating both heat and cold. Finally, the effect of the passions in generating both heat and cold, is suffi-

\* The scientific world is much indebted to Dr. Cullen for some of the most important doctrines on the subject of heat. His experiments on the cooling power of evaporation, published in the *Physical and Literary Essays of Edinburgh*, 1755, is ingenious and original; and he was the first who suggested and illustrated the power of the living principle in producing heat and cold. See an inaugural Dissertation of his Son, A. Cullen, *de Frigore*, 1780.

† See experiments annexed to a work, entitled *Medical Reports on the Effects of Water, cold and warm, in Fevers*, by J. Currie, M.D. Liverpool, 1797.

‡ See Croonian Lecture in the *Philosophical Transactions*, 1811.

§ See *Philosophical Transactions*, 1775.



cient proof that they do not depend on chemical causes.

The experiments of Dr. Crawford, in proof of the chemical origin of animal heat, are generally admitted to have been of so delicate and fallible a nature, as not to afford a solid ground for deciding this question; not to mention the assumption of the exploded principle of phlogiston which enters into his reasonings. The basis of this theory consists in the supposed extrication of sensible heat from oxygen, which possesses an extraordinary capacity for heat, and parts with a large portion of it upon entering into combination with other bodies. Though some heat may be produced in this way, it is quite inadequate to account for the quantity necessary for steadily maintaining, and equally distributing it through the body, which is chiefly done by the action of the vascular system; and it will still less account for that power, by which both heat and cold are resisted. But though oxygen may contribute but little to the generation of heat, it may act as a stimulus to the living power in generating it; for it plays



an interesting and active part as an exciting power throughout all nature, both animate and inanimate, being a main constituent in water and atmospheric air, and indispensable to combustion; and no animal can exist without more or less of its influence, whether by respiration or otherwise. The same may be said of vegetable life; and Mr. Hunter \* has proved, that this resists the action of cold in the same manner as animal life. A curious fact in proof of this last observation, is recorded in the American Medical and Philosophical Register, for the Year 1814, p. 19, where it is related by Dr. Brown, of Lexington, that ice was thawed by the roots of wheat which had shot into it.

The author has deemed it necessary to enter into these details, in order to justify himself in inserting the temperative energy in the list of powers peculiar to life.

It would appear that the due vigour, and alacrity of the mind and body, are main-

\* See Philosophical Transactions, 1768.



tained by a given degree of the exertion of the generating power of heat. The most salutary point of it, is that which is excited by the atmospheric heat of about 62° Fahr. As the heat of the living body, with very rare exceptions, is constantly above the natural heat of the atmosphere, the generating power in order to maintain this pitch, must be in perpetual requisition, and excitable in various degrees, in order to supply the consumption of what is carried off by the fluctuating temperature of the external air, and the quantity and quality of clothing. The requisite exertions of this animal energy, is necessarily a main element of health and disease, of pleasure and pain. This is a view of the subject, which has as yet but little attracted the attention of physiologists and pathologists. Is it not evident, that it is the sudden check which this energy meets with in the bodies of those who pass suddenly from a cold, or temperate climate, to the torrid zone, which produces those disturbances of the system, to which the natives, who are habituated to it, are strangers? And may not the sense of languor and debility from external heat, whe-



ther natural or artificial, which might, at first sight, be expected rather to stimulate, be owing to the generating power being repressed, and rendered more feeble in its exertions; as the invigorating effect of cold, within certain limits, may be accounted for, by this power being called upon for stronger exertion? This limit, as already stated, is about 62°. When cold is carried to an extreme, so as to master the resisting power, it comes to extinguish vital action, by extinguishing this fundamental principle of it, either destroying life, or producing local gangrene.

As the powers of art have considerable controul over this energetic principle, the means of inciting or repressing it, must of course constitute a large class among the resources of practice.

The excitability of Dr. John Brown, if the author understands it rightly, seems nearly the same as our principle of simple life, being expressive of the combined result of the conservative and temperative energies. The doctrines of this author led



to such destructive practice, that they are now generally and deservedly exploded. The argument of a *reductio ad absurdum*, may be legitimately employed in physical, as well as mathematical, reasoning; so that whenever a doctrine terminates in a conclusion, which is false in fact, we may rest assured, that it is itself false; and, as it is certainly false in fact, that depletory remedies are, almost in every case, that is, ninety-seven cases in one hundred, pernicious; and stimulating remedies, almost in every case, salutary, insomuch that it has been proposed by some of the partisans of this system, to destroy lancets, and throw away all purgative remedies, no further refutation need be sought for. The errors of this ingenious person, seem to have consisted in his having erected his system on the narrow foundation of only one of the principles of the animal economy, and in pushing that principle to an extreme. Had he referred a larger class of disease to over-excitement, which his system seems to admit of, his doctrines would have been more tenable, and would probably have lived longer. It



is doubtful, however, whether they would have spread so far, and been so enthusiastically embraced, for something strikingly new, and even absurd, seems indispensable for giving a popular prevalence to medical, as well as religious sects. There seems, nevertheless, to be in the doctrines of Brown, as far as their narrow principle will admit, and when received under a fair and temperate interpretation of their import and merits, some suggestions not undeserving the attention and imitation of a sober and candid practitioner.

4th. THE ASSIMILATIVE. This consists in the chemical changes brought about in the decompositions and combinations, effected by the power and processes peculiar to life; as manifested in digestion, secretion, and the preparation of the materials for the growth and repair of the body. When we consider the nature of the changes produced on the aliment in the living stomach, and the shortness of time in which it is effected, there will appear sufficient proof, even in this stage of assimilation,



that there are agents at work, totally different from those of inanimate matter \*. The familiar fact of the shortness of time in which the aliment becomes acid in depraved digestion, is also expressive of the singular powers of animal chemistry, a change being produced in a few hours, which, out of the body, could not be produced in twice as many days. But this difference becomes still more striking, when we contemplate the ultimate results of these processes, and that by virtue of the living powers, the aliment, whether vegetable, animal, or mixed, is converted into matter of the same chemical character, as existing in the flesh and bones of all quadrupeds. The flesh and bones, for instance, of an ox, a lion, and a hog, though differing in some of their sensible qualities, are identical, considered as chemical compounds, and exhibit changes totally different from, and utterly inimitable by any chemical processes, of which dead matter is susceptible. The new, important, and very interesting dis-

\* See Examination of Chyme, by Dr. Marcet, *Medico-Chirurgical Transactions*, Vol. VI. p. 626.



covery of the application of electricity, for effecting chemical changes, apparently bears some analogy to animal processes\*. The changes accomplished by the actions of life, may be conceived to be effected through the agency of some imponderable fluid; such as electricity, light, or magnetism. We can conceive, for instance, that each gland may be furnished with a sort of voltaic apparatus†, for effecting its specific change. That the accumulation and presence of such fluids, are not foreign to the animal functions, may be illustrated by the electrical battery of the torpedo, and electrical eel, the flashes of light from the eyes of some

\* The idea of the identity, or rather analogy, of the processes of the voltaic battery, with the processes of animal assimilation, was first broached in 1808, by Professor Brandis, of Kiel, in Holstein, in a work, entitled *Pathologie oder Lehre von den Affecten des lebendigen Organismus*, (See Hufeland's *Biblio-tek der practischen Heilkund*, 1809, Book I. p. 38, *et seq.*); and the next year, by Dr. Woolaston, in an article in *Tilloch's Magazine*. See also a paper in the *Philosophical Transactions*, by Sir E. Home, with experiments by Sir Humphrey Davy and Mr. W. T. Brande.

† See this illustrated farther in Dr. Young's *Medical Literature*, p. 110, Lond. 1813.



animals of the feline genus, and from the glow-worm and fire-fly. Some physiologists \* seem disposed to refer the assimilative process, to nervous influence; but the like assimilations take place in animals without nerves, and in vegetables: and the nerves seem in this, as in other cases, to act as stimulants and modifiers of action, and not to constitute the action itself: and when we farther reflect, that in inanimate matter there is a limited series of combinations and decompositions, whereas those of living organic beings present countless varieties; the qualities of the secretions and solid parts of all the species of animals and vegetables, being different, it must be confessed, that, although these newly discovered processes assist our conceptions, and abstract the imagination from the gross ideas of the old humoural pathology, they are quite inadequate to account for the transmutations of animal chemistry, and still leave a gulph between the actions of living and dead matter, which probably will never be passed.

\* See experiments by Mr. Brodie, *Phil. Trans.* 1814; and *Enquiry into the Laws of Life*, by Dr. Wilson Philip, London, 1818.



5th. **THE FORMATIVE.** This may be called also the organising or plastic. It has not usually been stated as a principle distinct from the last. The slightest reflexion however must evince, that it is quite a separate act of nature, and as different from the Assimilative, as the construction of an edifice is from the preparation and collection of its materials. Its operation also, is more immediately applicable and conducive to the rearing of the wonderful fabrick of the living body. It is stated by the ingenious and profound Blumenbach, as a continuance of the Generative energy. And it is certainly not less mysterious; for what can be more incomprehensible in the whole compass of nature, than the act by which that conversion and accretion of elementary particles is effected, by virtue of which, bone, cartilage, muscle, membrane, and every other form of organized animal substance is created, at the very point of time and space, in which this conversion and accretion is called for, and there and then moulded into the form of the respective organs which they constitute, and these organs precisely adapted to the ends of na-



ture ; and that these unceasing processes of growth and repair, should go on with such harmony on both sides of the body, as to produce that correspondence and symmetry which we behold ! This is a subject, the nature of which eludes the keenest research, and overwhelms the mind of man with astonishment and despair, from which it can find no refuge, but in resting on it as an ultimate fact, and referring the whole to supreme intelligence. Should any one attempt to scan it further, by ascending higher in the scale of natural causes, he will either find himself baffled, or will be in hazard of falling into some extravagance ; such as that of Van Helmont, who held that there was in living beings an intelligent principle, which he called Archæus, presiding over and directing the secret movements of the animal machine ; or of Stahl, who referred it to the rational soul. In that early stage of science, these were not unnatural notions to spring up, on a subject so dark and unfathomable, in ingenious and contemplative minds, endowed at the same time with a warm imagination ; and they are certainly not less venial, nor less abhorrent to reason,

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than the theory of the sun and planets, conceived by their cotemporary and countryman, Kepler, to whom the palm of high genius and intellectual excellence will not be denied. This very celebrated mathematician and astronomer, who first ascertained the ratio of the distance of the planets to their periodical times of revolution, conceived that the sun and planets were animated beings, that the rocks were to be considered as the bones of the earth, the seas and rivers as her blood, the metallic veins as abscesses, and that she performed her daily and yearly revolutionary journey in the ecliptic, by an act of her will, in concert with that of the sun.

The proper function of the Formative faculty, is growth and repair. The long and universally received mode of conceiving the progress of growth, was that of a constant accession of organic matter, giving additional length and breadth to the parts nourished. But it is evident, that this would render the preservation of shape utterly incompatible with the enlargement of dimension; and it was first clearly demonstrated by Mr. John



Hunter, that the only process by which the growth of solid parts, particularly bones, could be carried on, was by a constant removal and replacement of particles. The effete substances, the *ramenta* or *detritus* as it were of the living body, after being detached by absorption, and set afloat in the circulation, are conveyed through the mass of blood, and eliminated by the excretory glands and emunctory outlets\*, such as the

\* The fluid matter of the urine seems to be the effete part of the fluids, and the various species of solid matter found on evaporating it, the *detritus* of the various solid parts. The saliva seems to be the principal outlet of the *ramenta* of the bones for the phosphate of lime, of which bone is composed, is found on analyzing it, and concretions of it are found in the salivary ducts, or deposited on the teeth, in form of what is called tartar. The bile, and other intestinal secretions, are in part employed in the assimilation of the aliment, but a large part of them must be excrementitious, since they compose almost the whole of the fecal matter. Perspiration by the skin and lungs carries off that portion of the effete matter, which takes a gaseous form. If this were the place for indulging in pathological theory, might it not be plausibly maintained, that certain morbid phenomena, such as urinary concretions and cutaneous defecations, may be accounted for by an *error loci* of these excretions, the excrementitious matter of one outlet being determined to another.



kidnies, intestinal glands, the salivary glands, and exhalants.

The like operation goes on in the adult, as in the adolescent state; for the actions of life making an incessant consumption of the solids, as well as of the fluids, require an incessant repair by new supplies of aliment.

The operation of healing also, by the creation of new parts in cicatrization, and the formation of callus, for the repair of injuries, is an emanation of the same energy, by which these organs were originally formed and moulded. Inflammation itself, in so far as it is necessary for producing vascular texture, adhesions, and other new organic parts, is an act of the same energy, and is to be deemed morbid only when excessive, or misplaced. Tumours and exostosis may also be considered as exemplifications of the morbid excess of this energy; as emaciation, particularly that which leads to dissolution in old age, may be adduced as an example of its deficiency, and schirrosity and dropsy, as examples of the want of absorptive energy.



6. The RESTORATIVE. It is well remarked by Dr. Gregory \*, that the animal machine differs from all others in this, that it carries in itself the means of repairing the injuries and disorders incident to it. A species of restoration, consisting in the repair of solids and fluids, rendered necessary by their perpetual waste and depravation, has just been mentioned. This has reference to the support of the material fabrick of the body ; but what is meant here to be designated, consists of laws belonging, and indispensable, to the principle of life itself. The first is, "Nature's sweet restorer," SLEEP. That state of excitement of sensation, thought, and voluntary motion, by which all living beings subsist in their ordinary existence, cannot, by the constitution of nature, be maintained, without a daily suspension of several hours. As the author means in this disquisition, merely to state and enumerate, historically, the several peculiar properties of life, he will neither enter into any research into the nature of this attribute, nor into the disorders arising out of its excess or defect.

\* See *Conspectus Medecine Theoretica*, V, 1. p. 5.



He will only remark, that sleep being universal and indispensable, it is highly important, and one of the most prominent features in the natural history of life; and that being indispensable, nature, with her usual wisdom and kindness, has provided great powers of accommodation, suited to the emergencies of human life. With a view to this, it is observable, that the refreshment of sleep is not in the simple ratio of its duration, the principal share of this act of restoration being found to take place in the beginning of it. If a person be at any time deprived of one half, or more of his usual portion of it, the inconvenience experienced is by no means in proportion to this privation; and habit will bring persons, whose affairs require it, to subsist in health and vigour with a small allowance of sleep. General Pichegru\* informed me, in the course of my

\* The singular exertions and alertness of this leader of the revolutionary armies of France, need not excite surprise, when we reflect that he performed what I believe is not to be met with in the ancient or modern annals of Europe. In a climate, in which, during all former wars, it was deemed impracticable to carry on the operations of a campaign through the winter, he kept the field for two successive winters, that is, from the



professional attendance on him, that, in the career of his active campaigns, he had for a whole year, not more than one hour of sleep at an average in twenty-four hours. According to my own experience, I find, that when I have been called out of bed, after half an hour's sleep or less, I feel a very great difference in my feelings next day, from what I have felt when I have had no sleep at all. The powers of the *sensorium* seem to be wound up, as it were, at the most rapid rate in the first period of sleep; and great part of the refreshment in the later hours, seems more imputable to the simple repose of the organs, than to the peculiar influence of sleep. There are some persons,

time at which he stormed the lines of Weissemberg in the end of 1793, and drove the Duke of Brunswick across the Rhine, till he overran Holland in January and February 1795. He informed me, that in all that time his armies had no camp equipage, and that their only substitute for tents, was an occasional light hutting of turf and boughs which could be erected in a few hours. On enquiring respecting the health of his armies, he answered, that sickness never prevailed among them, except in a detachment which he sent against Sluys, in Dutch Flanders, which suffered severely by the intermitting fever incident to that marshy district.



to whom more or less sleep has become habitually necessary in the course of the day, particularly after dinner; and they find that a few minutes of it satisfy nature. But the most striking illustration of this principle, which I have met with, is what I learnt from a gentleman of great observation and intelligence, who had been long in China, and had an opportunity of seeing the habits of the Missionaries. These pious and conscientious persons felt themselves bound to abstract as little time as possible from their duties, and took the following method of abridging the period of that sleep which habit had made necessary to them, in the middle of the day. They threw themselves on a couch, with a brass ball in the hand, and under it a brass bason. The moment they dropped asleep, the ball dropped from their hand, and ringing on the bason, waked them. This they found afforded all the recruit which nature required.

There is reason to believe that sleep is necessary to the existence, not only of the lowest animals, but of vegetables. The temporary diurnal collapse in the leaves of



plants, is referred by Linnæus, and other naturalists, to sleep \*.

The other branch of the restorative principle here intended to be enumerated, is the spontaneous cure of diseases, or what is commonly understood by the *vis medicatrix naturæ*. The powers of nature, in combating and subduing disease, are so obvious and undeniable to the most cursory observation and reflection, as to need no proof. The benefit derivable to mankind at large from artificial remedies, is so limited, that if a spontaneous principle of restoration had not existed, the human species would long ago have been extinct. Death occurs from disease, such as those of a pestilential nature, only when the natural powers of recovery are inadequate. The main business of art consists in inciting or restraining these powers, or in removing obstacles to their due and fair action. There are many of the phenomena of disease, which consist in the struggles of this self-healing energy; and

\* See an article in the *Hamburgh Magazine* of 1759, by Professor Zinn, of Göttingen, entitled, *Von dem Schläfe der Planzen*.



it is one of the great difficulties of medical philosophy so to interpret nature, as to ascertain and determine what symptoms and sufferings are referable to positive disease, and what to the warfare of the restorative with the noxious principle. It might plausibly be maintained, for instance, that all the leading phenomena of fever, consisting in a regular series of movements, producing crisis and types, is the campaign which nature carries on in waging war against the hostile invasion of disease. This has been properly enough called the re-action of the system; but this re-action, though in its nature salutary, may exist, either in excess or defect. For instance, if one of the morbid poisons exciting fever, should assail life by attacking one of its fundamental principles, the generating power of heat, this principle may re-act with such violence, as to make it one of the main objects of practice to repress it, either by internal remedies, or by the external application of cold; and the converse of this will happen, should the re-action be too feeble. This is not to be considered as an adopted theory, but merely a matter of



hypothetical illustration. The subject will be more particularly adverted to, under another head.

7. The **MOTIVE**. By this is meant muscular action, in its most extensive sense. The motions taking place in the living animal body, for carrying on its various functions and actions, are strikingly distinguished from those of the external world, inasmuch as they are not referable to gravitation, chemical attraction, nor any of the other causes by which the particles of inanimate matter are put in motion. All the attempts that have been made to explain muscular motion, by referring it to any of these principles, have been grounded on gratuitous hypotheses, and have therefore terminated in vain and inconclusive speculations. It is stated here among the ultimate and inexplicable attribute of animal nature.

The living motions are manifested in the action of the voluntary and involuntary muscles, and the vascular system. The voluntary muscles are excited to action by the will: the involuntary muscular organs,



consisting chiefly of the heart and intestines, are excited by their contents, partly acting mechanically by the distention of their containing tubes and cavities, partly by the peculiar qualities of their contents acting as specific *stimuli*. The same may be said of the vascular system in all its ramifications; and tension is clearly indispensable to its action, and even to the existence of life itself, as is exemplified in the fatal effects of hæmorrhage. Indeed, without a certain degree of tension, no muscular fibre can have its natural play, except the sphincters, and the mouths of the exhalants.

The nature and degree of the dependance and relation subsisting between the nervous system and muscular organs, is one of the most important and difficult questions in physiology. That motions peculiar to life can exist without a nervous system, is proved by the existence of those simple animals which have no nerves, and still more clearly by the economy of vegetable life; for the actions of growth, the clasping of tendrils, and other phenomena, which here take place, are as remote from those of inanimate



matter, as those which take place in animal life. In the complex animals, the sensorium and system of nerves are never wanting, being indispensable, for the purposes of sensation and voluntary motion. That nervous power is not necessary for vascular action and circulation, is proved by there being none in the placenta. With regard to the influence of nervous energy on the involuntary motions in general, great light has been thrown on it by some recent experiments and researches, of which a complete and compendious account may be found in a work of Dr. Wilson Phillip \*, to which the reader is referred, this not being the place to dilate on this subject. Upon the whole, it appears that the susceptibility, in other words, the excitability or irritability of muscular organs, is not *constituted* † by nervous energy; but that this energy *incites, restrains, and regulates* these organs, *and connects* them

\* An experimental Enquiry into the Laws of vital Functions, &c. by A. P. Wilson Phillip, M.D. London, 1817.

† See Lecture on Muscular Motion, by G. Blane, M.D. p. 23. London, 1789. This Tract has been long out of print, but nearly the whole of it is transcribed into the Encyclopædia Britannica, under the Article Muscle.



with each other, and with the organs of sense, in carrying on the purposes of life.

There is a function, depending probably on the regulating influence of the nervous system on the motive organs of complex animals, which seems not to have been duly attended to by physiologists and pathologists. Is there not a tendency, not only of the common mass of circulating fluids, but of particular portions of them to particular organs, which ought to be stated as a function by itself, under the name of DETERMINATION? A thought in the mind, for instance, will suffuse the whole face and neck; and particular appetites will cause an increased determination to the organs of their gratification. There is a like partial and temporary afflux made on all the glands, in order to fulfil the purposes of nature. The secretory operation by which a total change is effected in the nature of the fluids which arrive at glands in the course of circulation, has already been adverted to. But there are some glands, such as the kidnies, which are destined for outlets of the effete and acrimonious portion of the fluids, to which these portions seem determined, by a selecting



and separating power peculiar to life. Another example might be taken from the determination of the chylous parts of the mass of fluids to the *mammæ*. Upon what other principle can we account for the rapid transmission of fluids from the stomach to certain glands? It is a common manner of speaking, to say, that the greater or less abundance of urine or milk is produced by the greater or less energy of the respective glandular organs. It seems clear, however, that no exertion of these organs could have this effect without the co-operation of the general circulating system, producing an afflux, not merely of the whole mass, but of selected portions of it. As well might a mill, put in action, go on to produce meal without a continual supply from the hopper. Is it not therefore worth the consideration of physiologists, whether determination should not be deemed and stated as a function *sui generis*, and the errors of this function will open a new path to pathologists for exploring the elements of disease, for a considerable proportion of morbid phenomena are referable to preternatural determination, and may be considered as a deviation from that



sound state, in which there is allotted by nature a given portion of circulating fluids and nervous energy to each organ.

The excess of muscular action, is chiefly manifested by those morbid contractions denominated convulsion and spasm in the muscular system, and by inflammation in the vascular; the defect of it by relaxation, as exemplified in debility, languid action, and other various forms of atony.

8. The SENSITIVE. Sensation, being a simple idea, does not admit of definition; but is readily understood by a reference to some of its descriptive attributes, such as pleasure and pain; and to the several perceptions conveyed through organs of sense, adapted for receiving the impression of their respective external objects. To these might be added consciousness and volition; but as these belong to man, in his character as a rational and moral being, they are foreign to the present subject, which professes to embrace only his animal nature. It belongs to this place, however, to advert to this aspect of the human character, in as far as



it exercises an influence over the corporeal frame through the passions and intellect ; and adds to that complexity of effect, which it is the purpose of this rapid sketch of the animal nature of man to elucidate.

It is deducible, from what has been said, that the conservative and temperative faculties are independent of the nervous system, being found to exist separately from it ; and it has been seen, that, though the assimilative, formative, and motive faculties are much influenced and guided by it, yet that they too may be said to have an existence independent of it, but the sensitive principle is of its very essence.

It has already been remarked, that all the senses bear a reference to the material world, each having a corresponding object in external nature, to which its structure is adapted. Were it not for the great familiarity of the subject, the adaptation of the eye and ear to the properties of light and air, would strike us with the most intense delight and astonishment. The same may be said of the other senses, and of the con-



formation of the hand and whole body in its stature, and the relative position of its limbs and viscera, as adapted to the laws of gravitation, and the mechanical properties of matter.

It has also been remarked, how essential it is that the human frame should be adapted to the constancy of nature. It is on this constancy that all our experience and judgments are founded, whether in operating upon matter, or in our intercourse with our fellow-creatures, the one having relation to that reliance which we have in the invariable course of nature in the physical world, and the other in the moral world. It follows, that the morbid state of the senses will consist not merely in simple excess and defect, constituting over acuteness or hebitude, in the corporeal frame, but in those false references in what may be called the rational and moral frame, in which depraved judgment and mania consist.

9. The SYMPATHETIC. This is the last in the enumeration of the faculties peculiar to life.



The term sympathy has been much objected to, but as I apprehend, rather fastidiously and unreasonably. It is like most other terms belonging to the science of life, figurative, being a metaphor taken from an affection of the mind. The import of words, according to the most correct and received rules of philology and rhetorick, is not at all to be deduced from derivation, but either to be assumed conventionally according to a definition, or to be adhered to in the sense affixed to it by established usage. By animal sympathy, is not meant the intelligent principle of Stahl's hypothesis, but that mutual influence of distant parts so subtle and rapid as to be compared to thought. If this term is to be rejected, some other must be invented to express what actually takes place in those operations of the living body, by which, without the transmission of ponderable matter, or the intervention of any of its properties, the most indispensable functions are carried on. The medium of this communication is probably some imponderable fluid; but it would here be out of place to discuss this, since we have only to do,



with a fact which we consider as belonging to an ultimate and inexplicable law of life.

It is through this energy, that all the preceding faculties act and re-act upon each other, in carrying on that harmonious play of the animal system, in which its sound state, and its perfection as a whole, consists. The most descriptive character of the healthy state, being the quietness and imperceptibility of the operation of the various functions and organs, this intercourse by sympathy is but little observable in health, and is only manifested in morbid actions, or by the operation of medicines. Every such action must therefore carry a reference to a corresponding action which is sound and habitual. The connexion of the stomach with the head, the heart, the surface of the body and the kidneys, and the reciprocal action of all the functions and organs with the skin, may be adduced as some of the most striking and important examples of sympathy \*.

\* See Observations on the Diseases of Seamen, by G. Blane, M.D. page 565, 3d Edit. London, 1798.



It becomes a question whether sympathy, in all cases, depends upon the nerves as its vehicle, for it is presumable that they form the medium of it in most cases. That species of sympathy, called by Mr. Hunter the contiguous, exemplified in the influence of the containing on the contained parts, as that of the integuments on the subjacent viscera, seems to have no relation or dependance on the distribution of nerves. And there are evident proofs of sympathy in vegetable life; for, not to mention the effect of the irritation of a single leaflet\* of the sensitive plant, in making the whole leaf and its foot-stalk contract, there is undeniable

\* My friend Dr. John Sims, so well known for his excellent taste and great knowledge of botany, was so kind as to communicate to me, the following interesting and hitherto unpublished experiment of his own. A leaflet of the sensitive plant being irritated, and the greatest pains being taken to avoid moving any other part of the leaf, the whole of it nevertheless contracted, and the foot-stalk dropped. But in order to be sure that mechanical motion, communicated by this irritation, had no share in this contraction, he threw a sun-beam concentrated by a glass lens on one of the leaflets: the whole leaf contracted, and the foot-stalk dropped.



proof of it in the excitement of the roots of trees, in sending up the sap in consequence of an influence from the trunk, branches, and leaves, on the return of the vernal warmth. The temperature of the earth, at eight feet deep, is lower in April when the sap is ascending, than in January, according to experiments related in the 59th Number of the Edinburgh Review, p. 6 and 7, in an article of which Professor Leslie is said to be the author. And if in the winter time the branch of a vine be introduced into a hot-house, it will produce a luxuriant crop of leaves and fruit, the materials of which could only be derived from the excitement of the roots propagated by sympathy: this will take place even during a frost. The action of the roots, therefore, must arise from what Mr. Knight, in treating on the same subject, calls “a vehicle of irritation, arising from  
“an intrinsic power of producing motion  
“in vegetable life \*.”

\* See a Series of ingenious Papers on Subjects of Vegetable Physiology, in the Phil. Trans. from 1801 to 1806, by T. Andrew Knight, Esq.



It is evident how much processes of health must be deranged from an excess, defect, or total suppression of the sympathetic faculty, and this opens a wide scope to the speculation of those, who search deeply into the proximate causes of disease, the operation of remedies, and the sources of error from the false reference of the seat of diseases.







## SECTION II.

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THE author has thus endeavoured to enumerate and define the primary elements, or ultimate facts, belonging exclusively to animated nature. In an attempt which is new, in a subject of which he has taken a view peculiar to himself, he does not dare to think that he has attained any thing like perfection. It is evident, however, that it is only by following out an analytical scheme of this kind, that a foundation can be laid for the genuine principles of theoretical medicine; for the elements of disease can only be expounded by a thorough knowledge of the elements of life and health. But it is not his intention now to apply it synthetically, by building upon it any system of nosology, physiology, or pathology; far less to bewilder himself, or his reader,



by agitating the question, whether life consists in the play of these principles, or if it is something superinduced on them. His sole object in this analysis, is to convey an adequate conception of the great difficulties which those have to encounter, who would found practical medicine on a knowledge of the animal economy, and to bespeak a liberal indulgence for the errors of those, who, in attempting this, have had to grope and wander in more dark and intricate mazes, than what has fallen to the lot of any class of enquirers into the various departments of nature. We have seen that the animal nature of man, besides being subject to the mechanical laws of inanimate matter, and to the influence of the passions, consists of a number of principles, which must all act in harmony in a state of health, and that the disorders will be multiplied according to the number of combinations of which these requisites of health admit. Is it not from physiologists not being aware of these difficulties, and from their not clearly perceiving that animal processes are quite inexplicable on mechanical and chemical principles, that the abuses and errors of theory



have been engendered and accumulated in this department of knowledge above any other, deforming and discrediting medicine as a science, and obstructing and retarding its advancement as an useful art?

Practical medicine seems more indebted to the sagacity of those, who, in a rude state of society, discovered active and useful medicines, than to the early labours of the learned. From what we know of Democritus, and his followers, of whom Hippocrates was the most eminent, it appears, that the cultivation of science in the early ages of Grecian philosophy, was undertaken on the soundest principles; namely, the observation of nature, and the collection of facts. Aristotle himself, was a most diligent observer of nature, and collector of facts; but unfortunately his logical and metaphysical writings, caught preferably the notice and taste of the learned world, and engrossed its attention for many ages, to the exclusion of all other useful and liberal knowledge. For more than a thousand years, the syllogistic logick of Aristotle usurped the place of all literary and scientific pur-



suits. In those dark times, however, it was better than no knowledge at all; and I am one of those who are of opinion, that this logick, though it affords little or no assistance in the discovery of practical truths, and the interpretation of nature, is yet an excellent discipline of the mind, tending to give precision to language and thought, more indeed in moral science and literature, than in the physical and exact sciences, and that some knowledge of it can hardly be dispensed with in a liberal education. I am thankful that it made part of the academical education which I received. The error has been in suffering that to obtain a predominant rank, which ought to hold a subordinate one, and in substituting the means for the end, that which was only one of the organs of knowledge, being exalted to the station of its main and almost sole constituent.

It is to Bacon that the world is chiefly indebted for dissipating the clouds of false philosophy, and for pointing out the road which led to solid learning, and the discovery of interesting truths. He himself, however, affords a proof how necessary it is,



that the mind should be subjected to a long course of discipline, in order to bring it into correct habits of thinking on matters, depending on that inductive principle of observation, in the recommending of which, his own chief merit consisted ; for there are in his own works evidences of his being infected with the credulity of the times in which he lived. He did not entirely disbelieve in the virtues of amulets and charms ; and when treating of the interpretation of Nature, he certainly fell into great error, in excluding the consideration of final causes, as one of the keys for unlocking her secrets \*.

\* Bacon, with that exuberance of imagination with which he was so richly gifted, and not without some savor of the quaintness of the age in which he lived, says, that final causes, like the vestal virgins, are devoted to the service of the Divinity ; but like them too, are unfruitful, the consideration of them not leading to scientific improvement and natural discoveries. Nothing is more certain, however, than that we are entirely indebted to the consideration of final causes for the discovery of the circulation of the blood, for it was a close attention to the use of the valves of the veins which led to it. Is it impossible, that the discouragement thrown out to the consideration of nature under this as-



Into errors of this kind, philosophers of this age, of a capacity infinitely inferior to Bacon, are in no danger of falling; for there is a certain maturity of the human mind, acquired from generation to generation in the mass, as there is in the different

pect in which Bacon perhaps was not singular, may have retarded this discovery? If the anatomists, who lived prior to this era, had contemplated the structure of the heart, with reference to final cause, they could hardly have failed to have made out the circulation. It is a principle fully recognized by modern anatomists and physiologists, as may be seen in Sir E. Home's late articles on the digestive organs in the *Phil. Trans.* and the Germans have invented a word *Zweckmassigkeit*, expressive of that property of organs by which they are adapted to their ends, which cannot be translated but by circumlocution, into any language with which I am acquainted. If a modern anatomist were to find in a new animal of uncommon structure, just brought from New Holland, a large muscular organ, the function of which was not obvious, he would not merely conjecture, but believe with the most confident certainty, that it performed some important function requiring powerful mechanical action, and would not rest till he found out to what purpose it was destined. A moment's serious reflection on the materials and structure of the heart, ought in like manner to have revealed the circulation to physiologists, many ages before it was detected by Harvey.



stages of life in the individual man, in which respect the ancients are the young, and we are the old, as is justly remarked by the author in question : so that he who in these times so clearly sees the defects of Bacon, and well knows how to improve and extend his views, may be compared to the dwarf on the giant's shoulders, who sees further than the giant himself. Hippocrates too, though one of the most chaste and accurate observers of nature, and collector of facts, was by no means exempt from that spirit of system, originating in the innate propensity of mankind, to assign causes, however lightly and hastily, manifesting itself in hypothetical and gratuitous assumptions of general principles. And there cannot be a more flagrant exemplification of this, than in this great man referring all diseases to excess, defect, or vitiation of the four humours, blood, phlegm, black bile, and yellow bile. We derive from this the benefit of an example, at least, serving as a beacon and safeguard against that premature generalization of facts, which, in our times, is considered as one of



the most sure criterions of an inferior capacity, or untutored mind.

After the revival of genuine philosophy in the 17th century, it might naturally be expected, that medical science would immediately avail itself of its light, and partake of its benefits; but this was so far from being the case, that, in the first instance, it proved a new source of error, and threw fresh impediments in the road, which was supposed to be thrown open to the improvement of rational medicine. The discovery of the circulation of the blood, may indeed be considered as one of the first fruits of the enquiries into nature begun in that age. But though this is a fundamental element in the economy of the living body, it throws little or no light on the principles peculiar to life, being purely of a mechanical nature; and abstractly considered, hardly admits of any application to the practice of medicine. On the contrary, by its perverted application, it tended to corrupt and mislead, by a loose adoption of the principles of mechanical philosophy, so well laid



down in that age, by Galileo, and others. Borelli, in investigating the force of the heart by experiment, estimated it at 180,000 pounds; Hales, at 51 pounds; Keil, at 1 pound. The mechanical powers of the stomach, were, about the same time, subjected to experimental research, by Pitcairn, who gravely gave out, that he found this viscus in the human subject, exerted a force equal to 12,900 pounds, in compressing food in the process of digestion. Others conceiving that chemical power had the chief share in this function, endeavoured to evince, that the change in the food was brought about by means of heat and fermentation. Sounder principles have referred these changes to powers, which have nothing in common with the mechanical and chemical powers which characterize inanimate nature \*.

\* Dr. William Hunter, whose peculiar sagacity and precision of mind, detected at a glance the hollowness of such delusive hypotheses, and saw the danger which theorists run in trusting themselves on such slippery ground, was heard by the author to express himself on this subject, in his publick lectures, with that solidity of judgment, combined with facetiousness, which ren-



But the most singular, the most celebrated, and I will add, *pace tanto viro*, the most mischievous abuse and misapplication of the principles of natural science to the animal economy, are to be found in the works of Dr. Boerhaave,

Towards the end of the 17th and beginning of the 18th century, physiologists had begun to perceive, that life was regulated by laws peculiar to itself, and that some other principles than those of mechanism and chemistry, ought to be resorted to in explaining the operations, whether of health or disease. Glisson and Willis, in England, Baglivi, in Italy, and Hoffman, in Germany, led the way in this reformation, and there was a fair prospect of a more legitimate system of reasoning being established. This

dered him unparalleled as a public teacher, in the following terms, as nearly as his memory serves him :  
“ Some physiologists, gentlemen, will have it, that the  
“ stomach is a mill ; others, that it is a fermenting vat ;  
“ others again, that it is a stew-pan : but, in my view  
“ of the matter, it is neither a mill, a fermenting vat,  
“ nor a stew-pan ; but a stomach, gentlemen, a  
“ stomach.”



was checked and retarded by the appearance of Boerhaave, in the beginning of the 18th century. He was a man of uncommon capacity, great erudition, and indefatigable industry, and a zealous and honest searcher after what he conceived to be truth. But, probably, from the habitual application to his favourite study, chemistry, he suffered himself to be deluded into what is now viewed as a most fallacious train of reasoning. This he delivered in language so imposing, that his doctrines prevailed universally for about fifty years in the schools of physick, and among the practitioners of all Europe: and it is equally astonishing and humiliating to contemplate, how the assent of an enlightened age, should have been won over to a body of doctrine, so puerile and shallow.

So heavy a censure ought not to be brought lightly against a person so celebrated in his day, who possessed many truly estimable qualities, and to whom science is much indebted for the improvement and diffusion of rational chemistry, and for be-



ing the first who brought the thermometer into general use and notice. In proof of our allegations, let his theory of inflammation, therefore, being one of his most important and peculiar doctrines, be tried by candid criticism and discussion.

He held, that the proximate cause of inflammation, was a morbid viscosity of the blood, obstructing the course of circulation in the small vessels. The main fact brought in proof of this, was, the coriaceous crust formed on the surface of blood, drawn from a patient labouring under an inflammatory affection. This is disproved by considerations so obvious, that it is truly unaccountable that they should not have occurred to this eminent physician and his followers. For, in the first place, it does not appear in blood taken away at the beginning of the inflammation, as it certainly would, if the alleged viscosity were the cause of the disorder. Secondly, The same crust appears on blood, taken from a person labouring under inflammation from a mechanical injury; such as a fractured bone: a sure



proof that it must be an effect, and not a cause. Thirdly, This crust is merely the separated coagulable lymph of the blood, at all times present in it, and an essential constituent of it; and when it separates itself on the surface, it is from increased fluidity: so far is this appearance from arguing viscosity. Fourthly, It is found usually in blood taken from pregnant women. This doctrine of Boerhaave, had an universal currency in the beginning of last century, and materially influenced practice, as appears by the terms attenuant, diluent, &c. introduced in conformity to the theory of the lentor of the blood, being one of the principle causes of disease.

The like judgment may be passed on this author's chemical principles of pathology, by which he referred, the cause of a large class of diseases, to certain acrimonious conditions of the fluids. That morbid acrimony, in various forms, does exist, that its effects are considerable in producing and aggravating disease, that the elimination of vitiated and redundant matter, constitutes some of the most valuable resources of practical medicine, cannot be denied.



But it is equally manifest, that, as the fluids owe their specific healthy condition to organic action, their morbid changes must be brought about by the same means; that is, by means entirely foreign to the processes of chemistry or inanimate matter\*.

We have seen melancholy proofs of the extreme errors into which physiologists have been betrayed, by a false and perverted application of science, in the instances quoted from Pitcairn and Borelli. But, though the principles belonging to inanimate matter, were to be applied with the utmost precision of correct induction, they would go a very short way in ascertaining causes, or guiding practice. It is only by touching the springs of life, that the actions of life can be regulated. The early physiologists, in all their reasonings, have almost entirely overlooked all those energies peculiar to life which have been enumerated; namely, the generative, the conservative,

\* It was in the School of Edinburgh, under Dr. Cullen, that the doctrines of Boerhaave received their chief overthrow. Cullen was the first who clearly marked, and defined the principles of life, as distinguished from those of dead matter.



the temperative, the assimilative, the formative, the restorative, the motive, the sensitive, and the sympathetic, not to mention the affections of the mind.

And it is evident, that, as the actions of life must depend on the compound operation, and reciprocal influence of all these powers, those who propose to found practical medicine on their knowledge of the laws of life, must encounter such difficulties in estimating and ascertaining the result of them, as must appal the boldest theorist. For, as in an algebraical problem, if any one element of the calculation should be omitted, or mis-stated, the result must be erroneous; so, if taking our measures in medicine, due weight is not assigned to each of these influences, our practical inferences must be illusory. These sentiments are admirably expressed by Bacon in the following passage: *Subjectum illud medecinæ (corpus nimirum humanum) ex omnibus quæ natura procreavit maxime est capax remedii; sed vicissim illud remedium maxime est obnoxium errori. Eadem namque subjecti subtilitas et varietas, ut*



*magnam medendi facultatem prebet sic maxime etiam aberrandi facilitatem.*

The circulation of the blood, the distribution of the blood-vessels and nerves, the relative position and co-aptation of the muscles, bones, and viscera, were well known before the middle of the last century; but the existence of the lymphatic system, as co-extended with the whole body, being unknown till that period, and as this knowledge was necessary for understanding the animal machine, professional men were in no degree qualified to account for its structure and diseased action. And who will affirm, that in the present improved state of knowledge, or even in the utmost attainable degree of it, he would be sufficiently confident that his measure of science was such that he could purely *a priori*, act upon it with practical effect? Could any one, though he had reached the very summit of anatomical and physiological knowledge, venture, without the utmost risk of error, to predict or controul the results of actions into which there falls to be considered, not only the



properties of inanimate matter, but the variously combined operations of all those properties peculiar to life, which have just been enumerated, and the influence of mental affections. And when it is further considered, what a mass of credulity and error has actually accumulated in medicine, from the presumptuous attempt to grasp at such objects, and to make hasty and dangerous application of them to practice: when we cast our eyes upon our shelves, loaded with volumes, few of which contain any genuine profitable knowledge, the greater part of them composed chiefly of matter, either nugatory, erroneous, inapplicable, or mischievous, in which the dear bought grain is to be sought in the bushel of chaff: may it not be questioned, whether such researches have not tended more to retard and corrupt, than to improve practical medicine?

Those who are disposed to depreciate the practical value of anatomy, might allege that there are several of the most important functions above enumerated, upon which the knowledge of the structure of the dead body, though ever so minute and perfect,



could throw little or no light; nay, that there are some morbid circumstances and indications in the living body, ascertained by empirical observation, in which mere anatomy is more apt to mislead than instruct. For example, it is known from experience, that impressions made on the external surface of the body, have a decided effect upon the subjacent *viscera*, though there subsists no anatomical relation between them: thus cold applied to the external surface of the thorax or abdomen, will more readily excite inflammation or spasm in the lungs or bowels, than in other parts. And in curing inflammation of these *viscera*, the extraction of blood from the adjacent surfaces, will, as we learn from experience, have much more effect than a bleeding from the system, though there is no anatomical connexion between these parts, that portion of the circulation, which is in the skin, being as remote from that of the lungs and intestines anatomically considered, as any other part of the body. It has also been alleged, by the disparagers of anatomy, that even for the purpose of detecting the seat of diseases after death, the in-



formation obtained is either extremely limited or altogether fallacious; for whether it be from the morbid affection existing in one organ, and the sensations from sympathy being in another; or that the symptoms of disease are different in different constitutions, or from its not being possible to decide what morbid appearance has been the cause, and what the effect of the disorder, it so happens that those even who are most practised in morbid anatomy, are in numberless instances deceived as to the preconceived nature and seat of the disease, as investigated by dissection; so that often little other instruction is acquired, except a lesson of modesty and distrust in ourselves.

Are we then to admit, that the greater part of what we have been taught at the schools of physick, and of what we have read, or may read, in books, is in no wise conducive to our practical improvement?—Far from it, for

1st. Though anatomy, physiology, and pathology, should be proved to be of little or no avail, nay of pernicious tendency in



the practice of physick, the acquisition of these branches of education is nevertheless indispensable, in order to appreciate their value, and in order to be armed with antidotes against the influence of fallacious theories, and to obtain the guidance of true beacons, instead of false lights. There is nothing better known to those, who are conversant in medical practice, than that the most ignorant and shallow, those of the least learning, nay those of no learning at all, are the most addicted to hypothetical reasoning, the most infected with presumption and conceit. The only means, therefore, of guarding ourselves from being misled with false theories, or from the misapplication of those that are true, is to gain a thorough acquaintance with both. I say *thorough*, for the philosophic poet in stating the beneficial influence of a liberal education on the practice of life, does not say simply *didicisse*\*, but FIDELITER *didicisse*; that is thoroughly, and in good earnest. In a word, we should strive to

\* *Ingenuas didicisse fideliter artes,  
Emollit mores nec sinit esse feros.*



attain that only criterion of substantial and profound knowledge, that of knowing how little we know.

2dly. The knowledge of nature, in all its branches, is an indispensable requisite in the cultivation of the mind. It is highly useful, were it only as a gymnastic exercise of the understanding, by that salutary discipline of the mental faculties, implied in the acquisition of habits of attention, and the practice of the reasoning powers. Besides, all arts and sciences have a bearing on each other; and the history and philosophy of animal life, is surely as necessary an accomplishment to a physician, as any other branch of science or literature; and we should be tempted to think, from this sense of the word *phycisian*, being peculiar to our language, that this notion especially prevailed in England.

3dly. The habitual consideration of natural causes, tends to banish superstition, and to abolish the frivolous practices riveted in ordinary minds by early impressions and imposing authorities, or sanctified



by immemorial usage and tradition. These practices are found to prevail not only in rude ages, but in those of considerable civilization ; for, besides amulets, incantations, and various other supernatural influences, certain practices, hardly referable to any ascertainable agency, either natural or preternatural, have prevailed in all ages. We find it, for instance, stated in very good Latin, by authors who flourished about 150 years ago, that turmerick is good for the jaundice, because it is of a yellow colour; and that fox's lungs are good for the asthma, because that animal has strong powers of respiration, as is proved by the long and hard run he makes when hunted. And have we not seen in our own times, persons of liberal education, of both sexes, who could persuade themselves that certain unmeaning motions of the hand, called magnetising, could exert sensible and salutary influences upon persons, however distant? But these need no longer blush for themselves, when they are told that some of our early instructors in the laws of nature, were nearly as absurd. Bacon did not disbelieve in amulets, sorcery, and magic; and Boyle seriously recommends the thigh-bone of an



executed criminal, prepared in a prescribed manner, as a remedy in certain disorders. And can it be doubted, that if practitioners of medicine were not habituated to the contemplation of the genuine agencies of nature, they would be liable to fall into the like weaknesses. The effect of the study of nature, in counteracting superstition, is nowhere so well expressed as by Virgil.

Felix qui potuit rerum cognoscere causas,  
Atque metus omnes et inexorabile fatum,  
Subjecit pedibus, *strepitumque acherontis avari.*

Men of great capacity, and high mental attainments of a different sort, also men of dignified stations, the heads of the law and the church, but who had not given their minds to such pursuits, have been known to become converts to the most grovelling imposture, and dupes of the vilest quackery. When to these considerations it is added, that the fair exercise of judgment is impeded by the inordinate love of life, and fear of death, among those in the full possession of their faculties; that it is not only impeded, but impaired in those who are under the influence of sickness; and when it is farther considered, that great allowance is to



- be made for those who become impatient, from protracted sufferings unrelieved, and perhaps unrelievable by regular art, we shall not find it difficult to account for persons of the best understanding, being occasionally betrayed by their own credulity, or that of their importunate and well-meaning friends, into the most irrational practices.

4thly. Though physiological and pathological researches, even the most correct have had little share in suggesting active and useful remedies, the greater part of these having been discovered in dark ages by fortuitous incidents, or in more enlightened ages by analogical reasoning, yet theories, though ever so visionary, afford useful suggestions. It is also very remarkable, that theories, however different, do often wonderfully coincide in matters of practice with each other, and with well established empirical usages, each bending and conforming, in order to do homage to truth and experience. It has been remarked, that Boerhaave, in following out his fanciful and erroneous views of the animal economy, stumbled upon some practices, the utility of



which were sanctioned by experience. For example, he believed that the blood owed its red colour to iron ; a doctrine found, by future chemists, to be erroneous ; and with this in view, he strongly recommends the internal use of this metal in chlorosis, and other cases of chronic debility, in which there is an evident deficiency of red globules. Though this remedy does not act on the principle which suggested it, it has, nevertheless, been found to be a most efficacious plan of treatment in those disorders. Such suggestions are at least preferable to random trials, and are fairly admissible, if duly tested by experience.

5thly. It must be obvious to every reflecting mind, that those who have made themselves acquainted with the various organs and functions engaged in the animal economy, must have a great advantage in practice over the unlearned empirick, in discriminating the morbid affections from each other, and in varying accordingly the respective means of relief. For instance, a physiologist and anatomist, from his knowledge of the intimate nature of morbid af-



fections, the difference of their seat, and other circumstances, is able to distinguish spasmodic from inflammatory pains; a distinction which would not readily occur to an uncultivated observer, but of the most vital importance in practice, for the remedies required for the relief and cure of a spasmodic pain in the stomach and bowels, demands a treatment, not only different, but opposite, to that which proceeds from inflammation. It is only anatomical and physiological science that can give a practitioner a clear and vivid conception of these and other distinctions, essential to the safe and efficient treatment of diseases.

6thly. Whatever doubts there may be with regard to the degree in which anatomy is useful in physick, there can be no doubt of it, with regard to surgery, in which an accurate knowledge of the relative position and structure of organs is indispensable.

Finally, the state of health ought to be fully known, as a standard by which to measure the magnitude, as well as to ascertain the nature of diseases, as is well ex-



pressed in the following passage of Galen.  
*Cujusque morbi tanta est magnitudo quantum a naturali statu recedit—quantum vero recedat is solus novit qui naturalem habitum ad amussim tenuerit.*

But if the benefits, derivable to medicine from physiological science, are so limited, from what other and better source is improvement to arise? The answer is, from accurate observation; in other words, from enlightened empiricism. It seems an abuse of words, to restrict the term science to physiology and pathology, and to withhold it from those processes of the understanding, by which facts are ascertained and accumulated, and useful inferences deduced from them, constituting OBSERVATION. Shall we dignify, with the title of science, the absurd positions of Pitcairn; the puerile and shallow hypotheses of Boerhaave and Sylvius; and deny it to those solid and applicable truths, the fruits of chaste observation and sober experience, ascertained by those methods of induction which it was the great aim of Bacon to recommend and introduce, as the only parent of legitimate, substantial,



and useful knowledge? The truth seems to be, that a higher order of intellect, a more rare and happy genius, a more correct and better tutored understanding, is required to elicit practical truths by observation, than to coin theories.

By empiricism, is vulgarly understood that knowledge of the virtues of divers medicines, which are supposed to have been ascertained by experience, as applicable to their respective maladies. A few of this description might be named, such as mercury in the venereal disease; fox-glove in a large proportion of cases of hydrothorax; the meadow saffron in most cases of articular gout; and, above all, the citric acid, in sea-scurvy. But the exhibition of these, and the like remedies, constitute a very small proportion of the whole practice of medicine. The number, variety, and complication of disorders, is such, that the most acute exercise of judgment is called for to discriminate cases, to adapt the treatment to the indefinite diversity which occurs in actual practice, and to ascertain the most advisable methods, all which can only be effected by



applying the rules of induction, that is, of enlightened empiricism, together with such lights as can be gathered from chaste and sober theory.

We have already more than once adverted to the profound wisdom displayed in the constitution of our mental faculties, whereby they are made responsive to the constitution of external nature, in the same manner as our senses, and that this is strikingly exemplified by the susceptibility of the human mind to durable associations and habits, from the frequent repetition of connected events, arising out of the constancy of the laws of nature. Unless these were indelibly imprinted, or recorded, as it were, in the early stage of our existence, life could not be maintained; all those instincts, by which we pursue what is salutary, and eschew what is noxious and dangerous, being founded on this principle. The avoiding of fire, of precipices, the collision of hard and pointed bodies, may be quoted as examples of this. And what is called sagacity, in the adult stages of life, is a sort of approach to, or imitation of this



intuitive faculty; but, instead of being the immediate suggestion of nature, is founded on cultivation, which by practice learns to connect cause and effect, means and ends, an operation which, in well turned minds, is performed with promptitude and precision, by interpreting fairly the appearances of nature, and stripping them of those adventitious fallacies, which mislead ordinary minds. In order to attain this, there are required an appropriate natural capacity, the good fortune of not being beset with prejudices in early life, an habitual exercise in the observation of nature, a candid and ingenuous disposition, an ardent love of truth, an exalted sense of duty, a large store of facts in a correct and tenacious memory, the power of combining, comparing, and discriminating these, by an intuitive glance, in the moment of applying them to the practical end in view. This is what is understood by the term *tact*, in English and French, and εὐστοχία, in Greek, being the faculty by which practical facts are decided on, and is performed by an instantaneous, silent, and almost unconscious calculation and induction, to be met



with only in minds, at once happily constituted and highly cultivated.

From this it will be seen, how vain all acquired knowledge is, without practical habits; for in the liberal, as well as in the mechanical arts, expertness can be attained only by frequent and long continued exercise of actual labour: and it is by a happy and appropriate figure, that those who become skilled in languages, painting, eloquence, physick, or the common business of life, are said in Latin, *callere*, whence *callidus*, words derived from *callus*, that is, a horny substance formed in the hands of mechanical artisans, by long and unremitting labour. The same law of nature is illustrated in the following passage from Cicero de Officiis: *Nec MEDICI, nec imperatores, nec oratores, quamvis artis præcepta perceperint, quidquam magnæ laudis dignum sine usu et exertatione consequi possunt.* Was it not clearly the intention of the author, in placing physicians here in the foremost rank, to intimate, that, of all professions, the hardest discipline of practice and experience, was required in phy-



sick? It would be presumptuous in us to think, that he meant thereby to concede to us the precedence in dignity over generals and orators. This collocation of words, may be merely for the sake of euphony, or it may be a climax in which he means to assign us the lowest post. Be this as it may, our best thanks are due to this great statesman, orator, and philosopher, for admitting us into such good company on any terms, after what has been said of us, by Dr. Conyers Middleton, in his dissertation, *De servili conditione Medicorum apud Romanos*.

From all that has been said, we ought to be in some measure qualified to come to a decision, on the celebrated question of the comparative merits of the empirical and dogmatical methods of cultivating physick. It seems pretty evident, that if either method were employed exclusively, or carried to an extreme, the art of physick would suffer, both in its efficiency, and its prospects of future improvement. It has clearly appeared, that, under such a complication of causes, influencing the operations of life,



it would be utterly hopeless to decide any point purely and strictly *a priori*, and that it is absolutely necessary, that experience be called in as an aid and a test to the inferences of theory. On the other hand, a blind empiricism would be found deficient, without that discriminative judgment, founded on an acquaintance with the laws of life, and without those enlarged and correct views of general nature, by which the excess of credulity and scepticism is equally repressed. This question was much agitated in antiquity, and is most ingeniously, eloquently, and judiciously discussed by Celsus, in the preface to his valuable work. He evidently leans to the side of empiricism, which, in the very crude state of anatomy and physiology in that age, certainly argues his good sense; but he by no means explodes the study of the structure and functions of the body, as of no practical utility, and concludes with the following recommendation, or rather apology, for dogmatism: "*Ista naturæ rerum contemplatio, quamvis non faciat medicum, aptiorem tamen medicinæ reddit.*"



The conclusion therefore upon the whole is, that these two methods ought not to be regarded as adversaries, but as allies ; and that good sense will consist in excluding neither, but in fairly appreciating what is due to each.

In the course of the preceding discussions, it has appeared, that, from the dark and complicated nature of the animal economy, there is the utmost risk of error in drawing inferences *a priori*, and that crude and hasty hypothesis has been a source of much false reasoning, and therefore of dangerous error in its practical application. In the following enumeration, therefore, of the several obstacles which have obstructed and retarded the improvement of medicine, this stands foremost :

I. The errors and abuses arising out of false or misapplied theory.

II. The great diversity observable in the constitution of individuals.

III. The difficulty of appreciating the



efforts of nature, and of discriminating them from those of art.

IV. Superstition.

V. The ambiguity of language.

VI. The fallacy of testimony.

The first of these having already been brought fully under review, we proceed to consider the second.



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brought fully under review, we proceed to  
consider the second.



### SECTION III.

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THE second head of causes, which retard and obstruct the progress of medical knowledge, is the Diversity of Constitutions.

Practical knowledge in medicine, as in every other art, proceeds on the assumption, that the course of nature is steady, and that what has a given effect on one human subject, will have the like effect upon another. From what has already been said, however, it is manifest, that this holds less strictly in the living human body, than in any other object in nature, to which art can be applied. The simplicity of the laws of inanimate nature, admits of the most certain inferences, whereas the indefinite action and re-action of the numerous faculties peculiar to life, enumerated in the last sec-



tion, add greatly to the difficulty and uncertainty of experiment and observation, as already set forth. But this is not all; for constitutions being gifted with various degrees of each of these faculties, an endless variety is found to take place among individuals, giving rise to that uncertainty in the results of medicine, which has brought upon it the character of a conjectural art. This diversity is but little observable in mere animals, and seems referable to the artificial habits, belonging to beings capable of exercising reason, and analogous to that variety in the stature and form of the body, and in the features of the face, peculiar to the human species.

What is here meant, is not exactly synonymous with what is technically termed idiosyncrasy, by which is understood, if I mistake not, some rare exceptions to the usual constitution and habitudes of the human body. The diversity here meant, runs through large portions of mankind, and in some degree, through the whole species; in-somuch, that the form and symptoms of the same disorder, and the effects of medicine,



are hardly the same in any two persons, and in many are widely different. What, for instance, can be more different than the usual cases of small-pox : in some, the disorder being so slight, as not to interrupt the business of life, even when not communicated by inoculation ; while in others, it is as malignant and mortal as the pestilence : and there are all the intermediate shades, diversified to infinity by the number of pustules, the degree and kind of febrile action. There is a like diversity, in some measure, in all diseases. The different degrees of susceptibility to small-pox, and other morbid poisons, may also be adduced as a striking exemplification of this diversity of natural constitution ; for, of persons equally exposed to casual infections, numbers escape altogether. And were it not for this diversity, in point of fatality and susceptibility, in small-pox, plague, and other epidemics, the human species might be extirpated by any one of them.

The like diversity is observable in the operation of certain remedies. What can be more different, and even opposite, for in-



stance, than the operation of opium on different constitutions. It is a medicine of such eminent and beneficial effects in the majority of constitutions, by procuring sleep and assuaging pain, that the art of medicine could hardly be practised without it. Yet there are innumerable individuals so constituted as not to admit of relief from it: some in which it is so far from producing these benignant effects, that it causes great inconvenience and distress, such as sensorial disorder, sickness at stomach, febrile heat and anxiety: some in which, though it disagrees in ordinary circumstances, it will procure relief in cases of intense spasmodic pains: some in which it proves ineffectual, or noxious, in every circumstance\*. There is more or less diversity, though not so remarkable as here, in the operation of most, if not all medicines.

It may be fairly questioned, whether diversity of constitution has had its due weight

\* Professor Kuhn, of Philadelphia, mentions a species of rhus growing in Pennsylvania, the exhalations from which are entirely innocuous to some persons, while they are deadly to others.



in the contemplation of medical observers. It is evident, that unless we are fully aware of it, we may expose ourselves to the same fallacy as those who made the contradictory report with regard to the colour of the camelion. This consideration strongly points out the necessity of accurate induction, extensive observation, and the comparison of facts. In a limited observation, to which only one or more cases, of the most infrequent effects of a medicine, may have occurred, one is in hazard of erecting an exception into a rule, by mistaking these facts for instances of the universal effects of it. It is only by computation, founded upon large averages, that truth can be ascertained, and hence the danger of founding a general practice on the experience of a single case, or a few cases.

Does not this diversity also account, in part, for the proverbial discrepancy imputed to medical opinions, and the deplorable controversies which have too often existed among practitioners and writers? Were the members of the profession fully aware of this cause of difference, would it



not lead them into an amicable endeavour to reconcile and account for the inconsistent reports of their respective modes of practice, instead of engaging in contentious argumentation, not always carried on with that dignified coolness and candour, which becomes a liberal profession.

There is nothing in which a young practitioner should be more on his guard, than being misled by the sweeping dogmas of schools, and the indiscriminate practices of sects, or of favourite practitioners. This evil may be conceived to grow up in the mind of a tiro, in the following manner. Let him at his outset, either at a school of physick, or in witnessing the practice of some private practitioner, meet with one or two impressive and imposing cases, terminating happily under a particular treatment, this will attach him undeviatingly to the like style of practice for the remainder of his life, unless his mind should be duly prepared by the caution here inculcated. In a typhous fever, for instance, it may be the lot of one ~~on his first~~ noviciate in practice, to have witnessed, either under his own care, or that

novice



of some respected instructor, one or two striking cures, from an exhibition of strong cordials; another has witnessed life saved, as he believed, by well timed and free evacuation from the bowels or blood-vessels; to a third, it has occurred to see one or two cases, which being left in a great measure to themselves, have by the salutary efforts of kind nature, been conducted to a safe termination. Now, each of these having from his limited opportunities of observation, imbibed a persuasion, that his own method is universally applicable, is guided by it as the rule of his future practice. Nothing seems more clear to a comprehensive mind, than that they are all three right, in so far as relates to their respective class of cases; and that they are all three wrong in regard to the general principles of practice. The cordial method of the first, is well calculated for those constitutions, in which the principles of life are on a reduced scale by original constitution, or brought to a low ebb by previous exhaustion; and in some such cases, the cordial practice in its utmost extent is required to save life. The second style of practice, is well calculated



for those cases, in which there is a redundant and vitiated secretion in the *viscera* of the abdomen, general plethora, or sanguineous determinations to the head, or other vital parts. The third method, is well adapted to those constitutions, which have none of the before mentioned tendencies, and to which the self-healing powers of nature, are all-sufficient. There may be cases, in which a mixture of these methods may be judged advisable; and, in most cases, the practice requires to be varied in different stages of the fever. To practitioners, who are fully aware of these varieties, it will require only an ordinary degree of judgment and sagacity, to make the due discriminations. The like remark will apply to scarlet fever, and other disorders.

In a work, by Dr. Hamilton of Edinburgh, on the utility of purgative medicines, these remedies are recommended, not only in typhous fever, but in scarlet fever; and it is not said, that there are any cases to be excepted, in which this treatment is not advisable. I have certainly seen cases of both these sorts of fever, without any perceptible de-



viation from the healthy state in the secretions of the abdomen, and in which purging would seem not to be called for, particularly in scarlet fever, in which the employment of purgatives, as a general practice, is considered by the best practitioners in this quarter, as highly pernicious; and there are few symptoms so certainly fatal in this disease, as a spontaneous diarrhœa. Though it is to be wished that the author of this instructive work, had expressed himself in a more qualified manner, he has by no means merited the severe criticism inflicted by a German Journal, where it is said, “that  
“ Hamilton’s recommendation of purga-  
“ tives in typhous fever, only proves what  
“ blunders and absurd methods of treatment  
“ the human body can, in certain circum-  
“ stances, withstand\*.” Whatever error our author may have committed, these journalists have been guilty of a still greater, by denying the utility of the practice in all cases whatever.

As the present subject of discussion re-

\* See Bibliothek der Heilhund H. Band, P. 184, Berlin, 1809.



gards the distinctive application of medicine, I shall take the liberty of making one or two more remarks on this work, which I should not stop to do, if I did not entertain a high respect for this author and his work; which is deservedly popular, and one of the most useful in practice, which has appeared for some years.

One of the diseases, in which he recommends, unqualifiedly, the employment of this treatment, is the *chorea sancti Viti*. The qualified adoption of this practice, I am so far from disputing, that I lately imitated it with success in the case of a young female from the East Indies, in which the vitiated quality, incredible quantity, and long continuance of alvine *sordes*, was such as to bid defiance to all the principles of physiology and pathology, to account for. But in a young English female, under my care, about the same time, for the same complaint, nothing preternatural being observed in the alvine discharges after the first clearance of the intestines, she was successfully treated by the cold bath and metallic tonics, chiefly the sulphate and oxyde of



zinc, and the recovery was effected in a shorter time than in the other case. I found, that in St. Thomas's Hospital, the like success attended the latter treatment of this disease. Opium, Hyosciamus, and leeches to the temples, were found good auxiliaries.

There is another point, in which I feel myself bound to dissent from this respectable practitioner. He denies that different species of purgative medicines possess distinct powers over the different species of matter to be evacuated ; and contemns the notion of cholagogues, hydragogues, &c. In the course of my experience, there is no practical fact with the truth of which I have been more satisfied, than the specific action of the various species of these remedies in stimulating different organs, in dislodging and eliminating different species of corrupted secretions, and other feculent matter. What, for instance, can be more different than the operation of aloes acting as a mere eccoprotick on the muscular fibres of the intestines, and only in a particular portion of them, namely, the descending colon and rectum, and expelling only solid fæces, from that of elate-



rium, of which half a grain, taken three or four times at the interval of half an hour, evacuates immense quantities of serous fluid, exhibiting an example of the wonderful power of sympathy; for an impression made on the internal surface of the stomach, by a few particles of matter, conveys by magick, as it were, an impulse to the most remote extremities, rousing their absorbents to action; and, in case of *œdema* there, awakening the sleeping energies of these vessels, which, like millions of pumps at work, transmit the morbid fluid to the intestines and urinary passages, effecting a detumescence of the hydropic limbs in the course of a few hours, and affording a striking illustration of the operation of sorbefacient medicines. Again, what can be more different than the operation of neutral salts and calomel, the former exerting but little influence beyond the surface of the intestines, exciting the action only of the serous exhalants, mucous glands and follicles; while the other, by extending its stimulus to the biliary ducts and pores, detaches foul congestions, which the other could not reach. Ipecacuanha acts specifically on the stomach; and other medicines



on other portions of the intestines and on different glands; and, it is presumable, that no two articles stimulate equally the same organs. It is for this reason that compound purges are found more beneficial than simple articles; as they touch a greater number of the intestinal organs\*. The combination found most convenient and efficient, and most employed as a general purgative in the practice of this metropolis, when no specific operation is indicated, is a mixture of purging salts and senna, a combination, but little employed by Dr. Hamilton. Dr. Cullen used to say, that senna was one of the best purgatives, if it could be divested of its griping quality; which he had in vain attempted by manna, and various aromatics. We find that combining it with salts, has completely answered this purpose. x

After the exposition which has been made of the great variety of constitutions, would it be too much to affirm, that all the practi-

\* See an Article, by Dr. G. Fordyce, in the second Volume of Transactions of a Society for the Improvement of Medical and Surgical Knowledge, p. 214, London, 1800.

*See also Galap in his father's*



cal works in existence ought to be re-composed, in order to insert in them, for the benefit of mankind, and the credit of the profession of physick, the following qualifying words. “ The practice recommended  
“ will be found to answer in a great majority of cases; but there are numerous exceptions to it, which it behoves every  
“ judicious and conscientious practitioner  
“ to bear in mind.” There is a sentiment, similar to this, in Dr. Anderson’s Agricultural Tracts. He says, “ the inutility of publications on agriculture, has chiefly been  
“ owing to the authors not specifying clearly  
“ the nature of the soil to which the practice recommended applies.”



## SECTION IV.

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THE next obstacle, in the way of our practical judgments, is the difficulty of ascertaining to what degree the efforts of nature operate in the restoration of health, in what cases, and to what point the interposition of art is necessary and salutary, and how the operations of nature and art are to be distinguished from each other.

The self-healing powers of nature have already been adverted to, in enumerating the principles peculiar to life: and that there is such an energy implanted in animal nature, must be obvious to the most illiterate and careless observer; for both, in mere animals and in man, not only wounds are cured, but various maladies are removed, without any interposition of art. This is effected by



virtue of the energies which sustain life from the beginning to the end of its existence, in opposition to the noxious and destructive causes with which it is incessantly assailed, and perpetually at war, as it were. Such, indeed, is the virtue of this self-preserving and presiding energy, that whatever deserves the name of *cure*, is referable to it as the work of nature; for the operations of art consist merely in regulating it, either by exciting it when languid, restraining it when vehement, in changing morbid action, or in obviating pain, or irritation, when they stand in the way of its salutary courses. This, I apprehend, is so well understood among well educated physicians, that the word *cure* as applied to themselves, is proscribed as presumptuous, and rarely, I believe, escapes the lips of any practitioner, whose mind is duly tinctured with that ingenuous modesty which characterizes the liberal and correct members of the profession.

It has already been fully argued, in the beginning of this Dissertation, that the interposition of art is founded in reason, and



necessary for the preservation and restoration of health; and it would be highly desirable, if the provinces of nature and art, could be defined by some precise line of demarcation, in order to prevent mutual encroachments.

From the nature of the subject, this can only be done by approximation. One of the principle steps towards it, will consist in an accurate description and history of diseases, particularly where there had been little or no interference of art; this being necessary, in order to ascertain by a standard of comparison, what in actual practice is assignable to nature, what to art. The settling of such points is liable to great ambiguity; and there is perhaps no subject more exposed to that most common of all fallacies in the practice of life, as well as medicine, the mistaking of the *propter hoc* for the *post hoc*. Some of the early records of physick are very valuable in this respect. Hippocrates, for instance, in his epidemics, gives a number of minutely detailed cases of fever, in which little or no medicine was employed, few having been then discovered. The results



are in support of the argument of those who maintain the opinion in favour of artificial means, for the proportion of mortality being that of 22 on 45, far exceeds that of any modern statement in the like cases\*. In the present times, so many remedies are known, that the omitting of them, with a view to ascertain the comparative powers of nature and art, would be deemed an unwarrantable experiment; so that an inquisitive mind, prone neither to scepticism nor credulity, but anxiously and honestly intent on observation, finds itself debarred from instituting satisfactory inductions for the regulation of practice.

It is also difficult to mark where nature ends and art begins; for, in the rudest state of society, or in the most destitute circumstances of life in civilized society, though no article of the *Materia Medica* should be administered, there will be an exercise of judgment called for in the application of heat and cold, of fresh or confined air; also

\* See Medico-Chirurgical Transactions, Vol. iv. p. 128, where this subject is treated more in detail.



of diet and exercise, and by the injudicious regulation of these, nature may be as much thwarted as by a perverted use of remedies. And is not the scope ~~for~~ nature more likely to be mistaken by the ignorant and vulgar, than by persons of cultivated minds? But the warmest advocates for the sufficiency of the ways of nature ever so wisely interpreted and conducted, will not controvert the evidence in favour of such remedies, as bleeding in pleurisy, or mercury in the venereal disease; and of certain remedies, whether depletory or cordial, in certain circumstances of continued fever.

It is manifest, therefore, that a large share of medical skill must, at all times, and in all cases, consist in ascertaining to what extent nature may safely and advantageously be entrusted with the cure of disease, so as to supersede the unseasonable and injurious interposition of art, and in discerning, as far as possible, what is due to the self-healing power of nature, and what to the co-operating resources of skill, which the practitioner is called upon to exert for the preservation of life, and the restoration of



health. Without some principle, more or less definite on this subject, he would be continually groping in the dark, and would feel himself full of discouraging hesitations and painful reflections. For if his mind had a bias to scepticism, he might on some occasions be unable to satisfy himself, in case of a fortunate result, whether his patient had recovered by *virtue* of the means employed, or in *spite* of them; and in case of a fatal result, his feelings would be still more distressing: if on the other hand, he should be prone to *credulity*, he might be so far blinded as, *bona fide*, to congratulate himself on a *great cure*, in what might only be a *happy escape*.



## SECTION V.

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### SUPERSTITION.

As most diseases, particularly calamitous epidemics, were supposed, in rude ages, to proceed from the anger of the gods, the remedy naturally suggesting itself was, the means of appeasing them. Sacrifices, even human sacrifices, were resorted to in times of Paganism; processions, and other religious observances in the Christian world.

Under the head of superstition, may be ranked fatalism; for it follows from this dogma of faith, that all means of averting predestined events, that is, all future events whatever, are not only unavailing, but impious. It is manifest, that if this were consistently adhered to, every effort conducive to



self-preservation, or even the common comforts and accommodations of life, would be paralyzed, there would be an end to all the pursuits and duties of social life; nay, to the very existence of the human species. Though this speculative principle, however, has never been able entirely to overpower and extinguish the feelings and dictates of nature to this extent, except among a few fanatical maniacs, there are proofs enough in the history of mankind, of its pernicious practical effects. One of the most conspicuous examples of this, is found among the professors of the Mahomedan faith, in their abstaining from the means of stopping the progress of the plague. Among Christian sects, professing this doctrine, the like evils have arisen in an inferior degree, as exemplified in the opposition which the inoculation of the small-pox met with from this religious prejudice.

There are minor species of superstition, which influence weak and uninstructed minds in all ages and countries. The vulgar, even in the most enlightened periods, are not entirely exempt from belief in the



powers of sorcery and magic, and other fantastic and imaginary agencies, such as exorcisms, charms, and amulets. It is pleasing, however, to contrast the present times, in which there is almost an extinction of these delusions, with ages not very remote. It is only 182 years since great numbers of persons were condemned to death in the ordinary course of law, and executed for witchcraft in England \*; and only 119 years since the like disgraceful proceeding took place in Scotland.

Many superstitious, or rather fantastical remedies, are to be found in the works of Sir Theodore Mayerne, who possessed all the medical erudition of the age in which he lived; that is, the first half of the 17th century. He was physician to three Sovereigns of England, was held in the highest estimation at court, and had, during that time, by

\* In the year 1646, two hundred persons were tried, condemned, and executed for witchcraft, at the assizes for Suffolk and Essex, (See Howel's Letters); and in 1699, five persons were tried by special commission at Paisley, in Scotland, condemned and burnt alive for the same imaginary crime.



far the greatest practice in this metropolis. We find among his remedies, the balsam of bats for hypochondria, remedies taken from certain parts of adders, sucking whelps, earth-worms, &c. We find also, as articles in his materia medica, the secundines of a woman in her first labour with a male child, the bowels of a mole cut open alive, mummy made of the lungs of a man who died a violent death, and other articles equally ridiculous, besides various amulets.

But superstitious practices ought not in all cases to be disregarded. The rust of Telephus's spear, mentioned in Homer, as a cure for the wound it had inflicted, was probably the *æruugo æris*, the weapons in those days being chiefly made of brass; and this is found by experience, one of the best applications for cleansing sores, and disposing them to heal. The inoculation for the small-pox, in India, Turkey, and Wales, was practised on a superstitious principle, long before it was introduced as a regular practice into this country. The superstition consisted in buying it; for the efficacy of the operation in giving safety, was supposed to



depend upon a piece of money being left by the person who took it for insertion. And it is not a little curious, that the same practice should have existed in countries so remote. (See Annual Register, and Russel's History of Aleppo. See also some interesting remarks on the practice of Savage Nations, in Stewart's Elements of Philosophy of the Human Mind.)

It has been already mentioned, that the specifically appropriated remedy, for these maladies of the mind, is the cultivation of natural knowledge; and it is equally curious and gratifying to observe, that, though the lights of science are attained by only a small proportion of the community, the benefits of it diffuse themselves universally; for the belief in ghosts, and witches, and judicial astrology, hardly exists, in these days, even amongst the lowest and vulgar. This effect of knowledge, in banishing the vain fears of superstition, is finely alluded to in the last words of the admirable lines, already quoted from Virgil, *strepitumque acherontis avari.*



The inference, from the whole of these reasonings, is, that the entire phenomena of animated, as well as inanimate nature, are referable to the established order of nature, and are so many exemplifications of her laws.



## SECTION VI.

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### THE AMBIGUITY OF LANGUAGE.

As the end of language is the communication of thought, it is self-evident, that there can be no such thing as correct reasoning, unless the same import be annexed to the same words, in the oral and written intercourse of mankind. A large proportion of all the false reasoning and controversy, which has existed among the learned and the unlearned of all ages, has arisen from the want of a precise definition of words. The most valuable parts of the writings of Locke, are those which relate to the abuse of language. It is a subject upon which there is great room here to dilate ; for none of the departments of practical knowledge have suffered more than



medicine, from ambiguous phrases, and verbal disputations. But this dissertation, having already drawn out to a great length, the author will satisfy himself with illustrating the subject by a very few examples, referring the reader to the chapters in Mr. Locke's work, which relate to this subject.

The term *scurvy*, in the English language, and *scorbutus* in the general medical language of Europe, has been employed to denote, both cutaneous eruptions, and that disease which is caused most commonly by a long use of salt provisions, and principally known by its appearance in ships which have been long at sea. By having this name in common, these two diseases, though widely different, may, with nothing in common in their characters, have been considered identical, and treated as such, particularly by a Dutch physician and author, named Eugeleus, in which he was followed by his countryman Boerhaave, and other authors, both British and continental. The consequence of this strange jumble was, that a very vague, inefficient, and inconsistent practice, of a piece with the doctrine,



was adopted. And it seems to have been chiefly to the want of the clear conception of the *diagnosis*, and peculiar nature of the sea-scurvy, that a simple, infallible, and easily procurable remedy for it, was long neglected, to the incalculable detriment of humanity, and the public service. Lemon-juice was well ascertained to be a certain preventive and cure of this dreadful malady, very early in the 17th century; but the attention of physicians having been absorbed in vain speculations, and their judgments ~~pre-~~<sup>perverted</sup> ~~ventive~~ by this ambiguity, this excellent remedy was so much overlooked and neglected, that it was not rendered available to the best interests of mankind, till after the middle of the 18th century. If the expedition fitted out under Commodore Anson, in 1740, had been furnished with a few casks of lemon or lime-juice, none of the dreadful sufferings, which freeze the blood in the narrative of that voyage could have occurred.

Another example, equally illustrative of the fatal effects of ambiguous language, may be adduced from the misapplication,



and vague acceptation, of the term *yellow fever*.

In order to treat this subject intelligibly, it is necessary to remind the reader, that there are three sorts of remote causes which give occasion to fevers, in whatever climates they may arise. One class of these causes is the exhalations of the soil, producing intermittent, and remittent fevers, which occasionally pass into continued fevers, particularly when under the influence of other remote causes.

The second class of occasional causes, is vitiated human *effluvia*, generated by the living body under circumstances of crowding, filth, want of ventilation, and change of apparel, aggravated occasionally by scanty and unwholesome food, as exemplified in the jail, hospital and ship fever, and that of the indigent part of the population : and all pestilential epidemics seem to have had a similar origin, diversified according to circumstances not always definable and ascertainable. In the pestilential distempers, recorded by historians and poets, which broke out in



besieged towns and besieging armies, famine appears to have been a main element in their production. That described by Thucydides, at Athens, was quite different in its characteristic symptoms from the modern plague of the Levant. The pestilential disease called the Sweating Sickness so fatal in England from 1486 to 1551, was still more different from it. It was engendered by the wretched state of the army brought to England by Henry the Seventh, previously to the battle of Bosworth. There are interspersed, in the records of physick, the histories of many other peculiar infections, some of them local, extremely limited, and transient \*. Infectious fevers, therefore, arise, prevail, and become extinct, under circumstances endlessly varied according to the indefinite combinations of the pre-disposition of the subjects acted upon by the different degrees and kinds of impure air, bodily and

\* See a remarkable instance of one of these in *Medico-Chirurg. Trans.* vol. ii. page 234; also some Observations on the Nature of Infection, in an Article in the 3d volume of *Transactions of the Society for the Improvement of Medical and Surgical Knowledge*, p. 425, by G. Blane, M.D.



mental hardship, and privations. These combinations are so delicate and inscrutable, that in ships and other communities in circumstances apparently similar, there shall be a grievous infection in one, while the other shall be mildly affected or entirely exempt. It is for this reason, that those who closely investigate the history of infection, meet with so many unaccountable and anomalous cases, much more perhaps than in any other branch of natural knowledge. There is here, therefore, the utmost risk in reasoning from analogy ; and no inferences can be trusted but those which consist in those pure matters of fact, which constitute each particular case.

The third class consists of that disturbance of the system, occasioned by fatigue, insolation, intemperance, the privation of food, and sleep, sudden alternations of heat, and cold, acting either jointly, or singly, in creating fever.

Of these three, the second only is found to be contagious.



They are all three found to exist in the West Indies, (by which is meant the Islands called the great and small Antilles, or Caribbean and Mexican Archipelago, and the adjacent coast of America) in common with other countries.

But in conformity to what has been said regarding the irregularity of the *phenomena* of infection, there are found to be peculiar and unaccountable circumstances regarding it on this station, for the fevers originating from these three remote causes are all accompanied with a yellow color of the skin, which being a conspicuous symptom, has procured for them all the appellation of yellow fever, giving occasion to great confusion, and serious mistakes. This color of the skin is not quite unknown in fevers in other parts of the world, even in cool or temperate climates. I have met with it in London, both in my hospital and private practice, several instances of it have occurred in the typhous fever which lately prevailed in Edinburgh. Dr. Cleghorn met with a few examples of it in the endemick of Minorca. But it is only in the West Indies that it is met with as a general and cha-



racteristic symptom of the endemic and epidemic fevers of the country. In this sense it is unknown even in all other intertropical regions. There is no such symptom in the bad fevers of the East Indies. Dr. James Lind \* has given one of the best accounts of that which prevailed at Calcutta in 1762, and this makes no part of its description. Dr. Johnson, in the history of an epidemic fever in Batavia, says, that the yellow color of the skin occurred in the course of the disease, in a great proportion of the cases, but this does not amount to a general diagnostic, as in the Antilles. It would be only matter of idle speculation and conjecture, to attempt to decide whether this peculiarity in the Antilles, is owing to some singular properties in the soil and air, or to the circumstances of the slave population, or importation, or as some have alleged, to some lurking specific virus, at all times more or less present in the sea-port towns of those Islands.

\* This is not Dr. James Lind, of Haslar Hospital, but a physician of great learning and ingenuity, who passed the latter part of his life at Windsor, where he was most highly respected.



Another remarkable circumstance, with regard to the origin of the West India fevers, is, that they sometimes are found to arise from the foul vapor of ships replete with filth, from long neglect of cleanliness. And it is remarkable, that the fevers arising from this cause, are found sometimes to be contagious, and sometimes not, according to the intensity and nature of the effluvia, and the susceptibility of the subjects exposed to it. It seems to be a general rule, that no effluvia, emanating from corrupted dead matter, even in a state of the rankest putrefaction, ever produces a fever of a contagious nature. It is presumable, therefore, when these exhalations do produce contagious fevers, that they consist, in part at least, of the vicious effluvia, generated by the living human body, constituting some form of the typhous morbidic poison.

In order to give that precision to language, which is necessary on every subject, and with a view to avoid misconception, and wrangling, these three classes of fevers shall be designated as follows ; the first, shall in the course of this discussion, be called



the ENDEMIC; the second, the PESTILENTIAL \*, or MALIGNANT EPIDEMIC, or TYPHUS ICTERODES, as it is termed by some systematick writers †; the third shall be called the SPORADIC.

All the three are in vague and vulgar language, styled the yellow fever, and the utmost confusion has arisen in treating of them, as must ever be the case, when one author, or disputant means one thing, and the other a different thing. It has accordingly been from want of precision, in naming and classing these fevers, that controversies highly unbecoming a liberal profession, and what is infinitely more unfortunate, errors of the most fatal practical tendency have been engendered by this confusion, and ambiguity of terms.

\* By pestilential, it is here meant only to express a very high rate of mortality, and in this respect the epidemic in question takes the precedence of the plague; for, on a population of 16,000 civil and military, at Gibraltar, the mortality in 1804 was 6000; a proportion considerably above the usual devastation of the pestilence of the Levant.

† Sauvages and Cullen.



This fallacy has in no instance been more glaring, than in some articles which have lately found admission into a respectable periodical publication \*. The fever there described, is, by the author's own account, and avowal, evidently of the endemic kind, and in strenuously maintaining that this is not contagious, he is fighting a phantom of his own creation ; for no rational advocate of contagion, has ever alleged that a fever of this kind is contagious. The shallow and perverted reasoning of this and some other authors, would not have claimed notice, but as what they give to the world may prove mischievous, by some inexperienced or weak practitioner, applying what is advanced by them, to the pestilential epidemic fever, it becomes the duty of one, who has been nearly forty years in the medical service of the State, to counteract the baneful impression it may make ; and as no example more apt could be selected to illustrate the subject in hand, he seizes this opportunity of doing so.

\* See Medico. Chirurgical Transactions for 1818, vol. ix. part 1.



The fevers proceeding from long confined human effluvia, seldom originate in the West Indies; for the heat of the atmosphere is such, that it is not necessary to exclude the fresh air, as in Europe, and other temperate climates. Such fevers, therefore, have become epidemic there, only in consequence of infection occasionally generated and imported by ships under peculiar circumstances of crowding, and filth, exasperated by the length of the voyage, and various hardships affecting the minds, and bodies of the crews, and passengers. Examples of these casual incidents are to be found in the history of the different maritime powers, who have planted colonies in this part of the world. Among the English, the most remarkable and well attested are that of a fever which arose in Barbadoes in 1647, as recorded by Captain Ligon, in which he says, that the mortality was so great, that the living could hardly bury the dead. He ascribes the origin of it to certain ships, which had just arrived from long voyages. The same epidemic broke out in the same Island in the year 1695, as related in Hughes's History of Barbadoes, at which



time, the former had been so far forgotten, that it was called *The New Distemper*, a proof at the same time, that it was totally different from the common endemic and sporadic fevers. But the most remarkable of all the epidemics of this kind, which affected the West Indies, was that which arose in 1793, first noticed in the Island of Grenada, in the month of March, a season, at which the endemic, and sporadic fevers, are the least prevalent, and favored by the peculiar circumstance of the French revolutionary war which broke out that year, it spread rapidly to the British, French, and Danish colonies, attacking them not simultaneously, as it would have done had it been the endemic, but successively, and therefore contagiously. In the September of that year, it reached Philadelphia, where it had been unknown since the year 1762. In the following year, it visited Charlestown, in South Carolina, and in the subsequent year 1795 New York, in all of which cities, it was attended with the calamities of a pestilence. It has since that period, at various intervals, visited all the maritime towns in the United



States of America, from Georgia to New England.

I should have remarked, that this fever was believed on good grounds, to have been brought to Grenada in a ship from the coast of Africa \*, from which it spread

\* The first and principal publication on this subject, is by Dr. Chisholme; who, like the inhabitants of Barbadoes in 1695, believed it to be a new disease, and calls it a *nova pestis*. This argued a want of knowledge of its history, but it argues at the same time that this belief could not proceed from *prejudice* or *prepossession*, for these imply the *pre-existence* of their object, which in his mind was a non-entity, and the notion of its novelty and its nature, must have been founded on the immediate local evidence of the case, operating an honest conviction in his mind. Dr. Chisholme seems also to betray a want of knowledge of the history of infection, in his great anxiety to prove that a malignant fever must previously have prevailed in this ship. It is well known, that typhous infection will accumulate to the most intense degree, without affecting those by whom it has been generated; and that it is on strangers that it exerts its virulent properties. All the medical practitioners of that Island had the like convictions; only one disbelieved in it, and he but for a short time; and it is no small proof of its contagious nature, that six practitioners of medicine died of it, a mortality far greater



to the ships at anchor around her, destroying in a short time two-fifths of their crew. It then passed to the inhabitants on the adjoining part of the shore, spreading by steps, which could be clearly traced from the original infection in the ship.

In the year 1800, it made its way to the shores of Europe, and exhibited all its tragical effects at Cadiz : also in a slight degree next year, but returned in 1804, with even greater violence than in 1800. Its next appearance here, was in 1813, and the last in 1815. It broke out at Malaga in 1803, and in 1804, at Gibraltar, where it also made its appearance slightly, in the year 1810 and 1813, but was checked in its progress by vigilant measures of police.

It next spread, at various intervals, to Carthagena and Alicant, and in 1804, to than that of any other class of men. Dr. Stuart, a practitioner of nineteen years standing, and to whose excellent judgment, and long previous experience of the climate, the utmost deference is due, has on various occasions publicly attested his firm belief, that this epidemic was a disease quite distinct from any that had ever before come under his observation. Dr. Gilpin physician to the army bears the same testimony.



Leghorn, which was probably the most northerly place to which it could, by its nature, reach ; and on that account it was here experienced in a very slight degree. The yellow skin and black vomit, however, put it beyond a doubt, what the nature of it was.

In Europe, its first appearance was at Lisbon in 1723, probably brought from Brazil, and it has never since appeared there. It next appeared at Cadiz in 1732, three years after its first appearance in their colonies. It returned in 1733, again in 1744 and 1746, and 1764, and not again till 1800 ; nor was it known in all that space of time, in any other part of Europe, except once at Malaga, in 1741. These dates are taken from Baron Humboldt, in whose accuracy, and fidelity, I have perfect reliance.

To return to the West Indies : among the French, the most remarkable instance of this epidemic, is that recorded by Père Labat, of its introduction into Martinique in 1686, by a fleet, under peculiarly calamitous circumstances, from Siam, whence it got the name of *Maladie de Siam*, and *fièvre de Matelôt*. And Pere du Tertre,



who wrote a history of the French colonies, in the end of the same century, calls it *La peste jusqu' alors inconnue dans les isles*.

In the Spanish West Indies, it is mentioned by the historians of that nation\*, that it was unknown in those colonies till the year 1729, when it appeared at Cartagena, in Terra Firma; and that it broke out in Guayaquil, in Peru, in 1740. With regard to the Portuguese, it is distinctly described by Ferrayo de Rosa, a physician of Olinda, in Brazil, where it broke out in 1687, immediately after the conquest of Pernambuco.

In North America, the first mention of it is at Boston, in 1693, where it was believed to be brought by the fleet from the West Indies, under Admiral Wheeler, and probably derived from what was imported in the fleet from Siam a few years before.

\* See a Voyage to South America to measure a degree of the Meridian, by Don George Juan, and Don Antonio D'Ulloa. The same very respectable historians say that those who have once had it are not liable to it a second time, vol. i. p. 46. English Translation, Lond. 1758.



The next mention of it on that continent, was at Philadelphia, in 1695, and at Charlestown the same year.

We next hear of it at New York in 1702. There is no detailed history of it, but being designated by the appellation of the *Great Sickness*, and described as little inferior to the plague, it can be no other than our pestilential epidemic imported by commercial intercourse with the West Indies.

The first medical description of it is by Dr. Mitchell of Virginia in 1744, who imputes it to contagious effluvia.

The next professional account of it is by Dr. Lining, of Charlestown, written in 1748, but comprising the description of this epidemic as it appeared there in the years 1732, 1739, 1745, and 1748. He says, it was undeniably infectious; and that in every one of these years, the introduction of it could be traced to persons arriving from the West Indies. (See *Physical and Literary Essays of Edinb. Vol. II.*) There is no



account of it that I know at Philadelphia in the last century, till 1751 when the infection was introduced in a trunk of clothes, belonging to a person who died of it in Barbadoes. It spread only to the family and a few of the neighbours, and no more than two hundred persons died of it. But it was not fatally epidemic till 1762, and did not re-appear there till 1793, as before mentioned.

There are two questions at issue, with regard to this epidemic. 1st. Whether it has been occasionally engendered on board of ships, and carried to the sea-port towns of the West Indies, and from thence spread, by contagion, to North America, and Europe; or whether it is only a different degree and form of the endemic, and sporadic fevers which at all times prevail more or less in these countries. The other question is, whether it is infectious. In the agitation of this controversy, these questions have in general been regarded as involved in one issue; importation being considered as implying contagion. But on this subject, a schism has arisen among the non-contagionists; for Dr. Bancroft had allowed



in his Essay on the Yellow Fever, 1811, and still more explicitly in the sequel to that work, 1817, that this disease might be excited by exhalations from the ballast, and stores of ships, while he denied that it could be conveyed on men's persons, or that when excited by these exhalations, it could become contagious. He adduces, in proof of this, the fact, that the fever in the cases in question, near New-York, did not extend beyond those who had connexion with the ships, overlooking however a passage in the narrative, stating, on an authority which he will not question, that of Dr. Edward Miller, the great champion of non-contagion, that the whole inhabitants fled from the spot, "by which it was suddenly arrested." It may here be remarked, that the unsophisticated good sense of the inhabitants of the countries in which this epidemic has prevailed, taught by direful experience, every where fly from it with instinctive horror.

Dr. Miller, and others of the party of non-contagionists, will not however concede to Dr. Bancroft, that the fever can originate



in any form, from a foreign source, continuing sturdily to maintain its exclusive domestic origin. To this there are two exceptions, D<sup>r</sup>. Lidyard, and Lord \*, both of whom publicly renounced their long entertained opinion of its endemic nature, and yielded to the irresistible evidence of its foreign origin. We have not learned whether they became converts to contagion, as well as to foreign origin, but the first mentioned of these candid and ingenuous gentlemen, has since unfortunately fallen a victim to the disorder.

But this is not the only point, in which importation, and non-contagion, have been disjoined, for Baron Humboldt, while he throws great doubts on importation, founding his opinion on the writings of Rush, and others to whom he refers, expresses his firm belief in the contagious nature of the Andalusian epidemic, founding it on the very able report of the three commissioners appointed by the French Government to investigate

\* See American Med. Phil. Regr. vol. I. page 484. and vol. I. page 299.



it, and who describe it as spreading from house to house, by contiguity, as a fire does, and he states it as fully ascertained, that those families who shut themselves up in the midst of it in Cadiz, escaped it.

It has been objected by the other party, that those commissioners were not on the spot when the epidemic prevailed. If this objection were well founded, it would go to invalidate all judicial investigations whatever. It is not deemed a necessary qualification for a judge on the bench that he should have been actually present at the transactions upon which he is to decide. On the contrary; by an accurate and comprehensive survey of the points and bearings of a complex case, he is better qualified to form an opinion, than the actual actors in them, besides being divested of prejudice. It is requisite, for the forming of a clear, calm, and impartial judgment, that objects, whether natural or moral, should be placed at a certain distance, in order that they may be seen in those relative positions and bearings, which the eye and mind of a close observer, or of a party concerned, is incapable



of taking in. A soldier in the midst of a battle, knows much less about the main incidents and results, than a shepherd on the neighbouring hill. And with regard to the distance of time, the investigation not having been made till 1805, four years after the epidemic of 1800, and one year after that of 1804, the same principle will apply to it as to general history, in which it is a maxim, universally admitted, that no impartial history can be written till a whole generation at least shall have passed away. The same length of time may not be necessary to mature medical as historical truth; but, after a still longer interval than what is here objected to, it is presumable that the question will be viewed in a still clearer light.

There was the same proof of the existence of contagion at Gibraltar in 1804, as at Cadiz, for the Spanish Consul, Colonel Myers of the Engineers and others, disregarding medical opinion, followed this practice, and by seclusion, saved themselves and their families,

In considering this subject on the general principles of abstract reason, it seems



much more presumable, that the malady in question should originate from a fortuitous cause, than from the common course of nature. Is it conceivable, that a disease, totally different from any till then known, in the memory of man, by tradition, or history, should in the course of seventy years, as at Cadiz, make its appearance six times, at unequal intervals, and in no other spot in Europe, except once at Malaga, unless from a foreign cause; and it is remarkable, that the degree of prevalence in those parts of the old world was in exact proportion to the degree of intercourse with the new world, and with each other; for the ships from the Spanish colonies hardly frequent any European port, but that of Cadiz, and the intercourse of this city with Malaga, Carthagena, Alicant and Leghorn, and of these with each other, is more frequent, and constant, than with any others. Nor has this epidemic ever made its appearance either in rural districts, nor in any inland or uncommercial towns, such as Rome, Naples, Palermo and others lying in the susceptible latitudes. Nor has it appeared in Turkey or Africa though in the same latitudes, except in one instance in the latter, in that part which is adjacent to Spain. Again, is it conceivable



that during the hundred years, that Gibraltar had been in possession of the English, that is from the year 1704, when this fortress was taken by the army under the command of the Prince of Hesse, to the year 1804, in which this pestilential epidemic fever for the first time broke out, this disease should never once have shewed itself, if it depended on causes at all times existing, and present? When it is gravely affirmed by a medical authority, that this singular, and till then unheard of epidemic, could here proceed only from the exhalations of the soil, and when the circumstances of this arid rock are taken into account, the author appeals to his reader, whether a proposition more extravagant, more repugnant to reason, more irreconcilable to history, and analogy, ever fell from the mouth or pen of any man. Is it not inconsistent with every conception of an endemic disorder, and contrary to observation, and experience, that it should thus be unknown for so long a series of years, and considering this abstractedly, is it not repugnant to the first principles of reason, that a casual effect should proceed from a constant cause? The same may be said of Cadiz, the whole sur-



face of which is either rock or sand; and while these pestilential epidemics raged in Cadiz and Gibraltar, the districts around which are really marshy, were entirely free from it, as well as all the rest of the country around.

The like reasoning will apply to the occurrence of this fever in the West Indies, and North America, and when it is farther taken into account, as an additional element of computation in the doctrine of chances, that these new and singular events thus combined in each of these tracts of the globe, widely disjoined indeed, but in a state of constant intercourse, fell out in all of them in the course of one and the same short series of years, in succession in the first instance, and then in co-existence; it is quite inconceivable on principles of calculation, as well as by the laws of nature, that these events should happen by fortuitous co-incidence, and without the least relation to each other, as cause and effect.

Whoever will take a steady view of the subject in this light, must perceive, that it would be equally inconsistent with mathe-



mathematical and physical probability, to admit such a supposition. It must be admitted, however, if the doctrine of non-importation, and non-contagion, shall be admitted.

Under an abstract view of the question, it is also presumable, and nearly demonstrable *a priori*, that unless the yellow fever, vulgarly and loosely so called, had been in some circumstances contagious, in others not, so much ambiguity and diversity of opinion could not have arisen. No such controversy has arisen regarding the intermittent fevers of the fens of Lincolnshire, and the marshes of Zealand, which are universally admitted to be endemic, and non-contagious ; nor do they vary \* greatly at any intervals of time ; but that they should take on a pestilential form at the season of the year least liable to them, as was the case with the malignant fever of Grenada, would be out of all bounds of probability. And if the fever in question were equally non-contagious, could such doubts ever have arisen ?

\* See Facts and Observations respecting intermittent Fevers, Medic. Chirurg. Transactions, vol. iii. page 1.



Having discussed the presumptive proofs in favor of the foreign origin, and contagious nature of this epidemic, on abstract principles, let us take a review of the matters of fact which can be adduced on the same side of the question.

1st, It has never shewn itself in the first instance but in a sea-port town, and never in the interior of the country, whether island or continent.

2dly, It has, in most cases, been ascertained, that it has made its appearance in that sea-port, after the arrival of one or more ships, either under those peculiar circumstances which engender infection, or conveying the infection from ports where it had already existed. Most of the great epidemics of the West Indies, North America, or Europe, can be traced to one or other of these sources. If it cannot in every instance be traced, this may happen from the want of historical facts: neither does it follow that infection does not exist, though it cannot be traced, for nothing is so subtle as infectious *effluvia*. It is well known, that small-pox



and measles find their way where there is no possibility of tracing the source of the infectious matter ; but will any one deny the existence of small-pox, or measles, in a family, because the source of them cannot be traced ?

3dly, No part of the population of the towns where it has broke out, has been affected, but such as had communication with shipping, directly or indirectly,

The authorities for this opinion, besides the strong ones already quoted, of Dr. Mitchell and Dr. Lining, are the testimonies and writings of Sir James Macgregor, Sir James Fellowes, Mr. Pym, Sir Joseph Gilpin, Dr. Stewart of Grenada, Dr. Gordon, of St. Croix, Dr. Arejula, of Madrid, the French commissioners already mentioned, and many other equally candid, competent, and honorable men, who had the best opportunities of closely investigating the subject.

The doubts have been maintained more plausibly in the West Indies than in the temperate climates, on account of the resem-



blance of this epidemic to the endemic, and sporadic fevers of those colonies ; but the testimonies of Mr. Pym \*, Sir Joseph Gilpin, Don George Juan, and Don Antonio D'Ulloa, and others, go directly to the point, and cannot be invalidated without impeaching the moral character of these honourable men, a species of argument, however, which it is to be deplored, has not in every instance been abstained from by the partisans of this question.

With regard to North America, evidence the most overpowering on this subject is to be met with in the American Medical and Philosophical Register, in four volumes, published at New York, in the year 1814. There are in this work such a multitude of clearly ascertained facts in proof of its foreign origin, and contagious nature, that there is not room here even for the bare recital of them.

\* See a very concise, plain, and satisfactory account of this matter in an article in the 5th vol. of the Med. Chirurgical Transactions, by Sir Joseph Gilpin, who had the advantage of seeing this epidemic in Grenada and Martinique, as well as Gibraltar. But the work of Mr. Pym is the most full and convincing, and the most insuperable objections to non-contagion will also be found in that of Sir J. Fellowes.



My own humble efforts have not been wanting in the same cause. In the year 1798, I wrote a letter to Mr Rufus King, Minister from the States of America to the British Court; and in the year 1805, another to Baron Jacobi, Minister from Prussia, for the information of their respective governments. In these letters, I laid particular stress on what occurred regarding a French ship taken in battle on the coast of America, in May 1795, on board of which this fever, or its infection, was found, and was communicated to the seamen of the British ship Hussar, by the men in health, who were shifted into her from the prize. It is evident that if it could be proved that this fever is communicable from one ship to another at sea, such a proof the reality of contagion would be of the nature of an *experimentum crucis*, there being no possibility of land exhalations to account for it. Such I then considered, and still consider the facts of this case to be. They were, however, so strongly and speciously contested by Dr. Bancroft, as greatly to frustrate the impressive effect which my statement was calculated to produce. The reader will be able to judge of the solidity



of his objections, from an annotation at the end of this work. I feel to myself that I was so far from making too much advantage of these facts, that I might and ought to have availed myself of them still more. I might have adduced them as a very striking illustration of the incompatibility of this disease with a certain temperate degree of atmospheric heat, for the change into cool and pure air, in proceeding to Halifax, did in a very short time first deprive it of its malignity, and then of its infectious nature, so as entirely to extinguish it. The few that were seized, after arriving at Halifax, might have imbibed the poison in the warmer latitudes through which they passed. It was on the strength of such facts as these, that in my conferences with the members of the British Government, and in my correspondence with those of Russia and Prussia, I ventured to assure them, that in none of those countries was there any thing to fear from the importation of this pestilential epidemic, which in the end of last century, and the beginning of this, had so afflicted the West Indies, North America, and Spain, as to excite a general alarm throughout Europe.



There have occurred, since the period alluded to, facts equally conclusive, regarding the communication of this disease, from one ship to another. It will be enough to specify one or two. A French ship of war, the *Palinurus*, lying at Martinique, severely affected with the yellow fever, was ordered on a cruise to try the effect of sea air on the disorder. She fell in with and captured the *Carnation* a merchant ship, on her passage from England, part of the crew of which were seized with the fever while at sea \*. Another French ship of war, in which this fever prevailed both at St. Domingo, and on the passage to Brest, made prize of a merchant ship from the Mediterranean, off Cape Finisterre, and having, without shifting the prisoners, sent a party of their own seamen to navigate her, the crew of the prize caught the fever, and almost all died of it †. The men having been seized on board of their own ship, makes this a stronger case than the other, in which this circumstance is not mentioned. Had they been taken ill on board of the capturing

\* See Dictionnaire des Sciences Medicales.

† See Traité de la Fievre Jaune, par Louis Caillot Dr. en Medicine, Paris, 1815, p. 202.



ship, it might have been said, that it was from the exhalations of the hold or stores.

There is still another useful remark, which I did wrong in omitting in my statement. Of fourteen men sent from the *Hussar* to navigate the prize, nine died before reaching Halifax, a passage of twelve days; the other five were sent to the hospital, where some of them probably died. Now, though it is mortifying to reflect that medical means should not have more controul over this disease, this statement seems to afford the consolation of reflecting that these means are better than none at all. For an opportunity here offered, which but rarely occurs, of ascertaining what are the results of the spontaneous tendency of unassisted nature. There was a mortality of nine in fourteen, and those who were sent on shore, not having had the benefit of medical attendance, at that stage of the disorder at which remedies are most availing, did probably not all survive. This is a rate of mortality far exceeding that of the most unsuccessful practice, even that of *Hippocrates*.



4thly, The effect of quarantine regulations and vigilant police in shutting it out, and repelling its first assault, and the equally effectual and salutary result, of shutting up in the midst of infection. All these good effects have been experienced at Gibraltar, and elsewhere. It has already been mentioned how it was arrested there, *in limine*, in the years 1810 and 1813, and that many families were preserved in the midst of the desolating epidemic of 1804, by cutting off all communication with the garrison and inhabitants. The effect of seclusion at Cadiz, has already been mentioned. The persons in the jail, hospital, and poor-houses of Philadelphia, remained exempt from the pestilential epidemic in its utmost rage, all external intercourse having been prohibited. The same was observed of prisoners of war at Jamaica. The American Register abounds with innumerable and irrefragable proofs of the good effects of seclusion and quarantine; and they ascribe to the more vigilant execution of the regulations of the latter, the exemption from it at New York since 1805, at which time, as well as in 1803, they make no doubt, that it had insinuated itself in



consequence of the loose measures of the quarantine, which, by a singular and unaccountable infatuation of the American Government, had been put under the directions of professional persons, who avowed their disbelief in importation and contagion. It may also be remarked here, that during the whole of the American war, no epidemic occurred, which they ascribe to the intercourse with the West Indies having been suspended.

It has been admitted by one party of the non-contagionists, as already mentioned, that this fever may be imported by ships having foul ballast, or tainted stores. Dr. Bancroft, one of those who admits this, has been at great pains in another part of his work, to shew that no accumulation of filth, however great, and however putrid and corrupt, can produce febrile diseases on shore. It would have been satisfactory, if he had specified in what peculiarity the exception of the holds of ships was founded. I have the good fortune to agree with him, both in thinking that febrile miasmata do not in any case consist in the exhalations of



simple putrefaction, and that fever may be produced by the exhalations from the holds of ships. But I am at no loss in specifying in what the corrupted exhalation of the latter, differ from the former, namely, in their involving morbid secretions, particularly the sordes of the skin, and tainted *effluvia* of the living human body. These may long lie latent, and harmless, both to the crew and passengers, who become habituated to them on long voyages; but immediately affect visitors, on the arrival in port, particularly when the foul materials come to be stirred, as I saw strikingly exemplified in the French prizes at Jamaica. (See Diseases of Seamen, 3d Edit. page 88 et seq.) It forms no objection, therefore, to the importation of a fever being referred to a particular ship, that the fever did not actually exist on board of her at her arrival.

It becomes a curious and interesting, though painful question, what are the grounds upon which this deplorable and mischievous delusion is founded, for every error must originate and rest upon some false and mistaken principles, and these



must have been of a nature uncommonly plausible and seductive, to have won so many partisans of respectable talents, and unquestionable good intentions. Of these the following seem to be the chief.

1st. The great similarity of this pestilential epidemic, to the endemic, and sporadic fevers of the Antilles. The most conspicuous point of resemblance, is the yellow color of the skin. The resemblance, however, is not perfect here; for the color of the former is a dingy orange, in the other two a bright yellow\*.

\* I have elsewhere, (See Diseases of Seamen, page 411.) started a doubt, whether the yellow color was owing to bile, but rather to some *error loci*, or depraved state of the red globules. This color does not appear first in the eyes, as in jaundice. Sir Isaac Newton observes, that the blood reduced to thin *laminæ*, assumes a yellow color. See Optics. B. i. Part 2. Prop. 10. The like color appears in *ecchymosis*, some time after a contusion. The yellowness sometimes does not come on till after death. It seems deducible from these facts, that this color may be owing to the red globules getting into the colorless order of vessels, in an attenuated, or vitiated state, and not to absorbed and circulating bile.



There is another symptom, in which all the three species bear a resemblance to each other; the vomiting of a coffee coloured liquid in the dangerous, and almost hopeless stage. This symptom, however, is by far more frequent in the pestilential epidemic than in the other two. It was so striking and constant in the former, that the Spaniards gave it the name of *Vomito Prieto*, dark colored vomit, when it made its first appearance among them, which was at Carthagená in 1729, as already mentioned, and it has retained that name ever since, though sometimes called *Fiebre Amarilla*. The College of Physicians of Philadelphia, besides mentioning the dusky color as distinguishing it from the endemic, states, that it differs from it also in having no intermissions in the first days. According to the description of Dr. Chisholme, and others, the epidemic is distinguished also by violent pains in the head and legs, a piercing pain in the eyes, generally in one eye, more aggravated sensorial affection, such as coma and delirium, and above all by a greater malignity, that is, a much higher rate of mortality than in the other two species.



The characters of this fever, as distinguished from the ordinary fever of the climate, are depicted in lively colors by the surgeon of the *Eurus* frigate, in his journal, examined officially by me in 1797. In a few days after arriving at Grenada, eighty men were rendered unfit for duty, that is, about a third part of the crew: the eyes swam, as it were, in blood, with excruciating pain chiefly in one eye; the eye-ball started in one; in seven cases, one of the eye-balls was absorbed; in the course of convalescence, some lost their sight, though they retained the substance of their eye. Who will say that this differs in nothing from the endemic and sporadic fever of the Carribean Islands? One of its farther and most material distinctions is, that few of those who have had the true typhous icterodes, or pestilential epidemic, are liable to it a second time. This must be understood under the qualification applicable to all diseases of this kind. Small-pox and measles admit of the fewest exceptions, scarlet fever more\*,  
*smallpox after bac. still more.*

\* Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. iii. page 445. London, 1800.



The principal Spanish author, Dr Arejula \*, speaks positively to this point, and adduces incontrovertible facts in proof of it. The same fact is equally well attested by Sir Joseph Gilpin, Sir James Fellowes, and Mr. Pym, under their own eyes, so that there does not seem to be the possibility of a fallacy. It is established by all the laws of evidence. The multitude of cases, the concurring testimony of persons who had no concert with each other, and no motive but that of truth. It is evident that no such inference could be drawn from a few cases, but the induction is founded on the cases of thousands, and the fact was so well established, that the Spanish Government made a proclamation, requiring those who had passed through the fever in the former epidemic, not to quit Cadiz, but to lend their assistance to the sick; and both Spanish and English selected their nurses from among those who had had it.

\* See his work entitled *Brieve Descricion de la Fiebre amarilla*, Madrid, 1806, page 229. This is a work of extraordinary merit, bearing the characters of great industry, accuracy, and sound reasoning.



Those who deny contagion, bring facts, which seem equally convincing on the opposite side, but they are evidently taken from the endemic cases, or are mere exceptions. And there cannot be a stronger proof than this, of the reality of the difference of the two disorders.

I have not experience of my own, to decide on the various points of difference, for the four campaigns in which I served in the West Indies, were in years comprehended in one of those intervals before alluded to, between the appearance of these great epidemics. The mortality was indeed comparatively very moderate there, during the whole of that war, chiefly owing, no doubt, to there not having been great bodies of land forces transported thither, during that time, the war having been almost entirely a maritime one, and from no ship specifically infected, having arrived at any of the ports on the station. Some mixture of it with the endemic and sporadic, did occur both at Barbadoes and Jamaica; but, from want of predisposed subjects, it did not spread. There was evident proof of it at



the hospital of Barbadoes, and the much greater mortality of medical officers at Jamaica in 1782, gave strong presumption of its existence there. I was not, therefore, impressed with a true conception of its nature, and my language in treating of it, was not so precise, as if I had witnessed its dreadful havock in the following war.

But admitting the symptoms to be ever so similar, it does not follow that they are *identical*. A great proportion of the cases of the true plague, were without the *tokens* or diagnostic characters, and some could not be distinguished from a continued fever; but in spite of this close resemblance, they are essentially different in their nature. Great inconvenience arose here also from their being externally undistinguishable; for we find that in the history of plagues, both in England and France, particularly at Marseilles, there were sharp professional contests between the contagionists and non-contagionists, the latter, pertinaciously denying the existence of it, and sometimes so far influencing the public authorities, that precautions were so long deferred as to al-



low the malady to get beyond the reach of prophylactic means. Will it be said, because there are ophthalmias, resembling the infectious ophthalmia, that there are therefore no such thing as infectious ophthalmia? Has it not also occurred to every practitioner in this country, to see sporadic fevers, when there was not the least reason to suspect contagion, so far resemble the typhous fever from infection, as to be undistinguishable? Those cases of Erysipelas\*, which arise from infection, do not differ in appearance from those which arise spontaneously, but are very different both in their nature and treatment. If it can be proved that the malignant epidemic has a different origin, and greater fatality, to be of a contagious nature, and never, or very rarely, capable of being caught a second time; these are sufficient diagnostics, let the outward symptoms be what they will.

2dly, The doubts, respecting the conveyance of this infection from the West Indies

\* See Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. xxii. page 213.



to North America and Europe, and the consequent doubt of its identity, may have proceeded from the fever in these temperate climates, being more extensive and fatal in its ravages, than in the country from which it is alleged to have been derived. Though it was severely afflicting in the Antilles, it did not attack so great a proportion of the population by far, as in Philadelphia, New York, Cadiz and Gibraltar. A very little reflection will clear up this difficulty. In the West Indies, the susceptible subjects are chiefly the new-come white people from Europe, for a very small proportion of the seasoned white inhabitants, and a still smaller of the creoles and negroes, are affected by it. But in North America and Spain, the whole population is in the same predicament, with regard to predisposition, as the white new-comers in the West Indies, who constitute a very trifling proportion of the whole population.

3dly, They allege, that an infection which vanishes of itself on the approach of winter, in North America, and even of the mild winter of Andalusia, and which does



not spread among the adjoining rural population, is either no infection, at all, or does not deserve that name; that the name of infection can only be applicable to such a disease as the small-pox, which make no distinction of climates and seasons. But in the first place, we can see no reason for believing that all infections are governed by the same laws \*. Variety, is as characteristic a feature of nature as uniformity, particularly in all that relates to organic beings and animal life. Mere analogy can only be held as presumptive evidence, and may serve as a fair ground for rational conjecture and suggestion, but must ever stand subordinate to facts and observation. It is little better than gratuitous assumption therefore, to say, that every infection must conform itself to that of small-pox, and the history of other infections militates against such an assumption. Would it not, therefore, be more conforma-

\* See an extremely judicious arrangement of the varieties of contagion by Professor Hosack, of New York American Medical and Philosophical Register; vol. ii. page 14. Also an article in the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. iii.



ble to reason and sound philosophy, to reconsider, and re-cast theories, in order to make them quadrate with facts, than to strain, suppress, and deny facts, and question the veracity of honourable men, because they cannot be brought to conform to the theory? But if analogy were to decide the question, a strong and obvious analogy might be alleged in favor of this infection being controuled by circumstances of time and place, taken from the history of the true plague. This latter epidemic has never been known either in the tropical or arctic regions, from what cause is not known. I profess myself unable even to guess whether this increase of heat acts upon the poison, by exhaling or decomposing it, or on the living powers of the body, by rendering it unsusceptible of its action. It would be still more difficult to assign a cause for one range of heat being necessary for the existence of plague, and another for the existence of the typhous icterodes. These are matters of pure observation, and so far from being founded upon or suggested by theory, as some anti-contagionists have affected to allege, they baffle all theory; and I can say for



myself, that I am incapable of forming any hypothesis or conjecture on the subject.

But the knowledge of this is by no means necessary. It is the fact only, which is wanted for the present purpose. And it is incontrovertibly established, by the experience of ages, that the existence of plague cannot consist with a heat of the atmosphere, above  $80^{\circ}$ , nor a little below  $60^{\circ}$ . It never fails to disappear in Egypt, at the summer solstice, the heat being then pretty uniformly at  $80^{\circ}$ , or upwards. On the other hand, it appears from the history of all the plagues, of which there is any account in England, that they have never begun to appear, epidemically, but in the end of June, or beginning of July; that they proceed increasing till September, when they are at their *acme*, and then decline till they entirely subside in winter.

The pestilential yellow-fever in like manner, has its own range of atmospheric temperature, but on a higher scale than the plague; for it cannot subsist long, if the thermometer falls below  $80^{\circ}$ . It has never



been known in North America, nor in the South of Europe, but at the season of the year in which tropical heats, that is of  $80^{\circ}$ , or upwards, prevail ; and it has never failed to disappear in winter, even in the mild winter of Spain, as already remarked. Nor has it ever appeared in those parts of Europe, where the summer heats do not rise to the tropical pitch. It has never been known in France, nor England, and but once in Portugal. Its range has been circumscribed to Cadiz, in latitude  $36^{\circ} 25'$ , the South coast of Spain, as far as Leghorn, in Italy, in latitude  $43^{\circ} 33'$ , the most northerly point which it has reached as an epidemic.

4thly, It has been farther argued, by the non-contagionists, that, if it were really contagious, it would spread from the seaport towns to the adjacent country, which it did not, either at Grenada, or in North America. If it can be made good, as a matter of fact, that it is actually infectious in these towns, this argument can be of no avail ; and it will be for those who delight in speculation, to exercise their ingenuity in accounting for it, by inventing some theory



that will tally with the fact. But here too, we are not without the countenance of analogy, for something very like this happens in the plague. This great epidemic has a wonderful attraction to dense population, and squalid habits of life. It is this, together with the neglect of precautionary means, which at all times fosters, and perpetuates it in the Mahomedan towns; and in London, it always fell first on the indigent, and ill-aired quarters of the town. The Lord Chancellor Clarendon \*, relates in the history of his own life, that when he quitted London, with the court, in the plague of 1665, and returned next year, he missed very few of his friends and acquaintances, who remained behind, the ravage having, in a great degree, been confined to the lowest orders of the people, who, in that age, lived in circumstances of great filth, and foul air †; and I am tempted to think,

\* See Medical and Chirurgical Transactions, vol. iv. p. 102.

† Since this work was put to the press, a general officer, who served in the Egyptian campaign of 1800, and was President of the Board of Health, informed the author, that it is a fact fully established, that the Bedouin Arabs were never known to be affected with the plague,



that the exemption of this city from plague ever since, has been owing to the improved habits, which began on occasion of the great fire, the year after the plague, and have rapidly gained ground ever since. The commerce, and general intercourse with the Levant, as well as all other parts of the world, has been so much greater than in former ages, that it is difficult to believe, that particles of infection have not at various times been imported, but they have failed to take effect, for want of a suitable *nidus*, or fuel, as it were, to foster and kindle them.

though they take no precaution against it, and use with impunity, the clothes of those who die of it. This can only be accounted for by their never living in houses, and by their being in constant motion and change of residence. The general very sagaciously availed himself of this knowledge, and contributed much to the extinction of the plague, by ordering a frequent shifting of the ground of the camp.—He resided some time in Brazil, and informed me, that, though Rio Janeiro, and the country around, have all the requisites of marsh miasmata, thick woods, and human effluvia, yet fevers, both epidemic and endemic, are there unknown; another proof of the capriciousness of morbid causes.



5thly, They still farther allege, that it is contrary to the common course of nature, that a disease, without any adequate or assignable cause, should thus visit these islands, at such long and uncertain intervals. The same arguments will apply to the visitation of hurricanes; for, it is beyond the reach of human sagacity, to say, why these islands should be more liable to them than any other portion of the globe; or why they should return at such uncertain and unequal intervals: far less can our philosophy detect upon what modifications of the atmosphere these convulsions of it depend. But shall we therefore deny the reality of hurricanes? This pestilential fever is the hurricane of the human frame, equally uncertain in its recurrence, equally dark and inscrutable in its cause, equally and deplorably certain as to the reality of its existence; but unequal as to its powers of destruction, if this is to be measured by the loss of human lives, having caused a greater waste of the species, than all the convulsions of nature put together, hurricanes, earthquakes, and inundations. Is it to be endured, that, by a piece of cavilling sophistry, a monster shall



be unchained, which, in the course of a few years, has devoured more than half a million of human victims?

But if any one should persist in the following declaration, "according to my definition, and the language I adopt, I do not apply the term contagion to any thing, but what has that property, in all circumstances of climate and seasons, of purity and impurity of air;" the rational answer will be, "Be it so, but are you, in order to indulge yourself in this miserable play of words, to sport with the lives of thousands of your fellow-creatures." It will hardly be believed, that, in this age, there are persons pretending to medical education and science, who actually argue on no better principles, that the plague itself is not contagious. I am told there is a work of two quarto volumes recently published, the chief object of which is to maintain this extraordinary doctrine: and a work has lately been published at Strasburg, the object of which is to disprove the contagious nature of the venereal disease. No rational reader will expect the author to enter into a serious refutation of



such disgusting and extravagant paradoxes. We are compelled to apply to physicians, what one of the ancients has said of philosophers, *Nemo ægrotus quicquid somniat tam nefandum quod non aliquis dicat philosophus*; but with this material difference, that the conceits and absurdities of philosophers, are generally harmless; whereas those of physicians, may draw along with them the most serious and fatal calamities; and it appears how important it is, to have devoted a section of this work to the abuse of words; and how apposite the present subject is as an exemplification of this abuse. We have endeavoured to find some excuse for the like error, with regard to the yellow fever; but neither will the experience and common sense of mankind, here surrender themselves, to any profusion of bewildering words, however confidently and imposingly pronounced, and spread through some thousands of pages. What can be so affecting and humiliating, as that persons of the plainest understandings, should form sounder judgments on these important points, than those who value themselves on scientific attainments and research? Will not the world,



without judging with its usual severity, be disposed to regard these our boasted attainments and researches, not as the legitimate lights which guide us in the avenues to truth, but as false lights, leading *us* into error, and *them* into danger; and tauntingly pronounce of our learned labours, that they only teach us *insanire ratione modoque*. Nor can it be matter of indifference to those, who feel for the interest and dignity of the profession, that any of us should become objects of disrespect, and be exposed to the sneers of the extra-professional part of the community, by falling into errors, which lie open to the detection of the most ordinary and uncultivated minds.

But, according to the strict principle upon which this question ought to be decided, all these reasonings are absolutely nugatory, and only a waste of time; for, if it can be made out by legitimate evidence, as a matter of fact, that this epidemic has never taken its rise but in seaport towns, where it can, in most cases, be traced to the arrival of shipping conveying infection; that it has never spread, but by



contact, or near approach to the sick, and that it can be excluded by quarantine and separation from the sick, then all discussions become vain regarding the resemblance of its symptoms to other diseases, or the previous state of the ship which imported it, or its communication being limited to a given temperature of the air, or to a given susceptibility of subjects; and whether it is conveyed in the holds of ships, or on men's persons, and whether there is a marsh or no marsh in the vicinity, the simple question being, whether the present disease is communicable from one person to another. It is only the matter of fact, as established by evidence, with which those civil and military authorities have to do, to whom is entrusted the sacred charge of the public health. These authorities perceiving the jarring opinions of medical men, may, without deference or reference to them, undertake to judge for themselves, on a point to which any man of good sense and understanding is competent, as it hangs upon matters to be decided on by the rules of evidence, not involving professional knowledge. And in case these authorities should wish for assessors to sit in judgment with them, they will probably deem it more



safe and advisable, to ask the assistance of some members of the bench or the bar, accustomed to weigh evidence, and investigate facts, or even of such plain men as compose juries, than medical men, having so much reason to suspect, that our minds are warped by prejudice, with our heads so overcharged with learning as to leave no room for common sense ; and so over-heated with contention, that we are more intent on victory than solicitous about truth.

In this view of the subject, the whole case is comprised in the following questions and answers.

1st, Has this malignant epidemic ever arisen, except in sea-port towns? No.

2d, Has it ever arisen in these towns but when it can in almost every case be traced to the arrival of one or more ships, under peculiarly suspicious circumstances, regarding the presence of infection either engendered, or transmitted? No.

3d, Has this disease ever arisen or spread



but by communication with the sick, or the matter of infection? No.

4th, Have quarantine regulations been found an efficient means of protection against it? Yes,

5th, Has separation from the sick, their clothing, bedding, &c. been found an effectual security against it? Yes.

The reader will judge for himself whether the facts adduced in the course of this enquiry, will justify these answers.

This discussion has been introduced as an exemplification of the perverted use of language; but there are causes of a moral nature, which have had no small share in sustaining the doctrine here impugned.

Every one acquainted with human nature, knows how difficult it is for the mind to extricate itself from the shackles of prejudice, when rivetted not only by time, and habit, but by that pride of opinion, which confirms, and perpetuates self delusion, in



opposition to the clearest evidence. But it is the duty of every liberal and considerate man to observe forbearance, and to judge with indulgence of weaknesses from which none of us are exempt. Gentlemen, whom I know to be men not only of the greatest integrity, and honor, but of superior attainments, have advocated the cause of non-contagion, *bonâ fide*, and with intentions, as pure as those of the opposite opinion. Nor is it easily conceivable, that any set of men, far less those of the medical profession, can be otherwise than sincere, or that they can mean positive mischief. If there have been persons who have stooped to the suppression of facts, and even to connive at perjury, as has been alleged, I should not impute even this conduct to bad intentions, but to that misguided conscience, which by a perverted casuistry, deems pious frauds to be justifiable, and holds that a *little evil* may fairly be done, in order that a *great good* may come of it. All this ought to serve as a warning to the junior members of the profession against embarking hastily as the partisans of any doctrine, particularly such as involves some of the dearest interests of so-



ciety, for in the course of time, they may be called upon to engage in a struggle, in which no man ought to be confident of his powers, namely, that severe ordeal of human virtue, by which they may be required to sacrifice the pride of opinion at the shrine of conscience by an abjuration of error.

It is obvious, from what has appeared in the course of this discussion, that an accurate and ample history of a disease is requisite, for the purpose of prevention, as well as for that of cure. The errors we have been adverting to, have arisen in the first instance, from the want of a sufficiently comprehensive knowledge of the subject. It was viewed on a local, partial, and narrow scale, instead of being surveyed in its whole extent, and various bearings. Accordingly, we see, that as knowledge has accumulated, error has vanished. In America, during the last years of the last century, the majority of opinions were in favor of non-contagion, and even public medical bodies gave their opinion on this side ; but in the year 1805, the College of Physicians of Philadelphia, as a body, gave their opinion in favor of conta-



gion, asserting that the evidence of this was as strong, as for that of the plague ; and almost every member of that of New York, has publicly given the same opinion : such the meliorating and maturing influence of time. The College of Physicians of London have given their verdict on the same side. *Commenta delet dies.* But such zeal has of late been exerted, and not without success, still to prop the opposite opinions, as to induce the auther to dwell upon the subject so fully and earnestly.

The question seems now to be brought to such a point, that we may venture to challenge any candid, intelligent, and unbiassed man, whether in or out of the profession, to open his eyes, and deny that this disease is contagious ; and if it be not, then has the author of this discussion lost every faculty of distinguishing truth from falsehood, of discerning light from darkness.







## SECTION VII.

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### THE FALLACY OF TESTIMONY.

LASTLY, Since the life of any individual, however long it may be, and whatever industry and sagacity may have been employed in the course of it, is greatly inadequate to the task of collecting a stock of knowledge sufficient for professional practice; and since he must be indebted to others, whether his cotemporaries and countrymen, or belonging to distant ages and countries for a large share of necessary information, it is obvious that the value of such information must depend on the credit due to the authors, and on the clearness of the language in which it is couched. Besides the sources of fallacy and error, already enumerated, the medical practitioner has to



encounter those, which spring from the credulity, ignorance, vanity, self-interest, self-delusion, the love of controversy, the want of candour, and the inveterate prejudices of those who report facts. It is a melancholy truth, that there is perhaps no department of human knowledge, in which there is so great a want of correctness, with regard to recorded observations, as well as reasonings. We ought, therefore, to be strongly fenced against the inroads of error in others, as well as ourselves.

It was a favourite saying of Dr. Cullen, that there are in physick more false facts, than false theories. It is by the want of due caution with regard to these, that quackery has chiefly been sustained; for those who do not belong to the profession, being off their guard, from not being in the habit of observing, and reflecting on the fallacy of testimony, and other sources of error; and being anxious to catch at relief, from whatever quarter, perhaps with minds soured by disappointment, and exquisitely sensitive to hopes and fears however vain. These impressions are also wonderfully favoured by



the operation of mystery and concealment ; for, by some principle of human nature, not easily explicable, there is a peculiar interest and importance attached to whatever is secret. *Il y a quelque chose de singulièrement piquant, dans le mystère*, says some French author. The credit of these remedies, is also greatly enhanced by the successful cases only being made public ; for the innumerable cases in which they are used, whether openly or secretly, without the boasted good effects, still more, if with bad effects, are never reported. And here is another source of false or dubious testimony ; for those who are induced to use these remedies, being anxious to ward off reproach or derision, justify themselves by making the most favourable report, and even by affecting to have received relief. It is farther a curious, and well ascertained fact, that no nostrum has, in any instance, maintained its character, after it was revealed. This was strikingly exemplified in the case of Ward's various remedies, which went entirely out of vogue, the moment they were published, which was done after his death, by an injunction in his last will.



And it is still farther in proof of the capriciousness of the world \* at large, on medical subjects, that all his remedies are excellent compositions, and, under the exercise of discretion, well adapted to the diseases which they professed to cure. Indeed, the injury done to the world by secret medicines in general, is not so much from any thing pernicious and inefficient in their nature, as from their indiscriminate use, and the false confidence they inspire to the exclusion of other, and better remedies. On the contrary, it is presumable, that it must have been from some eminent good effects observed from them, that the authors of them were first induced to offer them to the publick.

There is another source of error arising

\* I perhaps have wronged *the world at large*, and should have confined the remark to England. Professor Nemnich, of Hamburgh, in a narrative of a tour he made in this country, about twenty years ago, calls England the Paradise of Quacks, and enumerates quackery, among the national peculiarities, in the same list with boxing, horse-racing, bull-baiting, and cock-fighting.



from the delusions of patients, who, without any intention to deceive, permit themselves to be deluded by extreme anxiety of mind. It is impossible to set bounds to the power of imagination in creating sensations excited without the presence or operation of any actual corporeal impressions. I have frequently seen simple and ignorant persons, when under the false apprehension of having caught a certain impure disorder, tormented with real pains in various parts of their bodies, particularly their loins and noses. I have been assured by those, who have been in circumstances of exposure to the infection of the plague, and in momentary dread of catching it, that they have felt acute pains in the groins and arm-pits, these being the parts known to be most prominently affected in that epidemic. More familiar, though less aggravated examples of this, occur among those subject to hypochondria. Enthusiasm and mania give rise to all the diversified forms and gradations of the same species of delusion.

The value of that recorded knowledge, which rests on testimony, is also greatly



impaired, by the difficulty of ascertaining the exact import of the terms, by which the ancients, and even the earlier writers among the moderns, designate the remedies they employed, whether simple or compound. Of the simples mentioned by the ancients, very few are now recognizable. If we except opium, aloes, and perhaps one or two more, it is doubtful, whether there is a single article of the ancient *materia medica*, which can be satisfactorily ascertained; and for want of such knowledge, much valuable practical instruction has been lost. This will be best illustrated by an example. A secret medicine, under the title of *Eau medicinale de Husson*, was introduced into this country, one of the first years of this century, as a remedy for the gout \*, and it was found peculiarly beneficial in a great number of cases, particularly in that of Sir Joseph Banks, President of the Royal Society. A few years afterwards, it was disco-

\* See a clear exposition of the history and virtues of this medicine, in a work entitled "An Account of the remarkable Effects of the Eau Medicinale D'Husson, on the Gout." By Edwin Godden Jones, M.D. London, 1810.



vered that a vinous tincture of the *colchicum autumnale*, had the same virtue, and little doubt was entertained of the identity of this, and the secret medicine; and Sir Joseph Banks, among others, has experienced exactly the same effects from both. About the same time, a passage was noticed in the works of Alexander Trallian, a physician who practised in Asia Minor, in the 4th century, ascribing similar effects to a medicine, called hermodactyls. This was naturally supposed to be the same with the colchicum; but as there was no description of it in any ancient author, this was merely matter of conjecture, till enquiry having been made at Constantinople, it was discovered, that there was an herb still bearing that name in the vicinity of that city, and specimens of it being transmitted to Sir Joseph, it was actually found to be a colchicum. Here was a most valuable piece of knowledge, lost to the world for many ages, from the want of a description of the article, which therefore could not be recognized from one age and country to another. And we see what incalculable advantage must result to medicine in general, from the cultivation of



natural knowledge, from this consideration, as well as others already stated; an advantage peculiar to the present age, for never till now, have the various objects of natural history, particularly of botany, been so described, that in all ages to come, however remote, no ambiguity can arise, regarding the identity of the remedies recommended, among the medical practitioners of this, or any future age.

From the like consideration, we see a reason for the adoption of the scientific and systematic terms, in the *materia medica* and pharmacopœia, in preference to those loose and trivial names, of which the true import could never be ascertained by our posterity. But, for the like reason, the terms which have been abolished, and which have passed into desuetude, should be kept upon record, with explanations annexed to them; for there are many of the titles of medicines, especially of those that are compound, which are already so far forgotten, that much of the practical knowledge, contained in the works of the authors of the 16th, 17th, and even in the beginning of the 18th century,



such as Hoffman \*, is nearly lost. Their titles are so obscure and quaint, as to convey no knowledge of their ingredients, and it is next to impossible, for a common reader, to find a description of them, to serve as a key to the author's meaning. It would add greatly to the value of pharmacopœias, if a glossary to these compositions were annexed to them, in place of studiously avoiding all mention of obsolete terms, as is the custom in these works.

On this subject, it is necessary to beware of scepticism, as well as credulity. The numerous instances in which boasted remedies, and plans of cure, proposed by practitioners, are found to fail in the hands of others, produce a fastidiousness, which is occasionally carried to excess. It has already been remarked, that from the diversity of constitutions, the same remedies will not universally succeed. If a medicine therefore, newly recommended, on respectable authority, should

\* There is a translation into English, of the practical parts of this author, which, for want of a glossary for the compound medicines, is of little value.



not be found to answer to its character, on its first trial, it should not be abandoned. I can exemplify this in myself. About fifty years ago, the volatile tincture of *guaiacum* or *tinctura guaiaci ammoniata*, in doses of half an ounce, was proposed confidently by Dr. Dawson, on his own experience, as a remedy in acute rheumatism, to be administered after the fever had been lowered by the use of some evacuating medicines. My first trials of this practice were so discouraging, that I laid it aside for several years, and the more readily, that it did not seem very consonant to reason, that a medicine, so stimulant, could be adapted to a disease in which there was so much heat, and excitement: but on returning to its use, I found that there were cases, in which it succeeded to my utmost wish. It seemed to be best adapted to the habits in which there was scrofula, or a constitution allied to it.



## CONCLUSION.

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I HAVE thus exhausted the enumeration I made of the various causes which obstruct, or retard, the progress of practical medicine, namely, the errors and abuses arising out of false or misapplied theory; the great diversity observable in the constitution of individuals; the difficulty of appreciating the efforts of nature, and of discriminating them from those of art; superstition; the ambiguity of language; and the fallacy of testimony.

It will probably not have escaped the intelligent reader, that there are two other causes, neither uncommon, nor unimportant, which should not have been omitted in this enumeration. These are, the inveterate attachment to preconceived opinions, and the



excessive devotion to authority. The reasons for these omissions, were, 1st, that the purpose of this discussion was to advert only to those causes which are peculiar to medicine. These two are common to all other branches of knowledge, except perhaps mathematics. 2dly, they have already been so fully adverted to, incidentally, as to render it unnecessary any further to enforce, or exemplify them. With regard to the pertinacious adherence to inveterate opinion in the face of evidence, the discussion on the yellow fever is so illustrative, as to require no farther comment. And with regard to the excessive deference to authority, the history and fate of Boerhaave's system, affords a full and satisfactory exemplification, how doctrines, the most hollow and fugitive, can be established and sustained for a time, by the talisman of a great name.

From the picture that has been exhibited of the innumerable doubts and difficulties, which clog the attainment of medical knowledge, and embarrass the application of it to practical purposes, the timid, sceptical, and indolent, may be discouraged from studies



apparently so arduous in their prosecution, and so questionable as to the efficiency, and utility of their result. But it is not from characters of this description, that any good can be expected in any of the useful arts of life. If a like despondency were to pervade mankind in general, there would be an end to all that enterprize and energy, which alone can enable them to act up to their destiny, and follow up those pursuits, upon which the perfection of their nature depends. As the senses would have lain dormant for ever had there been no external objects to stimulate them, so the faculties and virtues, which characterize rational nature and civilized life, could never have been developed, but through the excitement of those pains, wants, difficulties, and dangers, inseparable from human life. No other arrangement could have bound together our duties, our happiness, our bodily, and mental perfections, in one harmonious and consistent system. Let us compare the art of medicine, under this aspect, with those of navigation and agriculture. If man had been furnished by the Creator, with wings, by which he could have traversed all seas



and oceans, so as to supersede the use of ships, where would have been that hardihood of character, and all those ingenious devices, which have called forth the active energies and deep researches of the human mind? If contrary to the actual institutions of Providence, the life of man had been sustained by the spontaneous productions of nature, instead of the products of industry, neither the faculties of the mind, nor the powers of the body, could ever have been developed, man would have been little superior to the brutes: his active and inventive energies, would have lain asleep for ever, there would have been no room for talents exercised in productive industry, and commercial intercourse, all the mutual and endearing ties, and dependences of social and civilized life, all trades, professions, arts, and sciences, whether ministering to accommodation, or elegance, constituting man's greatest felicity, whether as objects of pursuit, or enjoyment, would have been unknown, and untasted.

It is obvious, that this reasoning being founded on a law of Nature, must apply equally to Medicine. In a probationary



existence, it was necessary that man should be tried, not only by pain and sickness, but by the difficulties of remedying them, as exercises of virtue and ingenuity: Why should the road to medical relief lie through fewer and lighter struggles and dangers, than those of navigation and agriculture? but the subject is more concisely and emphatically illustrated by the philosophical poet, than by any amplitude of illustration, or farther multiplicity of words which I could employ.

———— Pater ipse colendi (*medendi*),  
Haud facilem esse viam voluit, primusque per artem,  
Movit agros, (*ægros*) curis acuens mortalia corda.







## NOTE

REFERRED TO AT PAGE 158.

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ON the 15<sup>th</sup> of May, 1795, the Thetis and Hussar British frigates, cruizing off the Capes of Virginia, fell in with five French armed ships, and after a severe action of an hour and a half, three of these struck their colors: but from the disabled state of the frigates, only two could be taken possession of. They proved to be the Prevoyante and Raison. The latter had been employed at Guadaloupe as a prison ship, a situation of all others the most likely to engender and harbour a stock of infectious filth. It was found, on boarding this ship, that there had been great sickness and mortality among the crew from the yellow fever since she sailed, which was on the 25th of April. Great care was taken in shifting the prisoners on board of the Hussar, to remove



only those in perfect health. It was found, that notwithstanding this precaution, the same fever began soon to spread among the crew of the frigate. On the 28th of May, the frigates and their prizes arrived at Halifax, but only a few of the sick were landed, for the inhabitants taking alarm, least the infection should be introduced into the town, it was determined, after a consultation of naval officers, that the remainder of the sick should be landed at some miles distance from the town, and accommodated in tents, and that the ships themselves should be put in quarantine. The like precautions were taken with regard to the sick prisoners. The number of sick landed from the Hussar, under the care of the surgeon of the ship, and his assistant, was eighty-three, all of whom recovered. The calamity which befel the men sent to man the prize, has been stated in the former part of this work.

The truth of this statement was denied by Dr. Bancroft, in a work published in 1811. His arguments are grounded on discoveries which he alleged he had made in documents deposited in the public offices in London. He perceived in the



muster-books at the Navy Office, that the whole of the prisoners' names were entered as victualled on board of the Hussar, to the number of 116; and that prize-head-money had been paid for the same number. From this he inferred that the whole of the French crew had been brought on board of the frigate; that there was not only no yellow fever, but no illness whatever among them, either on board of their own ship or in the frigate, for he understood that no men could be victualled but what were actually on board, and that all sick men were checked for their provisions; concluding from all this, that the whole story of the yellow fever was a fable, invented by the surgeon of the Hussar.

Having myself served for four years as physician to the largest fleet that ever was employed on foreign service\*; and, having in that time been present in six general

\* The ships of the line, engaged in the action of the 12th of April, 1782, was thirty-six, the greatest number in any naval battle recorded in the annals of England. There were four besides on the station, not present in the battle. See Schomberg's Naval Chronicle.



actions, these statements and assertions, so contrary to what I understood to be the rules of the service, greatly astonished and startled me. But least my recollection should have failed me, or least there should have been some change in the regulations, I addressed a letter to the Chairman of the Victualling Office, who having been a naval officer himself, was well acquainted with the practical rules, as well as the official business of the service. I also sent Dr. Bancroft's book to the office, begging them to make a report on that part of it which related to this subject. The report is in the following words.

“ Prisoners of war of every description,  
“ whether wounded, sick, or well ; whether  
“ remaining on board of their own ships,  
“ or transferred to the capturing ship, are  
“ entered on the books of the capturing  
“ ship, for two thirds allowance of provisions. There is no exception to this,  
“ unless when a prize is taken so near an  
“ English port as to be brought in the same  
“ day. It seldom happens that all the prisoners are shifted, not only the sick and  
“ wounded, but generally some of those in



“ health being left on board to assist in  
“ working the prize, in consequence of the  
“ few men ships of war like to part with on  
“ these occasions. If the prisoners should  
“ fall sick, they would not be checked for  
“ their provisions, and no sick on board of  
“ their own ships are ever checked of the  
“ allowance by the crown.

“ From this it will be clearly seen, that  
“ the information obtained by the person  
“ alluded to, has been erroneous or mis-  
“ conceived.

“ P. S.—Muster-books are not to be con-  
“ sidered as infallible, as on occasion of  
“ captures, great confusion arises about  
“ mens' names, particularly of the prisoners  
“ left in the prize, who are likely to be alto-  
“ gether omitted.”

It was therefore, on information, in every particular erroneous, that Dr. Bancroft ventured to charge Mr. Wilson, surgeon of the Hussar, with falsifying those statements upon which I grounded my argument in favor of the contagious nature of the yellow fever.



There are a number of other inaccuracies of inferior importance charged upon him ; and also on the surgeon of the hospital, to whom Dr. Bancroft will not allow even the faculty of eye-sight in discovering the color of the men's skins. Many of these are likely to be real ; and still more industry might have furnished still more aliment for captious cavilling ; particularly had Mr. Wilson so far forgot what was due to himself, as to have submitted to the endless humiliating interrogatories put to him. It would be strange, indeed, if in ordinary circumstances inaccuracies did not occur on such occasions from hurry and confusion, but when the dismay of pestilence was added to that of war in some of its roughest forms, it would be incredible were any one to assert that inaccuracies had not happened. It does not seem to have occurred to Dr. Bancroft to make any allowance for these circumstances.

I ought not to omit, that soon after I heard of the truth of these transactions being denied, I called on Captain (now Sir John) Beresford, who commanded the Hussar



He assured me that Mr. Wilson's statement was correct in every point; adding, that having had the yellow fever himself, he well knew what it meant. He informed me farther, that, after the sick were sent to the tents, he himself, his officers and men in health, went also on shore under tents at a prudent distance from the sick, and remained there till the ship was thoroughly cleansed, purified, and fumigated, the whole ballast being taken out, and the hold swept.

What now becomes of the vaunts of our author, exulting in the victoriousness of his cause \*, and of the many encomiums of his

\* This author's opinion of himself and his cause, will be readily inferred from the following copy of the introductory sentence to the sequel to his Essay on the Yellow Fever, published last year.—“ In the year 1811, “ I published an Essay on the Yellow Fever; and, by “ *facts not to be invalidated, while truth continues to* “ *be invariable*, proved it to be no other than an aggravated form of that multifarious disease, which is well “ known to result from the action of those exhalations “ commonly denominated marsh miasmata, though “ often extricated from soils and situations, which “ are not marshy, &c.”



partisans, proclaiming his statements to be infallible, and his arguments to be unanswerable? Were we to judge him with one half of the severity with which he has judged others, or were we to take this as a specimen of the reasonings with which his voluminous writings are swelled, what opinion should we form of the correctness of his facts and the accuracy of his research? I do not mean to arraign Dr. Bancroft of that wilful falsehood which he so unsparingly imputes to Mr. Wilson and others. He merely believed what he was told by a clerk in office, as ignorant as himself of the practical usages of actual service. I have here no personal exceptions, much less any hostile feelings. He has only questioned my accuracy and judgment, points of criticism, upon which all publick men are fairly accountable. He has been pleased even to mention my name with respect. But I here earnestly deprecate, nay, loudly protest, in my own name, and that of all those who value the dignity and the respectability of the profession, against those arguments on professional subjects, which consist in aspersions on moral rectitude:



character being a possession dearer to every man of good principles and sentiments, than life itself; and calumny being, of all wrongs, the most difficult to repel, and that which admits the least of atonement or redress\*.

In order to obtain still farther information on this matter, I inspected the Captains' Journals of the *Thetis* and *Hussar*, deposited at the Admiralty Office; and also the

\* The high value here set upon honest fame, is no extravagant and romantic notion, derived from the ages of chivalry. The justness of the sentiment can be maintained not only on ancient classical authority, but it has the sanction of Holy Writ. The Roman satirist says,

Summum crede nefas animam præferre pudori,  
Et propter vitam vivendi perdere causas.

And we find from the etymology of a scriptural word, (not I believe to be found in any profane Greek author) the horror, in which the depriving any one of his good name ought to be held. *Διζέολος* derivatur a *Διζέάλλω*, calumnior.—This attribute is, I believe, by many serious persons deemed not to be the worst which enters into the character of this malignant being; but it would appear that the Evangelists thought it one of the most descriptive, by framing this new word by which to designate him.



account of the engagement, as detailed in the London Gazette, of the 27th June, 1795. In the Captain's Journal of the Thetis, the number of prisoners in the Hussar, is stated at 130. In the Captain's Journal of the Hussar, they are stated at 125. We have seen, that the number stated in the muster-book of the Hussar, and of the charge for head-money, was 116. In the victualling-books, they are stated at 117.

Incorrect, and apparently inconsistent, as these various statements may be, they nevertheless admit of the following explanation.

The Thetis being the Commodore's ship, all the important documents belonging to the prizes, would be carried on board of her, and among the rest, the *rolle d'equipage*, containing the names and number of the men on board, when the ship took her departure from Guadaloupe, which was probably 130. The number on the Captain's Journal of the Hussar, was probably taken from the number, when the ship went into



action, as nearly as they could ascertain, this being an important point, for it is the number on which head-money is strictly due: this was 125. The number on the muster-book, 116, was the number of survivors after the battle; part of whom, as usual, would be transferred to the victor, and part left behind, but all equally entered on the books, as already stated. Among those left behind, would certainly be the wounded, for that there were wounded, as well as killed, it is impossible not to believe; for, it is stated in Captain Cochrane's letter, in the London Gazette, that the frigates did not open their fire, till they were within half-musket-shot. And there is this farther proof of the closeness and severity of the conflict, that the wounded bear a smaller proportion to the killed, than in any action I ever knew: the former, being in the *Thetis*, nine; and the latter, eight. The proportion of the wounded to the killed, is a sort of criterion of the distance of contending ships, on account of the number of splinters from spent shot, when they are less closely engaged.



I should say, therefore, that the difference between the number at departure, 130, and the number in going into battle, 125, was the number of those who died on the passage, being five; that the difference between the number which went into action, 125, and the number on the muster-book, 116 or 117, is the number killed in action, being eight or nine. With regard to the head-money, it is evident, the captors had not what was due to them, which was probably owing to its having been paid long afterwards in London, and the number being taken from the muster-books at the Navy-Office.

But it may be asked, Why all this discussion regarding historical incidents? Is there not sufficient internal evidence in this case, to repel the charge? For, is it conceivable, that Mr. Wilson, whose character is that of a man of honest simplicity of mind, could, without any assignable or adequate motive, become at once so flagitious, as to contrive such a systematick machination; or had he been capable of conceiving such a purpose, would it have been possible for him to



have blinded and deluded so many enlightened persons, professional and unprofessional?

It now only remains for me to vindicate myself, if I can, from an obvious charge which may be brought against me. It may fairly be asked, why, with my convictions on this subject, I should for seven years, have suffered the world to be imposed on, in a point of the highest importance, by statements, the fallacy of which I had ample means at any time of demonstrating.

In the first place, from my utter abhorrence of professional controversy, and having, through life, never publicly noticed the various obloquies, misconstructions, and contradictions, to which every man, in the exercise of his public duties, is exposed, I was in hopes, at the end of my life, to be able to say, that I had never, through the medium of the press, taken part in any professional contention. I was perhaps vain enough to think that my character might have proved some sort of shield and answer to them.



If it should be said in reply, that this may be a very good rule in the case of private injuries; but in cases, where the best interests of society, and the lives of myriads are eventually at stake, there is no room for pleading indolence and the love of peace, far less an over-weening conceit of character. To this I could only rejoin, that I trusted, that before long, the native force of truth would operate and prevail, especially as the cause was supported by advocates more able than myself. And I believed a few years ago, that this golden era had actually arrived; for it did not occur to me, how any doubts on the subject could possibly remain on the mind of any rational being, after the publications of Sir James Fellowes, Mr. Pym, Sir Joseph Gilpin, and others. But I was mistaken. Much as I had studied the human mind, much as I have striven to measure the extent, as well as variety, of its aberrations in all their dimensions, I had not, it would appear, duly fathomed the length and breadth, nor sounded the depth of self-delusion, and the pertinacious pride of opinion; for, within these few months, there have appeared publications, as full of sectarian zeal, of plausi-



ble and imposing language, as at any period ; and I am well assured, that they have even made converts. I should as soon hope to argue a good Catholic out of his belief of transubstantiation, as to change the opinions of the leading partisans of this doctrine. But I have felt an imperious obligation no longer to defer my best efforts, in stemming the torrent of what I consider as mischievous errors ; and in case the apology I have offered for my delay should not be deemed satisfactory, I am ready to make the *amende honorable* for this neglect of my public duty.

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ERRATA.

- Page 106, line 2d from the bottom, insert between the words *one* and *noviciate*, the following words, *in serving his*  
 109, in the Note, *for* heilhund, *read* heilkund  
 119, line 4th from the top, in place of *for* Nature, *read of*  
     Nature  
 129, line 12th and 13th from the top, *for* preventive, *read*  
     perverted  
 135, sixth line from the bottom, *for* lowest and vulgar, *read*  
     lowest vulgar  
 166, after the word *more*, in the last line, insert " Small  
     pox after vaccination still more"  
 205, first line, *for* 1st of May, *read* 16th of May.



the and imposing language, as at any  
period; and I am well assured, that they  
have even made converts. I should as  
soon hope to fight a good Catholic out of  
the belief of transubstantiation, as to change  
the opinions of the leading partisans of the  
doctrine. But I have felt an imperious obli-  
gation no longer to defer my best efforts in  
denouncing the torrent of what I consider as  
mischievous errors; and in consequence  
have I have offered for my dear country  
and its honored religion, I am sensible the  
will be made honorable for this neglect  
of my public duty.

ERRATA

Page 100. line 25. for the bottom line, read  
and corrected, the bottom line, read  
100. in the text, for bottom, read middle  
101. line 10. for the word, read bottom  
102. line 10. and then read, for the correction, read  
103. line 10. and then read, for the correction, read  
104. line 10. and then read, for the correction, read  
105. line 10. and then read, for the correction, read  
106. line 10. and then read, for the correction, read  
107. line 10. and then read, for the correction, read  
108. line 10. and then read, for the correction, read  
109. line 10. and then read, for the correction, read  
110. line 10. and then read, for the correction, read



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THE END.



