In what way is the action of drugs to be discovered? : an essay / by William Sharp, M.D.

IN WHAT WAY IS THE ACTION OF DRUGS TO BE DISCOVERED?

AN ESSAY,

BY

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"It is a part of science to make judicious enquiries."

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THE ACTION OF DRUGS.

This subject comprises several questions, among them the following:—

I. In what way is the action of drugs to be discovered?
II. What is the action of drugs?
III. How is the action of each drug to be distinguished from that of all others?

Part. I.

In this Essay the first question is considered:

In what way is the action of drugs to be discovered?

Nothing more clearly betrays the extent of the weakness and infirmity into which the human mind has fallen than the perverted ingenuity it has shown, and the mistaken ways it has followed, in respect to the ends at which it has aimed.

On most, if not on all subjects which man's mind has attempted to investigate, many erroneous methods, perhaps all possible ones, have been pursued before the true method has been found.

We shall see an eminent illustration of this fact if we now consider the action of drugs, and take up the first question proposed, namely, in what way is the action of drugs to be discovered?

If we look back through the avenue of five-and-twenty centuries we shall find that the knowledge of the action of drugs has been sought for in very various ways. We shall see that some of these ways are manifestly wrong, and that the rest are more or less imperfect. The orthodox school of medicine acknowledges that all have failed.

Let us briefly examine these ways.

1. *Drugs have been regarded superstitiously.*

Every kind of virtue has been attributed superstitiously

to the action of drugs. They have been given with incantations of every character. They have been worn as amulets and charms of every form, and of every material. And these things have been done in all ages, and in every country, to avert or to remedy disease. Alas! that such a method of discovering the action of drugs should have existed, should still exist in the world. It is checked only where, and so far as, the influence of pure Christianity is felt.

We may dismiss the further consideration of it here; but we should not do so without a blush: for we may well blush for the ignorance, for the folly, and for the sin which all such practices reveal.

2. *They have been viewed astrologically.*

For many centuries a belief has been maintained that the action of drugs is under the government of the sun, moon, and stars.

All the details of this misbelief are given with perfect good faith, and with entire confidence, so lately as the middle of the seventeenth century, in one of the most popular medical books of the time—by "popular" I here mean among medical men; this is the *Pharmacopœia* of John Shröder. (1656).

From the ninth chapter of this celebrated book, "De influentiis Stellarum"—a book, let me again observe, received and used by the orthodox physicians of the time—we may learn the puerile story of the "star-gazers."

A short extract will show the character of the statements:—

"*Saturnus* est planeta malignus, diurnus, masculinus, summè frigidus, Martis amicus, reliquis inimicus; *lieni* microcosmo correspondens.

"Res Saturninæ—plumbum, antimonium, aconitum, cannabis, agnus castus, hyoscyamus, helliborus niger, opium, sabina, &c.

"*Jupiter* est planeta benevolus, moderatè calidus, &c., *epati* correspondens.

"*Res Joviales* sunt stannum, argentum, berberis, mentha, quercus, symphitus, &c.

"*Mars* est planeta summè calidus, atque siccus, &c.

"*Res Martialis* sunt cinnabar, arum, carduus, plantago, urtica," &c.

And so through the planets, and also through the twelve signs of the zodiac.

These fancies having been received and assented to by the profession until times so recent, it is not wonderful that they still survive in "Culpepper's Herbal."

This starry method of discovering the action of drugs is wholly destitute of proofs; indeed, it rests upon the wildest conjectures. It may be dismissed without hesitation, notwithstanding its prevalence and popularity, as altogether erroneous and wrong.

3. *Drugs have been studied with reference to their sensible properties, such as form and colour, taste and odour.*

From the form and colour the "doctrine of signatures" was invented. Those who taught and practised according to this doctrine believed that "every natural substance which possesses any medical virtue indicates, by an obvious and well-marked external character, the disease for which it is a remedy, or the object for which it should be employed."

Dr. Paris,* from whom I have copied this definition of the doctrine, observes that "traces of its existence may be discovered in ancient authors . . . . . but the conceit did not assume the importance of a theory until the end of the fourteenth century, at which period we find several authors engaged in the support of its truth." Dr. Paris then gives several examples of the doctrine; such as "*Turmeric*, which has a brilliant yellow colour, which indicates that it has the power of curing the jaundice; for the same reason *poppies* must relieve diseases of the head; *agaricus* those of the bladder." &c., &c.

Dr. Dover, writing on jaundice, in 1732, has this paragraph:—

"Paracelsus, in his treatise *De Signatura rerum*, very much commends the inner bark of barberries, turmerick, rheubarb, and all plants of a yellow cast, in the cure of this disease. But they are too weak, as has been sufficiently experienced long since. He likewise commends the *arbor tremula* in agues; the *pulmonaria maculosa* in consumptions, *trachelium* in sore throats and quinseys, and so goes

* *Pharmacologia*, (1820).

on. I mention this to show on what weak foundations we often venture our lives."

The unsatisfactory nature of conjectures like these is, happily, now well acknowledged; and it is not necessary to pursue the subject further, either in the way of explanation or of refutation.

Nor can much that is less unsatisfactory be gathered from the taste and odour of drugs. A bitter taste has long had associated with it the idea of a tonic. A disagreeable odour has been supposed to indicate that the drug which is so distinguished is a remedy for hysteria.

It need scarcely be remarked that such indications as these are very little, if at all, better than those gathered from form and colour.

4. *Drugs have been studied chemically.*

Chemistry is an enchanted castle. When a man has once entered its captivating chambers it is seldom that he has even a desire to escape from them. The stress of circumstances or the sense of duty must be mighty indeed to drag him out of them. No wonder that medicine has been more than once made a prisoner in this castle, at one time by medical chemists, at another by chemical physicians.

Chemistry had its origin in cupidity. The dream of the alchemists was the transmutation of that which was base into that which was noble—in plainer English, the conversion of lead into gold—the cheaper into that which was worth most. The votaries at this altar of lucre sacrificed themselves; and the only recompense of their wasted time and misemployed talents was hopeless disappointment. Mankind gained from them the invention of furnaces, retorts, alembics, and other useful apparatus, since employed to better purpose.

In its aspect towards medicine chemistry has passed through three phases. First, as an art, in the hands of the alchemists, who spent much of their strength in experiments to discover the elixir of life. Secondly, as a science, in the hands of the physicians who were its discoverers and founders. Thirdly, as a theory, in the hands of modern medical and non-medical chemists, who have laboured with much zeal to establish a chemical physiology, a chemical pathology, and a chemical therapeutics.

We need not be detained by the labours of the al-

chemists, notwithstanding that great names are among them: such as Hermes Trismegistus, Geber (hence gebrish or gibberish), Albert of Cologne, Arnold of Villanova, Raymond Lully of Majorca, and even Isaac and John of Holland.

The physicians who discovered chemistry as a science succeeded in laying a foundation upon which a noble edifice has since been erected; but they failed in their application of chemistry to medicine. Their physiology was fermentation; their pathology was an excess of acid or alkali; their therapeutics, neutralisation.

It had been found that drugs refused to reveal their hidden virtues by their external features of form and colour and taste and odour; and it was seen that a further examination of them must be made. They were therefore put into the fire; a sort of torture which it was expected would compel them to confess. Hence these physicians—the iatro-chemists—were nick-named "furnace philosophers" by the iatro-mechanics of the next age. Basil Valentine of Erfurth, Van Helmont of Brussels, Glauber of Amsterdam, Sylvius of Leyden, and Thomas Willis of London, are eminent names as the founders of chemistry and its application to medicine. They fought a hard battle with the Galenists or orthodox physicians of their time, the rude blows of Paracelsus both helping them and hindering them. We have relinquished their medical hypotheses; we have retained a vast number of their chemical remedies.

The efforts of modern chemists claim a larger share of attention, and among these one non-medical name stands pre-eminent, that of Baron Liebig. It is necessary to notice some of his views with care. On the subjects of physiology and pathology briefly; on those connected with therapeutics more fully.

One or two sentences applicable to each of the former will be sufficient to show how completely chemical are the physiology and pathology of Liebig. With reference to the first he says:—

"Viewed as an object of scientific research, animal life exhibits itself in a series of phenomena, the connection and recurrence of which are determined by the changes which the food and the oxygen, absorbed from the atmosphere, undergo in the organism under the influence of the vital force.

"All vital activity arises from the mutual action of the oxygen of the atmosphere and the elements of the food."*

In pathology Liebig's "theory of disease" is as follows:

"Every substance or matter, every chemical or mechanical agency which changes or disturbs the restoration of the equilibrium between the manifestations of the causes of waste and supply in such a way as to add its action to the causes of waste, is called a *cause of disease. Disease* occurs when the sum of vital force, which tends to neutralize all causes of disturbance (in other words, when the resistance offered by the vital force) is weaker than the acting cause of disturbance.

"A deficiency of resistance in a living part to the causes of waste is obviously a deficiency of resistance to the action of the oxygen of the atmosphere." †

So, health is the chemical equilibrium between waste and supply; disease is the chemical disturbance of this equilibrium. At the same time Liebig admits the existence of a vital force distinct from chemical affinity.

A careful student of physiology and pathology will not find it difficult to discover that this hypothesis, though it brings to his notice a crowd of interesting and valuable facts, for which he will be grateful, is far too contracted and one-sided to embrace and explain either the operations of healthy life, or the phenomena of disease.

We now arrive at our own department, therapeutics and drug-action, and must go a little more into Liebig's details.

He gives the following classification of drugs:—

"Medicinal and poisonous substances form a most extensive class of compounds, the elements of which are capable of taking a direct or indirect share in the processes of secretion and of transformation.

"They may be subdivided into three great orders.

"The first (which includes the metallic poisons) consists of substances which enter into chemical combination with certain parts or constituents of the body, while the vital force is insufficient to destroy the compounds thus formed."

It will be observed that the metallic salts are here intended; and, with the exception of nitrate of silver, and one or two others as surgical applications, this order may

* *Animal Chemistry*, by Justus Liebig, edited by Dr. Gregory.

† Ibid.

be altogether dispensed with in medical treatment. They need never be given in such doses as shall distinguish them from the third order by their having any *chemical* action.

"The second division (consisting of the essential oils, camphor, empyrheumatic substances, and antiseptics), possesses the property of impeding or retarding those kinds of transformation to which certain very complex organic molecules are liable; transformations which, when they take place out of the body, are usually designated by the names of *fermentation* and *putrefaction.*"

We are here reminded of the chemistry of the 17th century. It seems to me that this description is a misapplication of terms, which can only increase obscurity and misunderstanding.

"The third division of medicinal substances is composed of bodies the elements of which take a direct share in the changes going on in the animal body. When introduced into the system they augment the energy of the vital activity of one or more organs; they excite morbid phenomena in the healthy body. All of them produce a marked effect in a comparatively small dose."

I apprehend that this division includes all substances which are truly entitled to be called medicinal. And it describes their two sets of properties. As poisons "they excite morbid phenomena in the healthy body;" and as remedies "they augment the energy of the vital activity of one or more organs." In other and safer words, as remedies they tend, in the organs where their actions take place, to diminish morbid phenomena, and to restore health. It will be observed that Liebig's words are hypothetical; they give an explanation which is merely a conjecture. To say that "they augment the energy of the vital activity" may be highly poetical language, but it is not the expression of a fact which can be proved. Aconite or digitalis, in certain doses, can bring down the beats of the heart from 120 to 40. Whether, in doing so, they augment the energy of the vital activity of the heart, may be questioned; they certainly diminish very notably its mechanical activity.

Liebig then makes an observation which will be readily agreed to. "None of the substances in this class (of medicines) can be said to take a decided share in the nutritive process, or to be employed by the organism in

the production of blood; partly because their composition is different from that of blood, and partly because the proportion in which they must be given to exert their influence, is as nothing compared with the mass of the blood."

He concludes with an endeavour to give a chemical explanation of the action of drugs:—

"Medicinal or remedial agents may be divided into two classes, the nitrogenised and the non-nitrogenised.

"The nitrogenised vegetable principles, whose composition differs from that of the proper nitrogenised elements of nutrition, also produced by a vegetable organism, are distinguished, beyond all others, for their powerful action on the animal economy."

It is not necessary for others to undertake to show that this classification of medicines is of no practical utility to the physician; Liebig does this himself. He goes on to say that

"The effects of these nitrogenised substances are singularly varied; from the mildest form of the action of aloes, to the most terrible poison, strychnia, we observe an endless variety of different actions.

"The medicinal or poisonous action of the nitrogenised vegetable principles . . . cannot be supposed to be independent of the nitrogen they contain, but is certainly not in direct proportion to the quantity of nitrogen. Solanine and picrotoxine, which contain least nitrogen, are powerful poisons. Quinine contains more nitrogen than morphia. Coffeine, theine, and theobromine (coffee, tea, and cocoa), the most highly nitrogenised of all vegetable principles, are not poisonous."*

We may, therefore, safely conclude that a chemical theory of the action of drugs, even when attempted by those most competent to give it, is a failure.

Chemical *analysis* has furnished physicians with an immense number of interesting and useful facts. It has made us acquainted with the elements of which organised bodies are constructed. It has taught us that these elements are the same as those which constitute non-organised bodies or minerals: that the vegetable kingdom is the producer of organic compounds: that the animal world is dependent upon vegetables for these organic compounds,

* Ibid.

as the prepared materials for its sustenance : that during the life of vegetables oxygen is set free, and the atmosphere is replenished with it, and refitted for the respiration of animals: while during the life of animals a continual absorption of the oxygen of the air takes place, and its combination with carbon forming carbonic acid, and with hydrogen forming water, is effected ; these compounds of carbon and hydrogen are restored to the atmosphere in expiration, and by them vegetables are nourished. It has also taught us the composition of drugs, and it has separated, in some of them, their active from their inert components. It has, moreover, shown us the presence of many of these drugs, when they have been taken either as poisons or as remedies, in the organs of the body where their action has taken place.

Chemical analysis has made surprising advances towards teaching us, not only the ultimate elements, but also the organic compounds of which vegetable and animal tissues are composed. But chemical analysis has its limits, and at present it stops just where we wish it could go further. It stops where we cannot but feel sure that it falls short of the objects desired to be attained ; and so leaves upon the mind an impression of unsatisfactoriness which is painful. In proof of this, we need only to be reminded of such facts as these :—

The most careful analysis tells us that there is no difference in composition between thein, caffein, and theobromine—the active ingredients of tea, coffee, and cocoa ; that the composition of fibrine and albumen is identical ; and that it can find nothing in the deadly poisons of serpents which it does not also find in the innocent gum-arabic.

When statements like these are made, the mind simply does not believe them ; it prefers to conclude either that chemical analysis is as yet imperfect, or that the solution of such problems as these is not within its limits.

Chemical *affinity* is a mighty power in nature. Within its proper province it acts with irresistible force, and brings about the most surprising changes: sometimes slowly, as in the rusting of a sword, sometimes rapidly, as in the firing of gunpowder. But chemical affinity also has its limits, though they have not yet been experimentally defined. Temperature determines one of its

boundaries, as is seen in the manufactures of glass* and of steel.†

Heat, indeed, often excites the action of chemical affinity, or brings it into play; but in the instances given, it produces compounds independent of the controlling power of chemical affinity, as that force is at present understood. The components are not combined in definite proportions, and yet the properties of the compounds greatly differ from those of the component parts.

Life has a still greater power in limiting the action of chemical affinity, which, however, it does not destroy nor supersede.

As regards the action of drugs, chemical analysis helps us when it can show the presence of the drug in the organ in which its action has taken place; but chemical affinity renders us no assistance in the investigation of the action.

A final remark on chemistry.

The forces in nature with which we are best acquainted are those of attraction and repulsion. Among attracting forces are *gravitation*, which acts between *all* particles of matter at sensible (sometimes at inconceivably great) distances; *cohesion*, which acts between *similar* particles of matter at insensible distances; and *chemical affinity*, which acts between *dissimilar* particles of matter at insensible distances. Chemistry is the branch of science which occupies itself with the phenomena belonging to this last kind of attraction. It is an experimental science. The results of its experiments are surprising and beautiful, and have been made eminently useful to man. When occu-

* Chemists speak of glass as a true chemical and therefore a definite combination, and suppose flint-glass to be a silicate of potash and oxide of lead; crown-glass a silicate of soda and lime; and so of other kinds of glass. But this has not been established by experiment. Many years ago, having at the time free access to, and ready use of some large glass furnaces, I availed myself of the opportunity to try a series of experiments, in order to test this point. The ingredients were carefully combined in the proportions of their chemical equivalents, coming as near to the empirical proportions used by the manufacturers as calculation permitted. My glass was made at the same time, and in the same furnace as the other. But the glass whose ingredients were mixed by "rule of thumb" far surpassed in all its properties that mixed by rule of equivalents.

† Steel, the properties of which, it is well known, differ much from those of iron, is a compound of iron with a variable percentage of carbon.

pied with organic substances, chemistry affords very interesting information to the physician, and puts him under obligations which he should always be ready to acknowledge. But chemistry cannot analyse *living* substances; and, therefore, when it attempts to give a chemical explanation of *living* operations, whether healthy or morbid, it has trespassed beyond its province. Chemistry can teach the physician many interesting things, which he could not otherwise know, concerning the composition of *dead* organic bodies, and can throw light upon *some parts* of living processes; but physiology, pathology, and therapeutics—the functions of life, the aberrations of disease, and the action of remedies—can never be included as chapters in a treatise on chemistry.

5. *Drugs have been studied mechanically.*

The aberrations of the human mind have been strange, but few have been more strange than that which led physicians to apply the mathematical principles of mechanics to the primary phenomena of living beings; to such actions, for example, as the circulation of the blood by the heart, and the production of the various secretions by the glands.

Men were drawn aside into this path of error by the notion, which has prevailed in all ages, that the progress of medicine is dependent upon the progress of science. Dr. Quincy, a great promulgator of the mechanical doctrines, and the most popular English medical writer of the last century, says:—

"The study of medicine has in all ages been influenced by the philosophy in vogue, because the theory thereof is inseparable from a good competency of knowledge in natural causes. . . . . .

"I say physics (physical science) and medicine, because the latter cannot subsist without the former. . . . . .

"And because what is brought from physics and mechanics takes up so much room here, it may be necessary to inform the reader that there is no knowledge in medicine but by such means. Experience without theory will never make a physician. . . . . .

"If there be anything of science in medicine, it is conducted by demonstration, because conversant with objects cognizable only by the evidence of sense; but without this it is chance and confusion, and the enthusiast and the

empiric are upon as good a foot as the scholar and the physician."*

This opinion is still adhered to in our time. The conviction is thus expressed by Professor Acland :—

"A profession dependent on science must vary with that on which it depends, and if it does not advance with the advance of science, that fact proves it to be in error."†

I am not of this opinion, but believe that the prevalence of it has been a stumbling-block in the way of the real improvement of medicine. This improvement can be brought about only by the study of medicine *per se ;* by its workmen quarrying in its own mine. It is not necessary to depreciate any branch of science, and the physician may gladly admit that occasional assistance of a valuable kind may be got from collateral sciences, and further help may be expected as these make further advances ; but not one of them can be made a safe peg to hang medicine upon. We have seen how true this is of chemistry. We shall now see that it is equally true of mechanics.

And that not for want of the devotion of talent and labour. The iatro-mechanics of the 17th and 18th centuries, like their predecessors the chemists, were men of great ability, and they took incredible pains in the direction of their research. Borelli, professor of mathematics in the University of Pisa, the founder of the school, Baglivi, called the Roman Hippocrates, and the illustrious Boerhaave were a noble triumvirate on the Continent ; nor were Dr. James Keil and Dr. Richard Mead mean representatives in England.

It has been readily admitted that chemistry gives the physician very interesting information. It is as readily conceded that mechanics do the same. That the size, form, strength, and situation of bones, and the number, position, power, and insertion in or attachment to the bones of muscles, are regulated with infinite skill upon mechanical principles, cannot be doubted. But the attempt made by these eminent men to apply these principles was far more ambitious than this. All the vital processes of the living economy were explained by mathematical formulæ ; diseases were an excess of relaxation, or

* *Lexicon Physico-Medicum*, by John Quincy, M.D. 9th ed. 1775.
† *Medicine in Modern Times.* Discourse II. by Dr. Acland. 1869.

an excess of tension ; and drugs acted vigorously, according to the laws of mechanics, by their spicula and angles, and by their gravity.

To carry out this mechanical hypothesis a vast variety of geometrical diagrams and algebraic formulæ are given. Some of these measure the power of the muscles, others estimate the force of the heart in the propulsion of the blood, others calculate the work done by the glands in the production of their secretions, which is " performed by a composition of two motions, direct and transverse."

Borelli's demonstration of the power of the heart's action brings him to the conclusion that its exercise is equal to the pressure of 180,000 lbs. weight to move 20 lbs. of blood.

Keil gives two different calculations, founded upon two different sets of experiments. The result of one is that the heart's force is equal to 5 oz., and of the other that it is equal to 8 oz.

The outrageous divergency between the demonstration of Borelli and that of Keil arises mainly from the different data taken by each. Borelli treats the column of blood as stationary ; Keil as already in motion ; " which, how it first came by," says Quincy, " seems out of human capacity to determine."

The difficulty of ascertaining the data from which to commence calculations, which is here made apparent, is a " glaring instance," in the words of Lord Bacon, of what will always be an absolute hindrance to any useful application of mathematics to these subjects. For a mathematical demonstration which shall be reliable, the data must be few and certain. In physiological questions, such as those we are considering, they are not only numerous, but very imperfectly ascertainable, a consideration which should have been sufficient to deter men from pursuing this path.

That the necessary data are too numerous, and of too uncertain a character to be successfully managed by geometry and algebra will be very evident, if the experiments of Keil are briefly related.

" Having uncovered the iliac artery and vein in the thigh of a dog, near to his body, and having passed convenient ligatures under them, he opened the whole diameter of the vein, and received into a cup all the blood which ran from it in the space of ten seconds of a minute;

after that the same was done by the artery for the same space of time, and both the quantities of blood were exactly weighed. . . . . This experiment was repeated, until the quantity of blood which runs from the artery, to the quantity of blood which runs from the vein, was found to be, in the same space of time, nearly as seven and a-half to three.

"Now, the velocity of the blood in the iliac artery, so near the aorta, is nearly the same as that in the aorta, and consequently the velocity with which it flows out of the iliac artery cut assunder, is the same with which it would flow out of the heart unresisted; or the blood runs through a wound in the iliac artery with all the velocity it received from the heart. Now, all the blood which runs along the iliac artery returns again by the iliac vein, and consequently the quantities of blood which pass through both, in the same space of time, are equal. The quantity of blood, therefore, which runs out of the iliac vein cut assunder, is the same which runs through the iliac artery before it was cut, in the same space of time. Having therefore the quantity which runs through the iliac artery when it is cut and when it is not cut, we have their velocities. . . . . .

"Now, if the heart throws out two ounces of blood every systole (as is most probable), then the blood moves through the aorta at the rate of 156 feet in a minute, and, therefore, the absolute velocity," &c.

The mathematical part I need not repeat; it is worked according to the second corollary of the 36th Proposition of the 2nd book of Newton's *Principia*.

The second series of experiments was founded on the properties of the *parabola*. "Upon opening the iliac artery of a dog laid in an horizontal direction, and 28 inches high from the ground, he found that the blood moving in the parabola A.F.C. touched the ground at C., which is about three feet distant from the perpendicular A.B. let fall from the heart." . . . . . The reasoning then gives $1\frac{1}{3}$ of an ounce=force of this dog's heart. "Now, the heart of this dog weighed two ounces; and *hearts being to one another as their weights*, and *supposing* that the weight of an ordinary human heart is 12 ounces, then its force will be almost equal to 8 ounces."

It cannot be necessary to point out how uncertain many of these data are, or how wide of the mark any calcula-

tions depending upon them may be. But, besides these, there is one datum strangely overlooked by all these eminent physicians, which, of itself, is sufficient to paralyse every effort in the direction of geometry as applied to living beings. This is the energy of *life*, an energy which admits of no calculation. Though the power of life has limits of its own, it ranges itself outside the boundaries of mechanics, and entirely refuses to submit to be measured by mathematics. Think of the living heart of a body strapped down to a table, and whose iliac artery is cut assunder and is bleeding it to death! Who can calculate the force of its convulsive beats? Who can even compare them with those of the same heart in a state of rest and peace? And how can the force of a *living* human heart be known to be to that of a dog's *living* heart, as the weight of one when it is dead is to the weight of the other when it is dead? Vain, indeed, is such science as this!

But, it will be said, this is physiology, and it behoves us to return to our subject, therapeutics.

The same mechanical principles were applied to all medicinal substances. Dr. Quincy, in his *Dispensatory*, a book as popular as his *Lexicon*, makes this application throughout. Let me give one example and copy what he says upon *iron*. After speaking disparagingly of the astrologists and the chemists, he proceeds:—

"We shall therefore inquire by what manifest properties this metal comes to afford so much of moment (momentum) in medical preparations. And to this purpose thus far in common may be concluded, as from all other metalline particles, that such as can be mixed with the blood and made part of the circulating fluid, must, of course, by the necessary laws of motion, from their superior gravities, be of great force to break their way where particles of less gravities cannot get through. For mechanics teach nothing more plainly than that the momenta of all percussions are as the rectangles under the gravities and celerities of the living bodies. By how much more gravity, then, a metalline particle has, more than any other particle in the blood, if their celerities are equal, by so much the greater will the stroke of the metalline particles be against everything that stands in its way than of any other not so heavy; and therefore any obstruction in the glands and capillaries will be sooner removed by such

particles than by those which are lighter. *This is a way of reasoning which is plain to the meanest capacity.*

"But, if *iron* has this property by virtue of the specific weight of its particles in common with some other metals, it has also a further advantage of being a powerful deobstruent, from the shape of its component parts; for both our taste and sight convince us of their pointed angular forms, especially if we view them in their shoots or crystals, in making the *vitriol* or salt of iron. From the sharp and pointed figures of the particles of iron, will they be efficacious to cut their way through many hindrances."*

This is doubtless a sufficient illustration of the mechanical theory of therapeutics of the eighteenth century. But let me also remind my readers of one of the essays of Dr. Mead. It is the first in his collected works, and is entitled "A Mechanical Account of Poisons," first published in 1702, and finally in 1747. It contains original experiments with the poison of the viper, the "saline spicula" of which do all the damage, in the first edition by pricking the blood, and in the latest edition by pricking the "nervous fluid."

The "Aphorisms" of Boerhaave have much in them of the same mechanical type. He treats first of "the diseases of a simple solid fibre"; then of those "of a weak and lax fibre"; then of those "of a stiff and elastic fibre."

This leads me to a final remark upon the views and principles of these famous men; they were distinguished from those of the ancients, which were humoral, by the prominence given to the solids of the body, and from those of their immediate predecessors, which were chemical, by their mathematics and mechanics.

A few years ago it was said of these mechanical speculations that they exist only in history. This cannot be said now, for they have been strangely revived in our time. It will be necessary, therefore, to notice this revival, and the Essays of the Rev. Professor Haughton may be taken as the representatives of this "school of thought."

Medicine in Modern Times (1869) contains a discourse

* Quincy's *Dispensatory*. 10th ed. 1736.

"on the relation of food to work," or, "on physic in relation to medicine in modern times," which puts the views of this school very accurately before us.

A few extracts will show what these are:—

"Man and other animals possess a double life, animal and organic, presided over respectively by two distinct though correlated centres of nervous force; of these, one thinks, moves, and feels; the other merely cooks, receiving the food supplied, changing and elaborating it into elements suitable for the use of the animal life." . . . .

"In the higher forms of animals, and more especially in man, the animal life dominates over the organic life, which becomes its slave, and exhibits the remarkable phenomena of mechanical force, of geometrical instinct, of animal cunning, and, finally, in man himself produces intellectual work, rising to its highest form in the religious feeling that recognises its great Creator, and bows in humility before Him. It is a simple matter of fact and of everyday observation, that all these forms of animal work are the result of the reception and assimilation of a few cubic feet of oxygen, a few ounces of water, of starch, of fat, and of flesh." . . . . .

"The food consumed in twenty-four hours, including air and water, undergoes a series of changes of a chemical character before leaving the body, in the form of one or other of its excretions. Some of these changes develope force, and others expend force, but the algebraic sum of all the gains and losses of force represents the quantity available for work. This work must be expended as follows:—

1. The work of growth.
2. The work of maintaining heat.
3. Mechanical work.
4. Vital (intellectual) work." . . . . .

"Let us take, as illustrations, the muscles and brain, regarded as the organs by means of which mechanical and intellectual work is done. These organs resemble the piston, beam, and fly-wheel of the steam engine, and like them only transmit or store up the force communicated by the steam in one case, and by the products of the food conveyed by the blood in the other case."

Then follow nearly fifty pages of mathematical calculations on the several relations between the food taken, the air breathed, and the work done.

To my mind such calculations as these, when supposed to represent the sum of vital phenomena, are a delusion, and as great a delusion as that which misled the talented physicians of the last century. In proof of this the following observations are offered :—

It is required for the solution of a problem by figures,

1. That all the factors be included.

2. That all admit of being numbered, measured, or weighed.

3. And that this numbering, measuring, and weighing has been accurately done.

Now, not one of these indispensable conditions has been fulfilled in Prof. Haughton's calculations.

1. The data, elements, or factors, in living operations, are too numerous for all of them to be taken into account. Moreover, the one which is the most essential, namely the vital force or life, is wholly omitted.

2. This vital force refuses to be either counted, measured, or weighed.

3. And even the counting, measuring, and weighing of those elements upon which the calculations are actually made, are at the best only approximations, falling more or less short of accuracy.

All this is true of calculations based upon experiments made upon animals. In man another element presents itself, and one which exercises a mighty power over the body, and the action of its several organs. This is the MIND, in its double influence, by its intellectual and its moral faculties. Hear what King Harry said to Cardinal Wolsey :—

> "——— Read o'er this,
> And after, this; and then to breakfast, *with*
> *What appetite you have.*"

It should be gratefully acknowledged that the information which is obtained by such pursuits as those of Prof. Haughton, when confined within their proper limits, are highly interesting, and sometimes of value. But they are worse than useless when they are ambitious of representing the circle of living phenomena.

Mathematical calculations may be applied to the action of muscles and bones where the elements are few, and the mechanical arrangements are obvious. When, therefore, Professor Haughton undertakes to prove the following propositions, he may be more safely listened to :—

'1. Each muscle is constructed in relation to its joint, in such a manner as to perform one kind of work only, and it performs that work to a maximum advantage.

"2. The number of muscles employed is determined by the number of distinct actions required from the limb.

"3. The shape and form of the bones employed are the necessary consequence of the shape and power of the muscles in action.

"4. The smallest muscle in the combination is as carefully adapted to its conditions of maximum work as the largest muscle."*

These, no doubt, are propositions in mechanics, to which mathematical calculations may be applied. But when these muscles contract, and these bones are moved, a force comes into play, which sets all calculations at defiance. For it must be borne in mind that every movement is dependent upon, and is regulated by the presence of *life*. This is a force which is absolutely unmeasurable by any methods we possess.

The "necessary consequence" which Prof. Haughton says follows from these propositions is—

"That a foreseeing mind planned the type of the limb and of its actions."

This remark brings to my recollection a charming Essay on *The Pleasures of Science*, written nearly fifty years ago, by Lord Brougham; in which he gives, as examples proving this "necessary consequence" (among many others which I have forgotten), the curve of the head of fishes, this being the solid of least resistance; and the hexagonal form of the cells of a bee-hive, and the roof and floor of the hive; these being formed upon the truest mathematical principles.

On other living processes, or vital work, such, for example, as respiration, all that can be truly said is, that during this process certain chemical changes take place, which it is in our power to examine qualitatively, and to some extent, quantitatively; and also certain mechanical actions are performed, which we can observe, but cannot measure their force. So, in the living process of digestion there are chemical changes, and there are mechanical actions; but neither respiration nor digestion can be defined as a chemical or as a mechanical operation.

* *British Medical Journal*, April 20, 1872, page 416.

Prof. Haughton goes on to apply the physical "theory to diseased conditions of the body." I must not follow him in his details. Difficulties and objections, like those we have considered in physiology, but in increased degrees, will be found; and the practical excesses which have resulted from the adoption of such views, show how mistaken and dangerous they are. The "furnishing of fuel in the form of wine and beef-tea" in fevers, to the extent to which it has been carried, may be mentioned as an example.

The application of similar calculations to therapeutics and the action of drugs, has not, so far as I am aware, been yet made in our time.

The mention of "fuel" makes it almost necessary that something should be said upon "innate" or "animal heat," as handled by Professor Haughton. This is the more readily done, because it offers another opportunity of opposing medical hypotheses, and of endeavouring to eradicate them from medical literature.

The subject is introduced thus :—

"Hippocrates was well aware of the connection between food and animal heat, although he erroneously regarded the animal heat as the innate property of the body that caused an appetite for food, instead of being itself produced by food; if we transpose his cause and effect, *mutatis mutandis*, all his maxims as to animal heat are true. Thus he says—

"'Growing animals possess most innate heat, hence they require most food; . . . . . but the old have least heat, and therefore require the least fuel.' (Aph. I. 14.)

"The doctrine of 'innate' heat taught by Hippocrates and Galen, ruled in medicine for 1500 years after Galen's death, until it received its death-blow from the genius of Lavoisier, who demonstrated, in his celebrated memoir read before the French Academy of Sciences in 1783, that the source of animal heat is to be found in the combustion of the carbon of the body by the oxygen of the air received into the lungs by respiration."

Lavoisier's memoir was founded upon experiments with a guinea-pig; and the hypothesis put forth by him, in opposition to that of Hippocrates, now rules in medicine.

Let me first speak, and speak reverently, of the Father of Medicine.

The first section of his matchless book contains twenty-five Aphorisms. The majority of these are as true, and as important, now as when they were written. They begin with that most suggestive and solemn one:—

Life is short, and the Art long; opportunity fleeting; experience deceitful; and judgment difficult. The physician must not only be prepared to do what is right himself, but also to make the patient, the attendants, and externals, co-operate."

Then follow excellent declarations of simple facts concerning diet, regimen, and the use of aperients. And it seems to me that the only blot in the first section of the Aphorisms of Hippocrates, is the introduction in three of them of hypotheses. Two of these refer to innate heat, the other to the concoction of humours.

The 13th is as follows:—

"Persons of advanced years endure a spare diet most easily; next adults; young persons not nearly so well; and especially infants, and of them such as are of a lively spirit."

Then follows the 14th, quoted by Prof. Haughton:—

"Growing persons have the most innate heat, they therefore require the most food, for otherwise their bodies are wasted. In old persons the heat is feeble, and therefore they require little fuel, as it were, to the flame, for it would be extinguished by much."

To have expressed this truth consistently with the tenor of the other Aphorisms, it should have been worded thus:—

"Growing persons require the most food. Old persons require less."

And I venture to think that the opposite hypothesis, founded upon Lavoisier's celebrated guinea-pig, and which forms the basis of Prof. Haughton's elaborate calculations, may be dispensed with in like manner, to the great advantage of practical medicine. It will receive its "death-blow," and be superseded by some others; indeed, it is already giving way to the "mode of motion" hypothesis; a notion, I presume to say, not one whit more worthy of adoption than any of its predecessors.

Let physicians learn and remember *facts*. They have not time to spend over the pros and cons of contending hypotheses. "Life is short, and the Art long."

Aristotle seems to have thought that the mind is composed of two parts; one by which we contemplate those things which cannot be otherwise than they are, and one by which we consider those things which can be otherwise than they are.* It is not necessary to cut the mind in two in this manner; nor is it necessary to seek a foundation for mathematics different from that of other branches of natural knowledge. One, two, three, four, a point, a straight line, an angle, a circle, are as much objects of our senses as a tree or an animal. The only difference is in the simplicity of the elements of the former, and the number and complexity of those of the latter. This difference, however, is so great, that what is possible to us in the one case, is impossible in the other. The simple cases may be handled mathematically, the complex ones cannot be so handled.

It might have been expected that these considerations would have been sufficient to prevent any one from engaging in the hopeless undertaking of applying mathematical reasoning, or even mechanical principles, (except to the limited extent noticed), to such complicated problems as occur in physiology, pathology, and therapeutics. But nothing can restrain the ingenuity of men when they are pursuing an object by a false route.

6. *Drugs have been studied botanically.*

The Greeks marred their therapeutics by introducing the philosophy of their times into medicine, and this in two ways; first, the philosophies themselves being erroneous, their introduction into medicine was the introduction of errors; and secondly, the attempt to apply philosophical speculations to therapeutics was a misapplication even of any truth they might contain.

The early physicians were not content with this double damage. They invented medical philosophies themselves, such as those of the temperaments, and of the humours, and in this third way disfigured and depreciated their really useful medical knowledge.

But they had neither the chemistry nor the mechanics, nor the botany which we have, and therefore, happily for themselves, could not make a further misapplication of

* *Ethics*, Book VI., "— καὶ ὑποκείσθω δύο · τὰ λόγον ἔχοντα," &c.

science by misapplying these branches of it to medicine. This was left to the moderns, and it has been thoroughly done by them.

We have seen to what extent by the chemists and by the mathematicians. The botanists have followed in train, though they have not been able to effect nearly so much mischief.

Scientific botany, like scientific chemistry, is indebted to physicians for its beginning. One of its earliest friends was Leonhart Fuchs, a physician of Tubingen, about three centuries ago. For a long time it was studied as a branch of natural history only; that is, its descriptions were confined to the outward forms of plants; now it embraces the internal structure of the organs, or constituent parts, which is called organography, and the functions of these organs, or their physiology. At first the descriptions of plants was connected with their supposed medical virtues, but now, like chemistry, it is an independent science, and the chemist and the botanist are no longer physicians.

It is, however, one of the collateral sciences with which the physician ought to be acquainted. The knowledge it can impart adds very much to the interest of the study of the Materia Medica, and it is occasionally useful; but it cannot fulfil such promises as those made by Professor Lindley when he says:—

"This science it is which teaches the physician how to discover in every region the medicines that are best adapted for the maladies prevalent in it, and which, by furnishing him with a certain clue to the knowledge of the tribes in which particular properties are or are not to be found, renders him as much at ease, alone and seemingly without resources, in a land of unknown herbs, as if he were in the midst of a magazine of drugs in some civilised country."

We shall see that these flattering promises cannot be realised.

There are two ways in which they might be fulfilled: first, in the plants themselves there might be evidence or information from which the physician might learn the medicines best adapted for special maladies. Or, secondly, the classification or arrangement of plants in the various systems of botany might furnish this evidence.

Let as ask, then, is there anything in plants themselves which indicates their medicinal properties? Not in them-

selves. The most careful examination, whether of their form, appearance, colour, taste, or smell, or of the different parts of them, or of the climates or regions in which they grow, or of the seasons in which they bloom, only forces upon the mind the conclusion that the medical virtues of plants cannot, in this way, be learned at all.

No: but, says Professor Lindley, it is the *science* of botany which teaches the physician this knowledge. Let us look at the science.

The view must be a brief one, but it is hoped that it may be clear and correct. The representative men of the modern systems of botany are Cœsalpinus, Ray, Tournefort, Linnæus, the two Jussieus, De Candolle.

Andreas Cœsalpinus of Arezzo, about 1580, was the first who accomplished a scientific classification of plants. He was a *fructicist.* He says :—

"If we take the root, or stem, or leaves, or blossom, as our guide in classification, we shall separate plants obviously alike, and approximate those which have merely superficial resemblances."

His system, therefore, is founded upon the difference in the number or covering of the *seeds.* He has ten classes formed in this way :—

"Some have, under one flower, one seed, as *amygdala;* or one seed receptacle, as *rosa ;* or two seeds, as *ferularia* ; or two seed receptacles, as *nasturtium ;* or three seeds, as the *tithymalum ;* or three receptacles, as the *bulbaceæ ;* or four seeds, as *marrubium ;* or four receptacles, as *siler ;* or more seeds, as *cicoraceæ ;* or more receptacles, as *pinus.*"

A strong testimony in favour of this arrangement is that it is consistent with natural orders, with many of which Cœsalpinus was acquainted.

John Ray, a century later—about 1680—the father of English scientific botany, based his system in part upon the *fruit,* and in part upon the *flower.* The vegetable kingdom being necessarily first divided into plants which have flowers and plants which have not flowers—*phanerogams* and *cryptogams*—the flowering plants are *simple* or *composite.* The simple flowers have the seeds *naked,* or in a *pericarp.* Those with naked seeds are arranged according to the number of the seeds. Of those which are surrounded by a pericarp, or fruit, some have the first large and soft, as apples ; some have it small and juicy, as

gooseberries. If this fruit is not juicy but *dry*, it is simple or multiple. The simple are the pea-tribe, *leguminosæ;* the multiple are arranged according to the differences in the flowers, *monopetalous*, &c.

The *composite* flowers are those which have many florets in the same *calyx*. They are divided into those having complete florets, and those having only half florets.

This system was in use through the 18th century. Like its predecessor it had the advantage of embracing some natural families.

Joseph Pitton de Tournefort, of Paris, published his "Institutio rei Herbariæ" in 1700. He was a *corollist*. His system is based upon the different forms of the *corolla*. Thus not only departing from the seed altogether, but not even including the whole flower. Tournefort's first division is into herbs and trees; herbs are divided into seventeen classes, according to the presence or absence of the corolla, and its form, regular or irregular, single or numerous petals. His second division of trees is divided in the same manner into five classes.

Even this arrangement, feeble as it would seem to be, includes some natural families also, as the *labiats*, *cruciforms*, *umbellifers*, *legumens*, &c.

Karl Linné, one of Sweden's greatest ornaments, had become celebrated half a century later—about 1750. Linnæus chose also only *a part of the flower* as the foundation of his system—not the corolla but the *stamens* and *pistils*, the former for the classes, the latter for the orders. He was a *stamenist*. In this manner he arranges the phanerogams or flowering plants in twenty-three classes, and the cryptogams, or flowerless plants in one class.

Linnæus thus returned to *numbers*, retracing steps in the path opened by Cæsalpinus, but quitted by Tournefort for one much less precise and fixed—that of *forms*. There was also in the system of Linnæus a tinge of physiology, which was a new feature. It grievously separates the members of some natural families.

This defect, by which plants manifestly closely related in nature were artificially detached from one another, became more and more felt, and in about another quarter of a century the two Jussieus, uncle and nephew, were able to gain attention to a method which is justly called the natural method.

It is not founded upon the consideration of a single

organ, but the characters presented by every part of a plant concur in fixing its position in the scheme.

And, further, anatomy and physiology are introduced. There are fifteen classes, which include 164 natural orders. These are arranged in three divisions. *Acolytedons*, which correspond to the cryptogams, mushrooms, lichens, lycopods, &c., eleven orders; *monocotyledons*, embracing thirty orders; and *dicotyledons* 123 orders. These three divisions are taken from the seed as germinating. Cœsalpinus took the seed as perfected; Jussieu takes it as beginning to grow. It has then either one lobe from which one little leaf springs—monocotyledons—or two lobes from which two little leaves spring—dicotyledons.

This natural method was a great improvement upon the artificial systems of Tournefort and Linnæus.

De Candolle—from 1824 to 1840—adopts the same method, but carries it further, availing himself of the discovery which in the meantime had been made by Des Fontaines of the distinct structure and growth of the *stem;* one portion of flowering plants increasing from *within*, the other from *without*, or on the external surface. De Candolle adopts this as the basis of his arrangement, and calls those growing from within *endogens*, and those growing from without *exogens*. The first correspond to Jussieu's *monocotyledons*, the second to the *dicotyledons*. The cryptogams or acotyledons are called *acrogens*.

This method is the one which prevails in our time. It is not founded, as the earlier systems were, upon external characters only, but very much upon the anatomy and physiology of plants. It is, so far, more likely to be permanent, but it is not without its difficulties, nor is it free from vagueness; for it brings many plants together which have no very obvious resemblances. This will be acknowledged to be no unimportant matter when it is remembered that the great object of science in classification is to bring together things which are alike, and to separate things which differ.

The latest improvement is that made by Professor Lindley, who has introduced into English botany the use of English names.

Of these six methods of classifying the vegetable kingdom the four first belong to natural history, the similars and dissimilars being exclusively external. They are

asked to tell us anything they know about medical virtues. They answer plainly, "it is not in us."

The two remaining methods advance into the interior of plants, and examine the structure and the functions of each part. This is a great increase of knowledge. Let us ask them the same question; what can you tell us of medical virtues? They reply as distinctly as the former, "it is not in us."

How is it, then, that many medical writers, Pereira among them, have adopted a botanical arrangement of the *Materia Medica?* thus apparently confirming the opinion of Professor Lindley that botany "teaches the physician how to discover medicines," and implying that the "natural orders" of plants contain some rule as to their medical virtues.

The examples of any rule of this kind are, at least, equally matched by the exceptions, both in number and importance; hence the rule is useless, and falls to pieces. Plants gathered together as they are at present, in natural orders, differ both as regards food and medicine, some in the same order being nutritious, and others poisonous—as carrots and hemlock—and among those which are poisonous, and therefore medicinal, there are very great differences of drug-action.

And as regards the example of Pereira he acknowledges that he has adopted this method in despair, "on account of the great difficulties attending any other method, especially that founded on the physiological effects of medicines," the only true method.

There are two ways of looking at this question. Either to suppose that the botanical arrangements yet made are not perfect, or the best that can be made; and that some botanist, perhaps yet unborn, may make an arrangement of plants which will put those which are nutritious and those which are poisonous in more consistent groups; or to believe that the arrangements of the physician have nothing to do with those of the botanist. This latter view seems to me to be the true one.

It would be obviously unreasonable to ask a botanist to arrange his plants according to their action, nutritive or poisonous, upon animals which it does not come within his province to observe. To me it appears equally unreasonable for the physician to adopt the method of the botanist, and arrange his drugs according to those characters only which are open to the botanist to observe.

It may be safely concluded that there is no observed connection between either the external forms of plants or the internal structure of the organs or their functions, and their action as drugs. Neither the outward appearance, nor the structure of any part, nor its function, nor the mechanical power, nor the chemical composition of vegetables, throws any light upon their therapeutic action.

I shall be charged with being an enemy to science. I contend that whoever points out the true limits of its several branches is one of its best friends. What, then, are the uses of botany to the medical man? Several; at present I will mention only one. That the English practitioner may be taught how he may substitute English species of medicinal plants for foreign ones. For instance:—

There are three principal species of *daphne*. The *D. gnidium* (flax-leaved daphne) of Hippocrates; the *D. mezerium* (spurge olive) of Dioscorides; and the *D. laureola* (spurge laurel) of Gerard and Parkinson. The first and second are natives of Greece, and, I believe, are still to be found in the Greek Pharmacopœia. The second is indigenous in Germany, and was proved by Hahnemann; and it was probably brought into this country, along with other medicinal plants, by the Romans. The third is indigenous in England. This last I have used instead of the mezerion, with good success, especially in some skin diseases. In the same manner the *bryonia* of England is the *B. dioica*, that of Germany is the *B. alba*, the latter being the species proved by Hahnemann. For many years I have used the English species instead of the German, and with great satisfaction. The same result may, perhaps, be expected if the *actæa spicata* (herb Christopher), an English species, be substituted for the *actæa racemosa* (or *Cimicifuga racemosa*, black cohosh) of America. It grows on high chalky ground in Yorkshire, and it deserves to be carefully proved. The same remarks will apply to many other foreign plants with equal force, and the attention of the younger members of the profession is earnestly invited to the investigation.

Before quitting the subject of botany let me ask leave to point out a comparison which may be made between the progress of the classification of plants and the progress of homœopathy.

Hahnemann's homœopathy is very exactly comparable with Tournefort's botany. The system of Tournefort, as we have seen, was founded upon the similar and the dissimilar in the form of the *corolla*. It put together the similar forms and separated the dissimilar. Hahnemann did the same with the symptoms of diseases and the symptoms of drugs; similarity put them together, dissimilarity separated them.

Linnæus added some physiology to this external system of botany, but it still remained in his hands what it had been in Tournefort's hands, a comparison of similars.

So modern homœopathists, feeling with Dr. Black how bare and unsatisfactory a medical system is without pathology, have tinged Hahnemann's external homœopathy with pathology, by adding the internal condition as so many additional symptoms to the external ones. They then carry out as before the doctrine of similars, and "still hold to the old empirical formula of Hahnemann drawn from the totality of the symptoms."*

Jussieu and De Candolle took a great step in advance when they made *anatomy* the basis of their method. It was nothing less than the substitution of a natural for an artificial system.

Though not suggested by this progress in botanical science, the thought I have breathed resembles that of Jussieu's. Make *anatomy* the basis of therapeutics; endeavour to discover the organs which are the seat of disease, and the organs which are acted on by drugs, and prescribe a drug which acts upon the organ where the disease is seated.† In this manner similarity is exchanged for identity. The difference between the two methods is the difference which exists between *similis* and *idem*—between similar symptoms and the same organ.

The next step—the *kind* of action and the connection

* Dr. Black, in *Monthly Homœopathic Review*, Oct., 1868, and my "Reply," Nov. 1868. Also as a "Postscript" to the Essay on "The Anatomical Basis of Therapeutics," 1868. Dr. Black endeavours to take advantage of the expression "tubercular cachexia." This expression, like many similar ones, serves only to hide our ignorance from ourselves. It may be used, for brevity's sake, as summing up the local diseases and the symptoms of such cases; but if any one thinks that it contains knowledge beyond this, he deceives himself.

† When there are several acting on the same organ, the *kind* of action will decide which. The kind of action of drugs will take its turn for consideration in the next Essay.

between the kind of action of disease and of drug—shall receive all the attention it claims in a future Essay.

Linnæus, by the tinge of physiology in his system, added a charm, but gave no real help; so the pathology of Dr. Black is comforting to the educated medical mind. But so long as its conditions are viewed as so many symptoms to be added to the outer ones, and similarity is to be aimed at in the selection of the remedy, the benefit is more imaginary than real. On the method proposed it is substantial and fundamental. Similarity of symptoms exhibits some mysterious relationship between disease and its remedies; but an anatomical connection brings them into actual contact. It is the substitution of a natural for an artificial system.

7. *Drugs have been studied pathologically.*

Drugs studied pathologically means that they have been prescribed in accordance with some pathological doctrine.

The methods pursued for the discovery of the action of drugs which we have hitherto noticed have been either entirely mistaken and vain, as the three first, or have rendered very partial service, as the three last.

We now come to a method which ought to have proved a good one; and, in proportion as a true pathology is established, it may become a good one. So far it has been a mistaken one, and, as carried into practice, has done more harm than good; because the pathological doctrines successively in vogue, and especially those among the ancients, have been erroneous.

There is one grand therapeutic axiom which has ruled in medicine for thousands of years, and has been supported by a succession of pathological doctrines, which claims to be considered in every medical investigation. This is the treatment of diseases by their contraries—*contraria contrariis curantur*. It will repay the trouble if we look, for a short time, at these successive doctrines.

The first was old in the days of Hippocrates, and notwithstanding that it was vigorously opposed in one of the Hippocratic writings, continued to hold its supreme authority till two centuries ago, and this mainly because it was founded upon, and was supported by, the prevailing philosophy of the times.

The doctrine of the ancient philosophers, which pre-

vailed amid many conflicts, was substantially this:—There is a primary matter which has neither form nor quality. This first becomes apparent to us by being made to assume in the hands of the Great Creator the form and qualities of *four elements*—fire, air, earth, and water. From these all other forms and qualities are elaborated.

To suit this doctrine of the philosophers the medical doctrine was invented of *four temperaments*—hot, cold, dry, and moist—health being a combination in due proportion of these, and sickness being a preponderance of one or more of them, and so constituting an *intemperament.*

Hence followed the therapeutic doctrine that drugs are possessed of similar qualities; and the arrangement of the Materia Medica in eight divisions—hot, cold, dry, and moist; and their combinations hot and dry, hot and moist, cold and dry, and cold and moist.

All the details of diseases and of remedies being thus arranged to the entire satisfaction of every one, it is easy to see how charming the treatment by contraries must be. As a matter of course a hot intemperament must be treated by a cold remedy, and a cold intemperament by a hot one.

I have remarked that this doctrine was ancient in the time of Hippocrates, that is, four centuries before Christ; it was brought to perfection, and established upon the ruins of every other medical doctrine, by Galen, in the second century after Christ; and it reigned without a rival for fifteen centuries more. It would be disrespectful, therefore, to the men of so many generations did we not acquaint ourselves to some extent with this wonderful hypothesis—wonderful indeed it must have been to bind down the medical profession as one man, to receive it without question as the only orthodox method, for so long a time.

In addition to what has been already said of the four elements, a few details relative to the four temperaments, the intemperaments, and the classification of drugs, will give us an intelligent understanding of this mighty system of medicine.

The temperaments are four simple and four compound ones, as already enumerated.

These temperaments are either of the whole body, or of some part of it, *e.g.*, the brain, the heart, the liver, &c.

A bone is the most dry and cold.
A cartilage less so.
A ligament less than a cartilage.
The heart is most hot.
The liver next to the heart.
Muscles are moist, and hot.
The skin is less so.
The spinal marrow is colder and moister than the skin.
The brain exceeds this in moisture.

There are, in the same manner, eight kinds of intemperaments. When it is said a man has a hot liver, it is meant that his liver is hotter than a justly tempered one should be.

Then there are four humours—the blood, the phlegm, the yellow bile, and the black bile. And from these, according to the predominance of any one of them, we have a sanguine person, a phlegmatic person, a choleric person, or a melancholic person.

A disease is defined to be an affection against nature, by itself hurting and depraving *the action of the part in which it resides.* It is sometimes called a distemper.

By symptoms are meant that change which the disease brings, and which follows a disease *as a shadow follows the body.*

A medicine is defined to be that which has power to change the body according to one or more qualities, but which cannot be changed into our nature—*cannot nourish.*

They are possessed of the four simple qualities—heat, cold, &c., and the four compound ones—heat and moisture, &c.

Medicines are hot, cold, &c., in four degrees, *e.g.*, warm water is temperate; that which is a little hotter is the first degree of heat; if manifestly hot, it is the second degree; if it heat more vehemently it has come to the third; if it scald, it has arrived at the fourth degree of heat. The following are a few examples:—

Simple Medicines hot in the

First degree—Absinthium. Althæa. Amygdala dulc. Beta.

Second degree—Ammoniacum. Artemisia. Anethum. Salvia.

Third degree—Abrotanum. Agnus castus. Anisum. Sabina.

Fourth degree—Allium cepa. Euphorbium. Sinapis. Ruta.

Simples cold in the

First degree—Atriplex. Hordeum. Malva. Pyra. Pruna. Rosa.

Second degree—Acacia. Cucumis. Plantago. Solanum hortense.

Third degree—Hyoscyamus. Solanum somniferum. Mandragora.

Fourth degree—Cicuta. Papaver. Opium.

Simples moist in the

First degree—Buglossum. Viola. Rapum. Spinacia.

Second degree—Lactuca. Cucurbita. Melones. Portulacca.

Simples dry in the

First degree—Thus. Chamæmelon. Brassica. Crocus. Faba.

Second degree—Pix arida. Nux moschata. Mastiche. Myrrha.

Third degree—Abrotanum ustum. Myrtus. Galla. Sabina.

Fourth degree—Piper. Allium. Nasturtium. Euphorbium.

This arrangement of the qualities, Galen says, is in order to proportion them to diseases. For example: To a disease hot in the second degree no other medicine must be used than that which is cold in the like degree.

So much for the first qualities or faculties of medicines; now for the second. Those are termed second faculties which are dependant upon the first. It is the part of heat to rarify, attract, open. Of cold to condense, repercuss, shut up. Of moisture to soften, relax. Of dryness, to harden, stiffen.

Hence, that is termed an attractive medicine which has an attractive faculty; as, on the contrary, that a repercussive which repels. So of emollients, relaxers, &c.

The third faculty of medicines depends upon the first and second, sometimes conjoined, at other times separate. Its operations are to agglutinate; to fill with flesh; to

cicatrize; to assuage pain; to promote or diminish the secretions of the kidneys, skin, uterus, &c. Thus the generation of flesh is produced by the concourse of dryness and cleansing. Coldness joined with some moisture procures rest; this is the sleepy faculty of opium.

But the fourth faculty of medicines does not depend upon any of the foregoing qualities, or upon any other manifest quality. This depends upon *an occult property* of the whole substance, by means of which it works rather upon this than that part; upon this rather than that humor. Wherefore physicians *cannot find out this faculty by reason, but only by experience.* Hence it is that names have been given to medicines from those parts that they chiefly respect. For they are termed *cephalics* which respect the head, as betony, marjoram, sage, rosemary; *pneumonics* which respect the lungs, as liquorice, sweet almonds, elecampane; *cordials* that strengthen the heart, as saffron, cinnamon, citrons (chiefly the rinds), bugloss, coral, ivory; *stomachics,* which respect the stomach, as nutmegs, mint, anise, mastic, pepper, ginger; *hepatics,* which respect the liver, as wormwood, agrimony, succory, sanders; *splenetics,* which have relation to the spleen, as thyme, broom flowers, capers (the bark of their roots), the bark of tamarisk; *diuretics,* such as respect the kidneys and urinary passages, as the roots of smallage, asparagus, fennel, butcher's broom, turpentine, plantain, saxifrage; *arthritics,* such as strengthen the joints, as cowslips, elecampane, calamint, hermodactils, &c. And to this rank is referred purging medicines, which, furnished with a "*specific property,*" show their efficacy on one humor more than on another, and that impact more in one part than in another. For thus *agaric* draws phlegm from the head and joints; *rhubarb* draws choler from the liver, and hurts the kidneys.

Such is a sketch of the most famous of all medical systems. It has often been remarked that no hypothesis becomes popular unless it has some truth mixed up with its fancies; and it is not difficult to see that this system, though it begins very high up in the clouds of imagination, descends at length to very practical matters of common observation. That, in fact, it is constrained to acknowledge itself compelled to abandon its boasted reasonings, and to take up with the previously despised

testimony of experience derived from the observation of the senses.

It is a melancholy fact that even Hippocrates himself could not prevail to put away the vain fancies of this system, and persuade men to be content with the truth of facts. The treatise "On Ancient Medicine," which is acknowledged by competent judges to be a genuine work of Hippocrates, commences thus:—

"Whoever having undertaken to speak or write on medicine, have first laid down for themselves some hypothesis to their argument, such as hot, or cold, or moist, or dry, or whatever else they choose, are all clearly mistaken in much that they say; and this is the more reprehensible as relating to an art which all men avail themselves of on the most important occasions, and the good operators and practitioners in which they hold in especial honour." He goes on to refute these speculations, and remarks with wonderful sense and precision, "*I have not thought that medicine stands in need of an empty hypothesis.*"*

This is the system of treatment by contraries of the *dogmatists*, ancient and modern. Another system having the same maxim, "*contraria contrariis curantur*," was maintained with much talent among the ancients under the title of *Methodism*. This system, after a vigorous struggle, was overpowered by dogmatism, and in consequence its books are nearly all lost.

Asclepiades of Bythinia, a disciple of Epicurus, gave to the elements the name of atoms, and taught that the human body is formed of tissues every way permeable, or pierced with invisible holes, which he named pores, through which atoms continually pass and repass. Health depends upon the symmetry of the pores and the atoms. Disease on their disproportion. The aim of medicine is to enlarge the pores when they are too contracted, and to close them when they are too open—"*contraria contrariis curantur.*" His remedies made him a popular practitioner; they were chiefly physical exercise, such as walking, riding on horseback or in a carriage, and boat rowing; frictions; and wine.

Themison of Laodicea, a disciple of Asclepiades, but

* *The Genuine Works of Hippocrates*, by F. Adams, LL.D. Sydenham Society's Edition, Vol. I., page 161.

not quite so visionary, separated diseases into three orders, the constrictive or contracted; the flexionary or relaxed; and the mixed.

It follows from this division of diseases that with the Methodists there were only two kinds of therapeutic indications to fulfil, viz., to relax when there was constriction, and to constrict when there was a flux or relaxation. All remedies therefore are either astringents or relaxants. Among the former are darkness, fresh air, cold or acid drinks, decoction of quince, red-wine vinegar, and solution of alum. Among the relaxants are bleeding, emollient poultices, warm and laxative drinks, sudorifics, warm air, sleep, and exercise carried to fatigue.

The works of Cœlius Aurelianus are the only writings which have not been lost, of the physicians belonging to this school. But many extracts have been preserved by other authors.*

A modern edition of Methodism was made popular for a time by Dr. John Brown, a pupil of Cullen's. "Life," he said, "is only sustained by incitation. It is only the result of the action of incitants on the incitability of organs." Consequently *sthenia* and *asthenia* were the only divisions necessary; "*contraria contrariis curantur*" being the rule of treatment, and nearly all diseases being *asthenic*, wine became with Brown almost the only remedy. As a serious commentary on this method of treatment, I cannot avoid quoting the following passage from the French *History of Medicine* of Renouard:—

"This doctrine, so seductive in its exposition, so easy in its application, is one of the most disastrous that man has been able to imagine, for it tends to propagate the abuse of diffusible stimulants, of which spirituous liquors make a part, an abuse excessively injurious to health in general, and the intellectual faculties in particular—an abuse to which man is too much inclined naturally, and which the sophisms of Brown may have contributed to spread in all classes of English society."

There is yet another form of the doctrine of contraries. This was held partially by Hippocrates himself, and it is very prevalent among modern physicians. It is a comparatively

* See the *History of Medicine*, by Dr. Renouard.

limited and feeble doctrine, but, so far as it goes, it is very firmly maintained. Its principal expression is in giving purgatives in constipation, astringents in diarrhœa, refrigerants in fever, and stimulants in debility.

The more ancient and prevalent doctrine was perfect; all diseases were hot, cold, dry or moist, or a combination of these; all remedies were possessed of exactly similar qualities; so that it was always possible, by the hypothesis, to prescribe a contrary. The defect of the doctrine was that it was imaginary—the whole story was a fiction. But a fiction of such beauty and attractiveness, that mankind were fascinated by it for thousands of years.

The Hippocratic and modern doctrine is lame in comparison. Many diseases, as viewed at present, have no contrary condition, by inducing which, they can be opposed. Many drugs have no recognised condition at all according to which they can have contrary actions; for example, all those that are called *alteratives*. It is a fragmentary doctrine, embracing only a small number of particulars. No doubt, there is some truth and reason in it, but it has been carried out to an extent which has buried this truth under mountains of error.

Hahnemann's doctrine of "*similia similibus curantur*" is opposed to this modern doctrine of contraries, and not to that of Galen.

There is yet another pathological and therapeutical doctrine of contraries, of great importance and of widespread influence in the practice of physic, which claims to be noticed as one of the methods by which the action of drugs has been studied. It is the doctrine of derivation, revulsion, or counter-irritation. Its contrariety to the disease lies in the action being produced in a healthy part. This has been brought before us on former occasions, and it is not necessary to go into its details now. It may be remarked, however, that its greatest justification, as it appears to me, is contained in one of the Aphorisms of Hippocrates, which says:—

"Of two pains occurring together, not in the same part of the body, the stronger weakens the other." (Section II. 46.)

The arguments against it are, that it makes two sores instead of one, and that it is practically possible to cure by acting upon the diseased parts, and leaving the healthy parts to remain healthy.

The method which is opposed to it has been called Organopathy, and is that which is advocated in these Essays.

8. *Drugs have been studied empirically.*

And what is *empiricism?* It is a word now so commonly used in an unfavourable sense, as synonymous with quackery, that an explanation is necessary. It was not always so used, nor is it so on the present occasion.

God has endowed us with reason, and has given us five bodily senses. With the former we can study the operations of mind; with the latter we can observe the phenomena of the universe, and upon these observations of our senses we can again exercise our reason.

Reason and sense (or the senses) are not always employed together. Sometimes men speculate upon the outer world mentally, without the foundation of observation—this is reason without sense—*idealism.* Plato is commonly considered the leader of this school.

Sometimes observation is not ignored, but it is kept in subjection to the mind—this is reason controlling sense. Aristotle has the reputation of being the father of this method. It teaches us to descend from general principles, thought out by the mind, to particulars observed by the senses; which is a *deduction* of a false kind.

A third method is that of subjecting mental work to observation—which is reason guided by sense. Lord Bacon has the honour of advocating this method. It teaches *induction* of generals, to be arrived at, not suddenly, but step by step from particulars.

Lastly, there is the method of observation alone—sense without reason—this is *empiricism.*

It may be added that in all these methods there is implied belief. When this is wanting, and there is doubt only, without belief, it is *scepticism.*

Empiricism in medicine has three aspects, one towards doctors, another towards diseases, and another towards drugs.

Empiricism looks at doctors with two expressions; one gracious, and recognising their position, the other scornful, and treating them with contempt. As already remarked, the word is used in this Essay with reference to medical men in its favourable sense. In this sense, to distinguish

it from quackery, it has been called methodical empiricism. This is empiricism with a rule or method which implies knowledge; quackery is without rule and without knowledge.

The aspect of empiricism towards diseases is distinguished by the absence of all speculations as to their essence or internal nature. It rejects equally the humoralism of the dogmatists: the pores and fibres of the ancient methodists, and the modern Brunonians; the spasms of Cullen; and the phlegmasies of Broussais; and contents itself with a simple but careful observation of the symptoms, their progress, and their apparent connection with the internal organs of the body.

Empiricism looks at drugs, and stores up the knowledge of them which has been acquired by what is called accident or chance, or by whatever other means. This empirical knowledge of drugs consists of a very large amount of useful facts, but which are not connected together by any chain of reasoning.

The story of the discovery of the medical virtues of Peruvian bark, whether true or imaginary, illustrates what is meant by the accidental or chance discovery of medicines. The story of the introduction of antimony as a medicine, in the *Currus Triumphalis* of Basil Valentine, is one instance of many for which medical men are indebted to persons outside the profession. For the introduction of iodine, and some others, we are indebted to the direct experiments of physicians.

At different periods of time, a large part of the medical profession has discarded the prevailing hypotheses of the age, and has used nearly all medicines empirically. This is especially the case at the present time.

The student of medical history may well be driven to despair. It is not surprising, though it is very melancholy, to see the greatest historian of medicine of modern times, Kurt Sprengel, end his laborious investigations by becoming a sceptic. Hear his final convictions:—

"The history of medicine shows us that a blind confidence in our opinions is almost always a proof of their falsehood, or of the weakness of the foundation upon which they rest. In studying this history one is persuaded with Pyrrho, that the way to fathom it is to suspend one's judgment, and that the wisest part is to look

upon all opinions with indifference, without adopting any."*

The historian who stands next to Sprengel is the French physician Renouard. His researches have not led him into scepticism but into empiricism. After advocating it with much earnestness at the close of his history, he concludes with these words:—

"This system, it is true, takes from the mind many illusions which flatter our vanity, but which are obstacles to progress. Although the world grows older, man remains a child whom fictions amuse. But in a science like medicine fictions are never innocent; they have always caused much evil, and have retarded the progress of light much more than doubt and ignorance."†

I have said that empiricism is sense without reason. This is true in one respect but not in another. It is true with reference to what had been said just before of the dogmatism and methodism of the ancients; and it is also true with reference to the moderns who base their systems of medicine upon the laws of physiology. Such physicians, whether ancient or modern, think they ought to know the nature of diseases, the physiological disturbance, and to reason out upon this knowledge a method of treatment. The empiric argues that this is a great mistake. "All such reasoning, though it appears just and natural, is only a subtle sophism, which clinical experience contradicts at every step. There is a crowd of diseases whose nature escapes our researches yet which we know how to cure, and there are others whose mode of formation is better known to us, whose treatment is, nevertheless, little improved by that knowledge,"‡

Empiricism, then, rests upon the observation of the senses, and rejects all elaborate reasonings upon the nature of diseases and the action of remedies. In this respect empiricism is sense without reason.

In another respect, however, it is not without reason; for it has framed for itself, from the observation of the senses, an axiom or rule by which it is guided. And the rule is this:—

"Those remedies which have cured one case of disease will cure all cases analogous to it." Or,

* Kurt Sprengel. Introduction. † Renouard. Conclusion.
‡ Renouard.

"Diseases must be treated by remedies which have been experimentally recognised as the most efficacious."*

Empiricism was first raised to a high position in the school of Alexandria, and it retained an honourable distinction till the time of Galen. But its fall was complete then, and its name became an epithet of reproach for fifteen hundred years. It was the produce of rare intelligence and great labour, but it had two fatal defects: one, the want of a chain to connect its numerous but isolated facts; the other, the absence of an element of progress; for it possessed no means whereby new remedies could be discovered, and the treatment of disease be made more successful. In this way its ruin became inevitable.

Modern empiricism must share the same fate. Its many facts are like a handful of separate grains which the first shake will scatter to the winds. They are unstrung beads which are easily lost; and there are no effectual means either of gathering them up or of adding to their number.

This latter defect, the want of some means of discovering new remedies, is honestly confessed by the most strenuous advocates of empiricism. Renouard, after having demolished all other systems, and deliberately taken refuge in empiricism, has this remarkable passage:—

"It must be avowed that the fundamental axiom of empiricism does not furnish any light to direct us in such researches; *it does not at all indicate the route to follow for the discovery of curative means.*"

This surely is to pronounce its condemnation. A system which must for ever stand still, and that in the face of manifest and daily failures, without adequate means of improvement, cannot be accepted as the final system of medicine.

We have now seen that the grand medical systems of the ancients, the Dogmatism of Galen, the Methodism of Themison, and the Empiricism of Serapio were either mischievously erroneous or fatally defective. Modern systems, which are, for the most part, physiological doctrines, do not offer much truer satisfaction, or much

* Renouard.

greater success. So that Empiricism is again prevalent, notwithstanding its acknowledged deficiencies.

Empiricism collects the assemblage or totality of symptoms, prescribes some remedy already known, and falls to pieces from vagueness; from the too great multiplication of details; from the want of a cement to bind the details together and to make them manageable; and from the absence of any definite means of improvement, or of making the addition of new remedies.

The general conclusion from this rapid sketch of medical history, and the lesson to be learned from it, seems to be this: that reason and the senses are twin handmaids, whose office it is to guide man into the knowledge of natural truth; and that they must be allowed the privileges claimed by each. If a partiality be shown to either, the consequences are ruinous: if to reason, by being led into error; if to the senses, by the arrest of progress.

Let us now for a moment, before passing on to the next part of the subject, look at Hahnemann's Homœopathy in the light which we have derived from medical history.

It is evident that Hahnemann's system is another form of empiricism; inasmuch as he limits himself to collecting the totality of the symptoms, both of diseases and of drugs.

Instead of the rule of past experience, he adopts that of resemblances—similar symptoms of drug and disease.

It has one great advantage over all other forms of empiricism: it possesses the means of adding new remedies, by the proving of drugs in health. This is a glorious gain.

But it is not freed from the other great defect of empiricism; for the system is made to rest upon the observation of symptoms only. Unless some chain of reasoning can be found, by which the details, as so many separate links, can be united together, it must expect the fate of other forms of empiricism, and fall to pieces from its very progress; that is, from the accumulation of an unmanageable amount of detached facts.

Homœopathy, indeed, is already becoming cumbrous and vague in this manner. The numbers of cases and of provings are now so vast that, like a hand too full of beads, they must be scattered and lost for want of a binding string.

But if, by some successful effort of reason, a thread can be discovered, by which these beads can be strung together, permanency may be safely predicted for it.

Physiology and pathology have been pressed into this service, but without success. The thread I have proposed is not a physiological, nor a pathological, but an *anatomical* one. The *seat* of the symptoms generally admits of being observed almost as clearly as the symptoms themselves; and what reason has to do is to connect the two observations together. This union becomes the thread which will preserve the symptoms, like so many single beads, from dispersion and oblivion.

It will sometimes be found that the same symptom belongs, in different cases, to different organs; and the precision and success of treatment will depend upon the organ which is its true seat in each case being carefully ascertained.

For example, palpitation, besides belonging to the heart, as it may do in the majority of cases, in others may have its origin in the brain, in the stomach, in the uterus, or in the muscles. Cough may have its cause in the stomach, or in the uterus, as well as in the various parts of the respiratory organs. It is well known that pain in the right shoulder-blade sometimes belongs to the liver; and that pain in the knee is sometimes caused by disease in the hip-joint.

But if we take anatomy for a basis, instead of physiology or pathology; if we study the organs of the body, and attach to each organ the symptoms of diseases and also the symptoms of drugs which belong to it; we shall unite diseases and their remedies together, as so many links, in a golden chain; and this system of medicine will remain, and be permanently useful.

9. *Drugs have been studied mainly for their indirect action.*

The indirect method of treatment has been a prevailing error from the earliest times that reasoning has been applied to medicine. It embraces a large part of the practice of physic of the present day.

This subject was discussed in a previous Essay, the title of which is *The Single Medicine*, and which was first published in 1857. In the same year the late Sir John Forbes published his interesting but to his own school

most discouraging book, *Of Nature and Art in the Cure of Disease*, which he calls "the legacy left by him to his younger brethren after fifty years of practice."

In this book Sir John Forbes delivers his judgment of homœopathy in these words; first in regard to infinitesimal doses:—

"The homœopathic remedies, so called, are utterly inert, and incapable of influencing the body, in any of its organs or functions, whether in health or disease. This is to me a demonstrated fact."

Secondly, in regard to its principle:—

"If it should repudiate this doctrine [the use of the infinitesimal dose] it must then be taken out of its present category, and placed under the head of empirical and pseudo-specific treatment of the lowest and worst kinds."

After this it will not be suspected that he and I had had any previous communications on the subject on which we were both, unknown to each other, engaged. Indeed, it was not until some years afterwards that I saw and read his book.

Probably many of my readers are familiar with the book; if not, it may be respectfully suggested that the three concluding chapters (x., xi., and xii.) well deserve a careful study. I dare not occupy much time in giving extracts from them, but must beg leave to produce one or two. In the opening of the tenth chapter we read thus:—

"They [medicines] would seem all capable of being arranged in two main classes, according to the more or less direct way in which they influence the morbid state.

"In the first class we would comprehend all those means which, in producing their effects, act or are supposed to act directly on the disease itself, or on the disordered parts and functions constituting it, or on its immediate and still persistent cause, and which may therefore be called *Direct means*.

"In the second class we place all the remaining means, those, namely, which, possessing no special relation with the morbid state itself, act on it merely in an indirect or vicarious manner, by modifying some other organs or functions or the system generally, and so influencing the disease. These may therefore be named *Indirect means*."

After a careful examination of these two classes of means, the conclusion is thus expressed:—

"In only a very minute proportion of the numerous diseases presented to us in practice—and these few, for the most part, of slight importance—are we able to act positively or certainly, that is directly or specifically, on the diseased part, or on its morbid condition; while the whole huge remainder of diseases can, as we have seen, be only indirectly, and distantly, and slightly touched by our agents in any case,—and in a large proportion of cases, cannot be touched at all. . . . ."

"From the survey in the last chapter, it appears that, with the exception of a very few, and those comparatively insignificant diseases, the Medical Art does not possess the power of curing diseases in a direct and positive manner. In the few diseases in which it may be said to do so speaking generally, it not seldom fails to do so in individual instances.

"In all other cases—that is, in the vast majority of diseases—the Medical Art, even when exerting its powers most successfully, can, in strict language, hardly be said to *cure* diseases at all. All that it professes to do, and all that it does, is to influence diseases in an indirect and partial or imperfect manner, by modifying, to a greater or less extent, the functions of certain organs, with the view and in the hope of thus modifying the processes in which the malady consists."

"The degree to which the Medical Art can fulfil even this humble office, we have seen to be infinitely less, generally speaking, than the public and even than the members of the medical profession have always believed, and still believe."

It was necessary and fair, after this overwhelming condemnation, to insert a saving clause, and it is added in these words:—

"The Medical Art, when it condescends to leave its imaginary heights of power, and take up its true position as a helper in man's infirmities, proves itself to be not simply useful, but most valuable in almost every case of disease, slight or severe, curable or incurable. . . . . Nature can almost always be helped, in some slight degree at least, either negatively or positively, if not in both ways, by those who possess the necessary knowledge and skill."

In this farewell testimony of Sir John Forbes we have, first, his condemnation of homœopathy; and then, as the

final result of a very long practice, in which great abilities, industry, and opportunities were united, we have his frank confession of the general prevalence of the indirect method of treating diseases, and of its painful inefficacy. A condemnation of his own school of medicine scarcely less severe than that of homœopathy. Why do we respect his judgment in the one case, and not in the other? Because he was competent to condemn the system with which he was practically acquainted; and was not competent to condemn homœopathy, of which he had no practical knowledge.

When a patient has been examined by a physician, and the symptoms have been observed and noted, they are viewed in three aspects. One is for the purpose of forming an opinion of the seat and nature of the disease—for diagnosis. Another is to judge of its probable progress and termination—for prognosis. The last is for treatment. All the symptoms are not of equal value in each of these points of view; some tell the seat and nature of the disease better than others; some guide the prognostications; and some give what are called the indications of treatment. It is with the indications of treatment that we have now to do. These indications, according to long prevailing notions, are many and diverse. They are nearly all embraced by two thoughts—elimination and counter-irritation.

As regards the first—elimination,—turning the complaint out of doors—the indications supposed to be supplied by the symptoms are many; for example, the practitioner sees in the case before him what suggests to him reasons for bleeding; or for purging; or for emetics; or for diaphoretics; to produce perspiration; or for diuretics to act on the kidneys; or for anodynes or soporifics, drugs possessing the sleepy property, the fashionable one just now being *chloral hydrate.*

It will be noticed that none of these indications suggest a *direct* cure of the malady, but the production of some effect, commonly a discharge or increase of some secretion, which it is presumed will *indirectly* mitigate the force of the disease.

The other thought is revulsion or counter-irritation. This contemplates acting on healthy parts with the intention of relieving, in this *indirect* way, the diseased parts.

For example, a congestion of the brain immediately suggests to the physician that the healthy bowels must be irritated with purgatives such as colocynth or gamboge (see these drugs in Pereira's *Materia Medica*); the healthy skin must be inflamed by a blister; the healthy kidneys must be set to do increased work; and so on. An artificial disease being thus necessarily added to the natural one.

The medical profession since the times of Hippocrates has been so indoctrinated with these notions of eliminating the essence of the disease by driving it out in some discharge, or of frightening it away by the perturbative practice of counter-irritation, that it has always been with extreme difficulty that any remedy could be introduced which offered to cure a patient in a less offensive manner.

Renouard, when describing the introduction of cinchona, tartar-emetic (at one time proscribed by a decree of the Parliament of Paris), ipecacuanha, belladonna, digitalis, vaccination, &c., has a very vigorous passage on this point. He says :—

"An important remark on the subject of all these beautiful improvements remains still to be made, which is, that they were all accomplished, not in virtue of prevailing theories, but in spite of them; but the greatest obstacles they had to surmount to become established, came from these very theories. What was the reproach that the adversaries of cinchona brought against that medicine? It was this, that it produced no sensible evacuation. In their opinion, founded on the authority of Galen and others, the proximate cause of intermittent fevers could be nothing else than vitiated bile or phlegm; so that a medicine which expelled neither the bile nor the phlegm, could not, according to their doctrine, cure an attack of ague."*

Many excellent remedies, which exert no power but the power of healing, have passed, mainly through the hands of homœopathists, into general use. And yet, so inveterate is the habit of inventing hypothetical explanations, that medical men will not acknowledge this power of healing simply as a fact. They will use terms which imply an explanation of the manner in which this power is exerted. Indeed, hypotheses and their invention are nothing less than a medical mania.

* Renouard's *History*, Reform Period, Ch. V.

Take, as an example, the following paragraph about copper, from Pereira :—

"If the cupreous preparations be used in *very small doses*, they sometimes give relief in certain diseases, without obviously disordering the functions; in other words, in these instances the only apparent effect is the modification observed in the morbid condition. These are the cases in which these preparations have been termed *tonic*, *antispasmodic*, or *alterative*, according *to the nature of the disease;* thus, in ague they have been termed tonic, in epilepsy antispasmodic, in dropsy alterative."*

And so the tonic property, the antispasmodic property, the alterative property—and why not all other properties?—reside, not in the drugs, but in the diseases! Such reasoning as this ought to require no refutation.

The conclusion seems to be irresistible that this indirect method of using drugs as remedies must be abandoned, and a better method must be sought for.

10. *Experiments on animals.*

In the paper on "The Physiological Action of Medicines" which I read before the British Association for the Advancement of Science, at its meeting at Nottingham in 1866, a protest was made against experiments for medical purposes on living animals. The objections to the practice are of two kinds: one set of objections arises out of considerations of its cruelty; the other comes from the evidences of its inutility. In my opinion any advantages which have hitherto been derived from such experiments are not sufficient to remove these objections.

Since this protest was made before the British Association, the subject has been considered by the General Committee of that scientific body, and the result of their deliberations is thus stated in the "Report" for 1871 :—

"A committee, consisting of ten individuals, having been appointed at the last meeting of the British Association, held at Liverpool in 1870, to consider the subject of Physiological Experimentation, in accordance with a resolution of the General Committee hereto annexed, the following Report was drawn up and signed by seven members of the Committee :—

* Pereira's *Materia Medica*, Vol. I., page 869. 4th Edition.

"REPORT.

"I. No experiment which can be performed under the influence of an anæsthetic ought to be done without it.

"II. No painful experiment is justifiable for the mere purpose of illustrating a law or fact already demonstrated; in other words, experimentation without the employment of anæsthetics is not a fitting exhibition for teaching purposes.

"III. Whenever, for the investigation of new truth, it is necessary to make a painful experiment, every effort should be made to ensure success, in order that the suffering inflicted may not be wasted. For this reason no painful experiment ought to be performed by an unskilled person with insufficient instruments and assistance, or in places not suitable to the purpose—that is to say, any where except in physiological and pathological laboratories, under proper regulations.

"IV. In the scientific preparation for veterinary practice, operations ought not to be performed upon living animals for the mere purpose of obtaining greater operative dexterity.

"Signed by

"M. A. LAWSON, Oxford; G. M. HUMPHREY, Cambridge; JOHN H. BALFOUR and ARTHUR GAMGEE, Edinburgh; WILLIAM FLOWER, Royal College of Surgeons, London; J. BURDON SANDERSON, London; GEORGE ROLLESTON, *Secretary*, Oxford."

The resolution of the General Committee annexed to this Report contains the following clause:—

"That the said Committee be further requested to consider from time to time whether any steps can be taken by them, or by the Association, which will tend to reduce to its minimum the suffering entailed by legitimate physiological enquiries; or any which will have the effect of employing the influence of this association in the discouragement of experiments which are not clearly legitimate on live animals."

A new committee was appointed to carry out this suggestion.*

At present, perhaps, it may be said that the subject is open to two opinions. Those who doubt the propriety of such experiments will not be willing to make them; but they may lawfully learn—indeed they cannot but learn—what may be taught by the results of the experiments made by others, whose convictions allow of their being made.

A remarkable series of experiments was undertaken by an Edinburgh Committee of the British Medical Association, presided over and reported upon by Professor Hughes Bennett, in 1868, which deserves careful notice. The subject of enquiry was the action of mercury on the liver. The experiments were performed by Drs. Rutherford and Gamgee. They occupied two years. Thirty-three dogs were operated upon. Satisfactory observations could be arrived at in only eight of these. The conclusions drawn by the Committee from these experiments are given as follows:—

"1st. The relation of food to the biliary secretions is not so invariable as previous experimenters appear to think. . . .

"2nd. The relation supposed to exist between the amount of biliary secretion and the size or weight of the animal has not been supported by the foregoing observations. . . .

"3rd. Although an animal will live for a certain time without any bile passing into its alimentary canal, it would appear that, even when a fistula has been established without accident, the health begins to suffer, in periods varying from a few days to a few months . . . [contrary to the opinion of some former experimenters.]

"4th. Various circumstances apparently diminish the amount of bile secreted. The chief of these, as shown by the preceding observations, are starvation, diarrhœa, and mercurial poisoning. . . .

"5th. As to anything that enables us to increase the amount of bile, beyond the giving food and supporting

* *Report of the British Association for the Advancement of Science.* 1871.

health, we are unacquainted with it. Perhaps there is no opinion in medicine more widely spread, and certainly there is none more universally acted upon, than that mercury does so; in short, that it acts as a cholagogue. Yet not only have the few experimenters who have directed their attention to this subject, invariably observed that mercury rather diminishes than increases the secretion of bile, but the general results of the trials made by your Committee fully confirm this conclusion. We have seen that in whatever form or dose it may be given, such as continuous moderate doses of blue pill, minute and frequently-repeated doses of calomel, or large doses varying from 10 to 15 grains, it utterly fails to stimulate the liver. Its constitutional action has been excited slowly and rapidly by means of corrosive sublimate with a like result. In poisonous doses it produces a marked diminution in the flow of bile. In all these varied attempts, carefully repeated, under every varying circumstance that could be thought of, no evidence was obtained that mercury acted specially upon the liver at all. The exact measurement of all the bile secreted in eight dogs, first without and then with mercury, tends rather to show that, so far from increasing the flow of bile, it causes its diminution, through its general depressing action on the entire organism. This fact seems now to be so certain and thoroughly established, that the Committee consider it unnecessary to make any further researches on the subject." *

In the corresponding Report presented to the British Association for the Advancement of Science, at its meeting at Norwich, 1868, the methods of performing the experiments are related; all details are given; and additional experiments with *podophilline* and *taraxacum* are described. The conclusions drawn from these experiments are that podophilline diminished the biliary secretion, and that taraxacum did not influence the biliary secretion in any way whatever. The Report, in conclusion, observes that—

"On this and many other topics connected with therapeutics, what we require are not unfounded assumptions and vague speculations, but positive knowledge based on

* *Medicine in Modern Times*, p. 229. 1869.

unquestionable data; these we have furnished, and consider them amply sufficient to demonstrate the fallacy of the opinions everywhere prevalent as to the cholagogue action of mercury."*

These extracts are long, but I trust their importance will justify me in giving them; and they will make the conclusions I have presumed to draw from the experiments of the Edinburgh Committee, and from their inferences, intelligible in few words:—

1. Mercury *does act locally* upon the liver; the result of the action is a diminution, and not an increase in the secretion of bile.

2. *This* action is the one which takes place in *healthy* livers.

3. Prof. Bennett is not entitled to infer from this action in health what may be the action in *unhealthy* livers. There is nothing in the experiments to show that this may not be the *opposite* of its action in health.

4. Experiments in disease are necessary, as well as experiments in health, to learn the action of drugs.

5. There is evidence, from experiments on the sick, that mercury increases, that is, restores when it is deficient, the secretion of bile, in some cases of disease. This may, and probably does, arise from the restoration of a more healthy state of the organ.

6. The conclusion drawn from these experiments by the committee, or the principal fact ascertained by them in reference to the action of mercury, is directly in accordance with the teaching of homœopathy and organopathy concerning the action of drugs.

Other experiments upon animals are being carried on at present. One series is conducted by Drs. A. Crum Brown and T. R. Fraser, the object of which is to prove the connection between chemical constitution and physiological action. Numerous experiments have been made upon rabbits and frogs. Another series is conducted by Dr. B. W. Richardson, partly with the view of introducing new anæsthetics, but mainly with "the idea of studying the action of substances which are to become remedies,

* *Report of the British Association for the Advancement of Science.* Norwich, 1868.

not by the old and faulty method of so-called experience, but by proving physiological action and the relation of chemical constitution to physiological action." With the same object, therefore, as that of Drs. Brown and Fraser.

Dr. Richardson is very bold in his expectations. He says:—

"I am certain the time must soon come when the books we call 'pharmacopœias" will be everywhere reconstructed on this basis of thought, and when the chemist and physician will become one and one."

He even expresses the earnest hope that this "huge reform" will be commenced by Act of Parliament.

Enough has been said in the chapter on chemistry to damp such expectations as these, but all can sympathise with Dr. Richardson in the concluding words of his Report:—

"We cannot pretend, in reports like these, to vie with our more fortunate brethren in other departments of science. The physiologist has no ground of pleasant work in common with the astronomer, the geographer, geologist, ethnologist, or chemist. His researches are hard (unrelenting, I had almost said), excessively minute, laborious, and at all times, however absorbing, painful; many of them can, in fact, only be carried on under a sense of duty amounting to necessity, and with the sincerest, the most solemn feeling that they are being conducted for the ultimate benefit of all the higher classes of animal existence. In the preparation of this report I have held on throughout by this sense of duty, and earnest faith that good must come out of the labour." *

11. *Experiments on the sick.*

These bring us to a debating club of contention. In every age, but especially in ages of freedom and activity of thought, partly from good motives and partly from bad ones, a hot warfare of disputation has been carried on, and it has been continued without other change than that which fashion has made in the means in use. It is commonly asserted of opponents that they kill their patients, whether the doses given are poisonous on the one hand, or infinitesimal on the other.

They entangle us in a labyrinth of confusion. There is

* *Report of the British Association.* Exeter, 1870.

just light enough to make its darkness and chaos visible. The very various means which have been pressed into the service ; the very different quantities of those means which have been made use of; and the multiplicity and diversity of the effects which have been produced by them—and this during a period of more than two thousand years—are more than enough to bewilder and discourage the most sagacious and painstaking student. Many times the same medicines are given in the most dissimilar diseases, and with opposite intentions ; and yet hypotheses are never wanting to explain and vindicate these contradictory prescriptions.

They usher us into a chamber of horrors. On entering it we see first the "helleborism" of the Greeks; on looking round, one set of violent measures presents itself after another, till we end with the intoxicating stimulants of the present day. It has one great redeeming character—the chamber is paved with good intentions. But the traditionary reference of this character to another place ought to teach us the stern lesson that good meanings and wishes do not justify bad works; and that, therefore, it behoves us all to see to it that we be found, not only influenced by good intentions, but also wisely practising right things. We are to remember that the anxiety of the patient for a cure, and the desire of the doctor for success, though they have encouraged, do not justify the frightfully painful, and not unfrequently fatal experiments which, in every age, have been performed upon the sick.

What are experiments on the sick? Every dose of medicine which is given to a patient is an experiment. This is self-evident. A grave responsibility, therefore, rests on the physician who prescribes them, and he ought to be able, on every occasion, to give a sufficient reason for their administration.

Let us enquire what occupies the mind of the physician when he prescribes a dose of medicine.

His mind has been running in one of three grooves. The first has been cut out for him by hypotheses ; and so deeply has this been cut, that a mind once fairly in it cannot, without the greatest difficulty, be got out of it. These hypotheses extend to the nature of the case, to the indications supposed to be furnished by the symptoms, and to the properties supposed to be possessed by the medicines. These hypotheses are speculations congenial

to man's nature (Lord Bacon's idols of the tribe), or they are pleasant reveries of the individual man (idols of the den), or they originate in intercourse with other men (idols of the market), or they are the teachings of popular professors (idols of the theatre). How these hypotheses have prevailed for a season, how each in its turn has been supplanted by another, how erroneous and mischievous they have all been, has, to some extent, been considered already in this Essay.

The second groove is that of empirical experience. This leads the practitioner simply to give again that which has been given before in cases presumed to be similar. When we reflect how wanting in plain evidence of success the giving of medicines has hitherto been, and consider what a multitude of diseases occur for which we have no known remedies at all, it becomes painfully conspicuous that the empirical method is defective and unsatisfactory beyond description.

The third is the groove of enterprise. This tempts the earnest-minded physician to try some new thing. It is not wonderful that the dissatisfaction arising from the actual condition of medicine should urge men forward in search of something better. And so new drugs are tried, or old ones are tried afresh, after a random manner, without rule or principle, in the hope that, by chance, some better remedies may turn up. In this way, while I was a student at Guy's and St. Thomas's Hospitals (then adjoining each other), Dr. Elliotson, at the latter, gave large doses of carbonate of iron (rust) for some time to every patient he had in the hospital, in the hope that it would cure some of them.

After all, very little has been learned, during two thousand years, from all these experiments upon the sick :—

Because pure observation has been clouded and distorted by the hypotheses of the speculating physician, or has been made vague and objectless by the want of a principle to guide the empirical practitioner.

Because, almost always, several drugs have been mixed together in the same prescription, and given at the same time; so preventing the effects of each being distinguished. And

Because the symptoms or effects of the drug given to a sick man are necessarily obscured by being complicated or mixed up with the symptoms or effects of the disease.

To have a distinct notion of what experiments on the sick have been, it is necessary to go into some details; and yet the subject is so vast that it is not easy to select any details which shall be sufficiently brief, and at the same time so illustrative of the whole as to give a distinct notion.

Perhaps this can be best done by selecting a single disease, and by going briefly, but with care, through the history of its treatment. For this purpose, I think, *gout* may be chosen as a representative disease. This also is an extensive subject; so that its history must be limited to the last two centuries, and to English writers.

Let us begin with *Sydenham*,* who tells us, in 1683, in the introduction to his Treatise on the Gout, that he had himself been a great sufferer from it for thirty-four years.

As to the nature of gout Sydenham is very explicit. He says:—

"Its only forerunner is indigestion and crudity of the stomach, of which the patient labours some weeks before."

"The more closely I have thought upon gout, the more I have referred it to indigestion." This indigestion he calls the *antecedent cause*.

The result of the indigestion is a foreign product in the humours. This he calls the *causa continens*. It is now called the *materies morbi*.

On the treatment he says:—

"In gout but three methods have been proposed for the ejection of the *causa continens*—bleeding, purging, sweating. Now, none of these succeed."

"Bleeding is clearly contrary to that indication which is required by the *antecedent cause*, which is indigestion."

"Sure I am that all purging above or below, mild or sharp, is mostly injurious."

"Evacuation by sweats, although less mischievous than the other two forms, is still prejudicial."

"I confidently affirm that the greater part of those who are supposed to have died of gout, have died of the medicine rather than the disease."

"If evacuants are out of place in gout, what are the

* *Tractatus de Podagra et Hydrope*, per Thos. Sydenham, M.D. 1683.

indications of treatment? Two points are most particularly to be considered. The first is the *causa antecedens*, or the indigestion of the humours. The other is the *causa continens*, or the heat and exæstuation of the same, when they have become putrid and acrid. These two are as far as the poles asunder. What helps one hurts the other. Hence the difficulty of treatment. If we strive by heating medicines to subdue the indigestion, we run the risk of inflaming the humours; whilst moderate diet and cooling medicines, which allay the heat and acridity, cause indigestion, and impair the natural warmth."

But as medicines must be given, the following are recommended as *digestives*:—roots of angelica, elecampane, leaves of wormwood, lesser centaury, germander, ground-pine, &c., &c. And the *antiscorbutics* horse-radish, scurvy-grass, water-cress, with the remark that these last are too acrid and pungent.

Then a prescription is given containing *thirty* simples, with this very interesting comment:—

"Different species of these herbs, in the form of a skilful mixture, do better than any particular ones alone. However much the rule of *the simpler the better* may apply to *specifics*, as often as we purpose to cure the patient by satisfying any particular *intentions*, a variety is best."

It need scarcely be pointed out how remarkably this vindicates by anticipation the single medicine of homœopathy; each medicine which is given according to this method, having the character of a specific.

Sydenham says no external remedies are known; and then adds: "We must look beyond medicine." For, "in gout the cause is a change and new nature of the system;" and to be cured the patient "must change his whole habit of body." This is to be attempted mainly by diet and exercise. "Moderation in meat and drink, so that the stomach receive no more food than it can digest." "The other extreme, as I have found in my own person, is equally injurious." "The palate of the patient must be consulted."

Salt and spices are injurious; he is not to take supper, bed being "for the digestion of the humours, not for the concoction of the food." But a free draught of small beer may be taken, for this is an excellent preventive of renal calculi. "It cools and washes out the kidneys."

A milk diet, he says, has prevailed for the last twenty

years. It does good while continued, but when left off the gout returns worse than ever.

London small-beer, hopped or without hops, is the best drink; if this cannot be taken, then weak wine and water.

Wine does harm, and increases the pain. Water is bad, unless taken always through life. Over-cooling draughts do not cause pain as wine does; they cause death.

If wine has been long taken it must not be left off suddenly and entirely. Sherry is better than Rhenish or French wines.

Late hours are bad—next to bleeding and purging.

Tranquility of mind is essential.

But far above everything else is bodily exercise; but unless taken daily, even exercise will be useless. This also must not be excessive.

The exercise of driving should be taken *in* the fits; riding on horseback, except in old age or when there is calculus, *out* of the fits.

All these things, however, will not absolutely prevent the recurrence of fits. A radical cure of gout "lies, like truth, *at the bottom of a well.*" "Some such remedy may at some future time be discovered."

The very great value of these remarks of Sydenham will, I hope, be a sufficient apology for the space they have occupied. Nothing could more distinctly show the failure of drug treatment, up to that time, of a disease only too well known in every age of civilised life.

The next author to be noticed is *Dr. Cheyne*,* who, in in 1721, advances a step further in the discovery of the nature of the *causa continens* of gout. He declares it to consist in tartarous and urinous salts introduced into the blood by the food; the former from the wine drank, the latter from the animal food eaten. Dr. Cheyne's hypothesis is that in gouty persons the capillaries or "smallest vessels are narrower and more stiff or tense than those of others." That a fit of gout is caused by the obstruction which these salts meet with in the capillaries where they are compressed in the joints, and thus pain, inflammation, and fever arise from "an effort of nature to throw off these salts through the stiff and narrow strainers."

* *An Essay on the Gout*, by Geo. Cheyne, M.D., F.R.S. 3rd. Ed. 1721.

"Hence it is evident there can be only two direct ways of treating the gout with any prospect of success. The one is by stretching and widening the capacities of the small vessels, and relaxing their fibres. The other is by lessening the quantity of the salts introduced into the fluids by the food."

The first intention can be best answered by "wisely managed exercise"—not too violent, and "moderation in eating and drinking being joined with it." And

"There be two distinct ways of lessening the quantity of the salts which produce the gout. The first is, by a total abstinence from, or a great abstemiousness in flesh, fish, and strong liquors, which introduce those salts into human fluids. But this requires great caution, because an entire vegetable diet weakens all the digestive powers and all the functions of life; and because this diet once entered upon is never after to be changed under the danger of certain death."

"The other direct way of banishing the salts out of the habit is by evacuations. Sydenham, otherwise a most accurate observer of nature, and a most judicious practitioner, has been the occasion, I think, of a great mistake in the management of the gout, by forbidding almost all evacuations."

"The secondary or less direct methods of relieving the gout are first, dilution by proper liquors; secondly, strengtheners of the instruments of the digestion." The first are Bath and other waters; dwarf-elder tea; trefoil tea; light, quick, green tea; and small spicy bitters in water.

For the second, "*the Jesuit's bark*, (cinchona) in generous claret is the most powerful strengthener of relaxed fibres in the instruments of digestion and the greatest antidote of the urinous salts." *Chalybeates* and the "*glans quercina* or acorn," may be added.

"*Mercury*, by its weight, seems to offer fairest for breaking the gouty salts, for relaxing the fibres, and for enlarging the small vessels, and the fact is, that by a full and free salivation gouty people have been freed from all its symptoms for several years. But it is also matter of fact that the body becomes in a worse state in respect of the future fits, than it would have been under the common symptoms; the man is seldom or ever the same as he was before the salivation."

During the fit several medicines are to be given, "such as Gascoin powder, Goa stone, bezoar, Sir Walter Raliegh's cordial, diascordium, confection of Alkermes, and the like."

In the intervals a succession of specifics are to be administered. The first is,

"*Rhubarb*, taken regularly, once, twice, or oftener a week, in such dose as to procure two or three motions." "It is worth taking notice that all the preparations of this medicine serve only to spoil it."

"Next to rhubarb, and even far before it, I would recommend *sulphur*. It is but in little use at present in physic, except in the itch and the piles, and yet in the whole extent of the *Materia Medica* I know not a more safe and more active medicine."

"I have known half a drachm of powdered sulphur, taken regularly twice a day, in a spoonful of milk, prevent the fit for many years, and lessen both its pain and duration when it happened, for it moved the body gently once or twice a day." "Especially if to these (rhubarb and sulphur) be added plentiful dilution by some blood-warm infusion of a spicy and diuretic plant in water, so as to provoke a gentle breathing sweat, and pass freely by urine. Thus large draughts of sage, dwarf-elder, buck-bean, or green tea; but especially of weak whey made on old Mountain, drunk blood-warm, and on an empty stomach, and joined to any of the now mentioned medicines."

"But beyond all other things, a well ordered course of *Bath* waters, with chalybeates, and warm bitters, and a frequent and regular use of stomach purges, will be found to succeed best."

"For a concluding observation," Cheyne, like Sydenham, says:—

"Temperance only, divine, innocent, joyous temperance, can cure or effectually relieve the gout. For let us, or our brethren the quacks, brag what we will,

"Tollere nodosam nescit medicina podagram."

It would have been well for the sufferers from gout, if the moderate counsels of Sydenham had never been further departed from than they were by the rhubarb and sulphur of Cheyne. But this did not happen. The more violent measures continued to be the favourite plans.

Dr. Dover,* in 1732, gave the result of forty-nine years of practice, as a legacy to his country. His treatment of disease is not unfairly represented by a patient who was "very weak in a consumption," and who recovered after having been "blooded at least fifty times;" and by his favourite prescription, an ounce of quicksilver to be taken every morning, for a month, as "the most beneficial thing in all the world."

On Gout, he says:—

"There have been so many unsuccessful attempts made to master this disease, that patients have very little faith left, and, as they commonly say, have no hopes from any thing but patience and warm flannel: but with submission, keeping the part warm is wrong, because it is *proprium caloris attrahere;* and does, beyond doubt, attract gouty matter to the part."

He then prescribes tamarinds, senna, rhubarb, manna, purging syrup of roses, syrup of buckthorn, and elixir proprietatis, with posset-drink between the motions; and opium, saltpetre, tartar vitriolated, ipecacuanha and liquorice on going to bed—"covering up warm and drinking a quart or three pints of posset-drink while sweating."

"Mynsycht's elixir of vitriol taken often in large quantities, most certainly destroys gouty matter, yet for some time it may cause pain; but taken in its due latitude, if water will quench fire, it must in the end have its desired effect."

Dr. Cadogan† (1760) and his opponents‡ appear next in a very lively discussion on gout and its treatment. I must content myself with a single extract from his book, as a specimen of the style in which the subject is handled:—

"The gout is so common a disease that there is scarcely a man in the world, whether he has had it or not, but thinks he knows perfectly what it is. So does a cook-maid think she knows what fire is as well as Sir Isaac Newton. It may therefore seem needless to trouble ourselves to say

* *The Ancient Physician's Legacy to his Country.* 1732.

† *A Dissertation on the Gout,* by William Cadogan, Fellow of the College of Physicians. Sixth edition. 1771.

‡ *Observations on Dr. Cadogan's Dissertation on the Gout,* by William Falconer, M.D. Second edition. 1772. &c.

what it is: but I will venture to say what I am persuaded it is not; though contrary to the general opinion. It is not hereditary, it is not periodical, and it is not incurable."

*Cullen** in 1780, after trying all plans and meeting with nothing but disappointment, fell back upon "patience and flannel alone." His discouraging experience makes him say:—

"I am much disposed to believe the impossibility of a cure of the gout by medicines; and more certainly still incline to think, that whatever may be the possible power of medicines, yet no medicine for curing the gout has hitherto been found."

Heberden,† who was at the head of his profession in London while Cullen was flourishing in Edinburgh, on the contrary, writes, in 1782, against flannel and rest, and says:—

"I have known several, who instead of nursing a beginning gout with warmth and repose, have used the utmost resolution and exertion in moving and exercising the limb, which they found themselves gradually able to do more and more, till at last they recovered its perfect use, free from any feelings of pain, and without any manifest ill consequences."

"The great Dr. Harvey, as I have been told by some of his relations, upon the first approach of gouty pains in his foot, would instantly put them off by plunging the leg into a pail of cold water.

"I do not recommend Dr. Harvey's example as proper to be imitated, though it is known he lived to a good old age; but I am not warranted by any experience to condemn the practice of endeavouring by exercising the limb to prevent the gout from settling there." "I never could see any reason for adding at all to the usual covering of the limb." Heberden gave a variety of stomach medicines, but he adds, "we are still greatly in the dark about the causes and effects of gout, and the right method in which it should be treated."

* *First Lines of the Practice of Physic*, by William Cullen, M.D. Ed. of 1812.

† *Commentaries on the History and Cure of Diseases*, by William Heberden, M.D. Second edition. 1803.

*Mason Good,** who was also himself a great sufferer from gout, contends, in 1822, that when the constitution is otherwise healthy and vigorous, what is called the antiphlogistic treatment may be fully carried out, without fear of a metastasis to an internal organ. For several years he obtained, in his own person, great benefit from the external use of cold water; but afterwards, as his general health became weaker, he confined himself to the wine of colchicum.

Dr. Todd,† in 1843, prescribed moderate purging with blue-pill and salines, as Epsom salts, and *alkalies*. In sthenic gout colchicum in small doses, *so as not to excite nausea, vomiting, or purging*. In 1851 he gave opium and sesqui-carbonate of ammonia, with free counter-irritation by mustard and turpentine and blisters, and says, "*lemon-juice* is a valuable remedy."

Dr. W. Gairdner,‡ on the other hand, in 1849, thinks that the watery evacuations of neutral salts are injurious, and that the warm aperients are far better—such as senna, rhubarb, aloes, jalap, and scammony, with warm aromatics, which is consistent with the older writers. But he also recommends small bleedings, which he says, "act as a tonic!" With regard to colchicum, he agrees with Dr. Todd, that it "never more effectually relieves the patient than when it acts *silently and peacefully*, without producing any evacuations whatever, or in any way disturbing the patient's comfort and ease."

Mr. Anthony White§ approaches to Sydenham as a writer on gout. In 1818 Mr. White was surgeon to Westminster School, where I had the pain of knowing him, for he ordered Mapleson to cup me, and followed the cupping with a blister, for inflamed eyes from hooping-cough, when I was a boy in the school; he was afterwards President of the Royal College of Surgeons. In 1848 he had been a subject of gout for forty years. He gives us

* *The Study of Medicine*, by John Mason Good, M.D., F.R.S. Third edition, edited by Samuel Cooper. 1829.

† *London and Edin. Medical Journal*, 1843. *Medical Gazette*, 1851.

‡ *Medical Times*, 1849.

§ *On the Nature and Treatment of Gout*, by Anthony White, Esq., *Medical Gazette*, 1848.

in his paper, first, "the actual state of our knowledge as to the intimate nature of gout," from Dr. (now Sir Henry) Holland. This is—1. That it is hereditary. 2. A *materies morbi*. 3. Has a relation with lithic acid and the calculous diathesis. 4. Is in the blood. 5. An attack is the removal of this matter. 6. Both liver and kidneys are implicated.

Mr. White considers the cardinal principle to be "a *materies morbi* circulating with the blood." He thinks Dr. Holland is wrong in blaming the kidneys; on the contrary, their action is beneficial in removing the offensive matter. He thinks the gouty poison is not identical with lithic acid, because the paroxysm may occur without excess of this acid in the urine and *vice versâ*. (We shall see this explained by Dr. Garrod.) But the remarkable fact in Mr. White's personal experience is the part played by the *liver* in his case. Whenever he adopted the patience and flannel method, the fit always ended in a violent discharge of bile from both the stomach and bowels. This he could always prevent by taking four medicines, viz., calomel, colchicum, aloes, and ipecacuanha. It was very natural for him to conclude that the principal seat of the gout is the liver. It was so in his case, and such a case may therefore be met with again. But it remains true that Sydenham's experience is much more common, and that the stomach is generally the organ chiefly disturbed.

We now come to *Dr. Garrod*,* whose analyses of the fluids in gout have been persevered in for so many years, and who seems to have told us what the *causa continens* of Sydenham, the *materies morbi* of Sir Henry Holland, the gouty poison of every body, really is.

In 1848 Dr. Garrod ascertained that the *blood* in gout always contains uric (or lithic) acid; that it is diminished in quantity, or is absent in the urine *before* a fit. So that the failure in its excretion by the kidneys seems to be connected with the accession of a paroxysm, when the uric acid is thrown upon the joint. No uric acid is found in the blood in rheumatism.

In 1858 Dr. Garrod is still more definite in his announcements.

* *Medical Gazette*, 1848. *Medico-Chirurgical Transactions*, 1848, &c., &c.

In acute gout ;—

1. The urine is small in quantity, and the uric acid contained in it is diminished.

2. As the attack is mitigated, much larger quantities are passed.

3. The uric acid becomes again less, but not so little as at the beginning.

In chronic gout :—

The uric acid in the urine is very much diminished ; there is a small amount of albumen ; the urea remains as in health.

Between the attacks :—

The uric acid excreted is less than in health.

As to the influence of *colchicum* (between 50 and 60 analyses) :—

In healthy cases either a slight diminution of urine and uric acid ; or a notable diminution of urine, with an increase of uric acid.

In recovering cases, no positive change ; or both urine and uric acid diminished.

The conclusions of Dr. Garrod respecting the action of colchicum are :—

1. That there is no evidence to prove that colchicum produces its effects upon the system by causing an increased excretion of uric acid.

2. That colchicum is not always a diuretic.

3. That colchicum has no marked influence on the urea.

Dr. Garrod's treatment in 1859 was as follows :—

In acute gout—to give some simple *alkaline* saline with moderate doses of colchicum ; if necessary, purgatives, and to take away a few ounces of blood. If the patient is low, sesquicarbonate of ammonia, and no colchicum ; cotton wool and oiled silk, and a small blister, with amylaceous diet and diluents.

In chronic gout—to augment the various secretions ; restore the digestive organs ; attend to the local mischief ; to regulate carefully the diet ; and to give a new remedy, *carbonate of lithia*, which forms soluble salts with uric acid.

Mr. Alexander Ure,* in 1841, proposed as a chemical

* *Medico-Chirurgical Transactions*, 1841.

remedy, *benzoic acid.* This forms hippuric acid and hippurate of soda in the urine, in the place, as he supposed, of uric acid and urate of soda. In 1849 Dr. Garrod, after giving benzoic acid, found, indeed, hippuric acid in the urine, but that the amount of uric acid was not thereby altered. The effect of the fixed alkali *lithia* has not yet been sufficiently examined to be fully ascertained; but Dr. Jas. Duncan* says, in 1865, that dilute hydrochloric acid with cascarilla is of more service than potash or lithia water.

The contradiction on the subject of an alkaline, or an acid treatment, is very remarkable. Dr. Todd in 1843, Dr. Garrod, and others strongly advocate alkalies; Dr. G. O. Rees, Dr. Todd in 1851, and Dr. Wm. Moore, give lemon-juice, and Dr. Duncan, as just noticed, hydrochloric acid. Monsieur Trusseau (if I may mention a continental writer) contends that as to alkaline preparations, such as Carlsbad, Vals, and Vichy waters, there is not a more dangerous medication in the world. On the other hand, he does not advocate acids, but Peruvian bark, quoting a sentence of Held, who said, "Uno verbo, cortex Peruvianus in podagrâ divinum est remedium." He calls flannel an evil habit, and recommends washing in cold water in summer, wet sheets, &c., to accustom the body to resist cold.

Amid all this confusion and contradiction there is but one thing respecting gout in which all authors agree, namely, that it rarely, "if ever," occurs in young persons. Perhaps, therefore, the following fragment of personal history may be sufficiently interesting to justify its introduction. My family has been subject to gout. William Sharp, my uncle, whom I succeeded as a surgeon at Bradford, died in about two hours from gout transferred from the ball of the thumb to the stomach. While on a visit to this uncle I had a fit of gout in the great toe, *when about ten years old,* and have never had another. This entire freedom since from gout I attribute, under God, to the abundance of exercise taken every day. Sydenham preferred riding on horseback; this I practised for sixteen years, but was compelled to give it up from the indigestion it caused; since that time all my exercise has been on foot. It may perhaps be useful to add that

* *Dublin Quarterly Journal,* 1865.

though always living temperately, I have never lived abstemiously, unless drinking only water till I was twenty-five be considered abstemious living; and that I have been greatly indebted to *pulsatilla* as a remedy for indigestion.

On looking back upon this brief survey of our medical knowledge of gout, a crowd of reflections force themselves upon the mind. Some of these must be noticed before the subject is left.

The hereditary predisposition does exist in some families.

It may be successfully kept in abeyance.

Full living, indolence, and vexation may induce gout in any one, whether predisposed to it or not.

The peccant matter, gout poison, or urate of soda, should be viewed as a product of the disease, rather than as the *materies morbi* or disease itself.

There is a previous derangement of the digestive organs, particularly of the stomach, more rarely of the liver.

It is of less consequence to treat cases of gout as acute or chronic, than to consider whether the patient is in an entonic or atonic condition.

To eliminate the poison of gout should not be the primary object of treatment.

Treatment by evacuations of any kind is a mistake, and in the end does harm.

Chemical treatment, up to the present time, is a failure.

Dr. Garrod's analyses, confirmed by Dr. Harley's, are positive as to the presence of uric acid or urate of soda in the blood of gouty persons; but they are negative as to the action of colchicum upon these substances.

Colchicum is as useful in rheumatism as in gout; and there is no uric acid or urate, but lactic acid, in rheumatism.

The manner of acting, therefore, of small doses of colchicum, is as entirely unknown to us as is that of all other specifics.

The treatment which does most good is specific treatment, which is *silent and secret.* We do not know its nature now, and I believe we shall never know it.

The specifics for the relief of the gouty paroxysm which we know are bryonia, colchicum, rhus, and cinchona.

There are probably others, such as ash-leaves or seeds, germander (teucrium chamædrys), ranunculus, winter-cherry (physalis alkakengi), veratrum viride, piper methisticum.

The experience of Drs. Todd, Gairdner, and Garrod, as to the efficacy of the "*silent and peaceful*" action of colchicum, is strong testimony in favour of homœopathic or specific treatment.

The advice to regulate or augment the secretions is very plausible, but it is fallacious. If we cure disease the secretions will regulate themselves.

To restore the functions of the digestive organs to a healthy state, in the intervals of the fits, should be the first object in the treatment of gout. *Pulsatilla* must not be forgotten in cases like Sydenham's; nor *chamomilla* in a case like Mr. Anthony White's.

Much may be done by specific medicines; but even these must in the end fail, unless they are seconded and supported by a suitable manner of life as regards food, exercise, and peace of mind.

This history of experiments on gout is a fair sample of experiments on the sick. And it is a sufficient proof that these experiments, which have been carried on for three-and-twenty centuries, have failed to establish a principle for the treatment of disease by medicines.

12. *Experiments on the healthy.*

The truest vindication of these experiments, and therefore the most becoming introduction to the subject, is contained in the first sentence of "The Great Instauration" of Lord Bacon:—

"Francis of Verulam
thought thus—

"Of the state of learning.—That it is neither prosperous nor greatly advanced, and that *an entirely different way* from any known to our predecessors must be opened to the human understanding, and *different helps* be obtained, in order that the mind may exercise its jurisdiction over the nature of things."

If we substitute the word "medicine" for "learning," the sentence will still be true of this branch of knowledge. It is the unanimous confession of those best acquainted

with medicine, that as a science it is "neither prosperous nor greatly advanced;" though it has not hitherto been their conviction that "an entirely different way" must be opened, if it is to make any great advance. Without experience to the contrary, it would have seemed natural that such a conviction would necessarily follow the confession. But this has not happened.

It is the truth of this sentence of Lord Bacon which justified Hahnemann, and which justifies us, in his and in our endeavours after the discovery of an entirely different way from any known to our predecessors.

And the way proposed and entered upon by Hahnemann, and pursued by ourselves, is the way of learning the properties of medicines by experimenting with them, not only on the sick, but also on the healthy.

As two Essays (on the "Proving of Drugs," and on the "Physiological Action of Medicines") have already been occupied with this subject, I will content myself, on this occasion, with offering only a few remarks.

1. Let us not aim at impossibilities. In undertaking these experiments on the healthy, it is of great importance that we limit our endeavours within the bounds of what is possible to us. If we neglect this precaution, much time and effort will be thrown away, and we shall reap disappointment. For example, if we propose to ourselves to find out the *manner in which drugs act*, we shall soon get out of our depth, and our labour will be lost. I know that some do not agree with me in this persuasion. The discussion of it cannot be entered upon at present, but I commend to those who differ from me on this point the following sentence of Sydenham :—

"However much, by seriously inclining our minds, we may discover what nature does, and by what organs she does it, the way in which she does it will always be unknown to man."*

2. Let us not stop short of what is possible to us. It is not surprising to find that the first efforts to make progress in a new path are defective. And many are now disposed to agree with me that the limitation of these experiments on health, by Hahnemann and his followers,

* *Sydenham's Works*, by Sydenham Society, Vol. II. p. 84.

to the enumeration of symptoms, is not doing all that it is needful to do. For if, on the one hand, it is a mistake to attempt what is beyond our power; it is, on the other hand, also a mistake to neglect to aim at doing all that is within our power. It has often been contended in these Essays, that when experiments with drugs in health are undertaken, besides the phenomena or appearances produced, called symptoms, being noted, the seat of these symptoms, or *the organs to which they belong*, should be noted also. It is freely granted that the connection between a symptom and its origin may sometimes be very difficult to trace; but it will not be argued that it is impossible to trace it—that it is beyond our natural powers; and if it be granted that it is within our power, it must immediately be granted that it forms part of our duty.

3. We are pursuing "an entirely different way from any known to our predecessors." It has been earnestly contended that, with very few exceptions, the only action of drugs which is required of them, as remedies in disease, is that which they perform "silently and peacefully." This is the action which was formerly called "alterative," and now "specific." These terms imply two things:—that it is unaccompanied by signs perceptible by our senses; and that we know nothing of the manner in which it is performed.

The general adoption of this method of prescribing drugs would bring about a greater revolution in the practice of physic than has ever yet been dreamt of. And yet it is obviously in the right direction. "Nature is pleased with simplicity," said Sir Isaac Newton, and "more is in vain where less will serve." All real improvements in art are in the direction of greater simplicity.

When it is considered that this method leads to the ignoring of all established indications; to the renouncing of all former intentions; to the laying aside all that is usually called "active" treatment; it is not surprising that it is very repugnant to the majority of the profession, or that from them it meets with very determined opposition.

To see, in our future works on Materia Medica, no more catalogues of drugs arranged under the heads of "emetics" and "purgatives," "diaphoretics" and "diu-

retics," "sialagogues" and "deobstruents," is an anticipation too astounding to be contemplated with equanimity—too impossible to be realised. But this is the future which we humbly hope is before the profession.

And when the prejudices of education and the power of present habits have been overcome by the force of truth and the evidence of facts, what a beneficial change will have been brought about! Medicines, instead of being nauseous draughts, will have become pleasant charms; and physicians, instead of being shunned and dreaded, especially by children, will be welcomed and loved.

This silent and peaceable action of medicines, secret and hidden from our knowledge as to its manner, but very visible in its beneficial effects, is the "entirely different way from any known to our predecessors" which it is our happy privilege to advocate and defend. Instead of all the perturbative methods of the past, this is the curative method of the future.

4. The experiments with drugs in health are the "different helps" which Lord Bacon says must be obtained, in order that this entirely different way may be opened.

We want to learn of a drug, not whether it is a purgative or a diuretic, but what is its specific action—that action which is a disturbing action in health, and a silent and peacful action as a remedy in disease. Experiments in health are *helps* in the acquirement of this knowledge. The specific action of a very few drugs has been discovered accidentally. The experiments in health which have already been instituted have helped to increase this number greatly; a continuance of them may reasonably be expected to add many more to the list, and to make our knowledge of them much more perfect.

5. The help which Hahnemann got from these experiments was obtained by observing the similarity of the *symptoms* produced by them to those of diseases; and he prescribed according to this similarity.

The help which I am now seeking to obtain is by observing the *seat* of the action of drugs, and its identity with that of diseases; so that a drug may be prescribed which has its action where the disease is principally situated.

There cannot be a question that this is more definite knowledge than that obtained by the mere observation of symptoms. And if more definite, then in the same proportion must it be more valuable.

6. When this identification of the seat of the action of the causes of disease, with the seat of the action of each drug, has to some extent been accomplished, a further *help* may be sought from experiments in health, by the discovery of the similarity and contrariety in the *kinds* of action of diseases and drugs.

After observing the symptoms, or signs of an action; and after discovering its seat, or the organs in which it is taking place; we may try to learn what sort of action it is. This is a further step in advance, and consequently the difficulties will increase.

In the study of nature our first task is the observation of facts. This belongs to our bodily senses, and the value of the performance of the task is determined by its accuracy. The difficulties which beset the duty, are the imperfections of our senses, and our hastiness in using them. Our next task is the interpretation of these facts, the learning their true meaning. This belongs to the mind. The difficulties which hinder its being rightly performed, are the weakness of our mental faculties, and the strong tendency which exists in us to invent hypotheses, that is, to guess at interpretations, rather than wait till the mind can observe them.

When, therefore, we endeavour to discover the kind of action which a disease or a drug is producing in an organ, we cannot exercise too much caution. The observations must be wary and prolonged to learn the facts; and it is absolutely necessary to confine ourselves to the use of words which simply express the facts observed, and which do not suggest any hypothetical explanation. For instance, if we see vessels become smaller or larger than they were before, instead of calling this a stimulant, an astringent, or a relaxant effect, let us be content to say of the vessels, as we say of the pupil, that they are contracted or dilated. The same caution is, if possible, still more imperatively required, when the mind attempts to interpret the meaning of the facts.

7. It may be noted in this place, that the fact which

lies at the foundation of the difference between the disturbing action in health and the peaceful action in disease, is the difference in the dose. Organs in a state of health are, generally, not disturbed by a dose which is found by experience to be sufficient to act upon them curatively in disease.

Different doses of the same drug sometimes act upon different organs of the body in health; but doses, smaller than those given in health, can always be found, which act upon the corresponding organs in disease.

The subject of doses is a wide and interesting field for dispassionate and very careful enquiry and observation; but it is one of extreme difficulty and obscurity. The motto for it Lord Bacon has given us in these words:—

"In all our investigations of nature we must observe what quantity or dose of the body is requisite for a given effect; and must guard ourselves from estimating it at too much or too little."

8. Let us also note, once more, the direct object and the use of these experiments in health. They are these:—

First. To learn the action of each drug by itself, unmixed with other drugs.

Second. To learn the action of drugs, uncomplicated with the symptoms of disease.

Third. To discover the specific action of each drug; that is, to learn the organs upon which it acts, and the kind of action.

Fourth. To apply these discoveries to the treatment of disease; experience having taught us that the same organs which are disturbed in health by certain doses, are silently and peacefully cured in disease, by certain smaller doses.

9. Let us observe the indirect uses of these experiments. One branch of knowledge can generally throw some light upon another, and this indirect use is not wanting in experiments upon the healthy. If we take the drugs which are known by experience to have a useful specific action in a particular disease, and make comparative experiments with them in health, we are sure to gain some information as to the seat or nature of the disease in question.

We have lately been studying gout. I may suggest, in illustration of the last remark, that if we were to take

up this disease, and examine the drugs given for it as remedies, it is obvious that, at starting, we must lay aside the remedies given on the evacuating plan; we must also lay aside such disputes as whether cold purgatives, like Epsom salts, or warm ones, like jalap, are to be employed; or, whether alkalies or acids are to be preferred; and, for the present at least, we must lay aside the tentative experiments of the chemists, such as the benzoic acid of Mr. Ure, the alkaline lithia of Dr. Garrod, and the phosphate of ammonia suggested by Liebig. Only the specifics remain; these may be taken, and a comparative examination be made, in order to discover what there is in common in their action in health. The reflex benefit would be a better knowledge of the nature of gout.

The drugs are such as bryonia, colchicum, rhus, and cinchona; pulsatilla, nux vomica, rhubarb, and sulphur. Let me propose as a subject of study, the question—How far do these drugs agree in their symptoms, and in the seat and nature of their specific action?

In the same manner the principal known remedies for other ailments may be studied, *e.g.*, ipecacuanha, sambucus, and arsenic, for asthma.

The curative action of even a single remedy in a disease, the pathology of which is obscure, may throw considerable light upon that obscure pathology, simply in consequence of the experiments made with the remedy in health. To my mind chamomilla has done this for some cases of diabetes.

10. Another indirect use of experiments with drugs upon the healthy. It awakens attention to the possible difference of cases whose symptoms are similar. It is a fact that cases of disease present themselves, the symptoms of which are so similar that they may readily be considered cases of the same disease; but the causes of which are so different that, to confound them in this way, would be to make a serious mistake. For example: Belladonna may be so taken in health as to produce all the symptoms of scarlet fever. This has often happened. But this similar of scarlet fever has never been communicated to others in the manner that real scarlet fever is. It is wanting, therefore, in the infectious or contagious element, which is the true cause of genuine scarlet fever.

11. It may be worth while to repeat that the information sought from these experiments is derived mainly from two sources. The more severe effects of drugs are learned from cases of poisoning, in whatever way brought about; the less severe from voluntary provings. On this account Christison on Poisons, and other similar publications, are very valuable, in a direction not contemplated by these writers on "Legal Medicine."

12. Some of the effects of drugs can be obtained by the topical application of them; and these become striking proofs of local action. For example: The pupil of one eye may be dilated to the uttermost by belladonna; and at the same time, in the same person, the pupil of the other eye may be contracted to the size of a small pin's head, by the *Calabar bean.* But generally the experiments are made by the drug being taken internally. In many instances the effects are the same, whether the drug is swallowed or introduced through a wound in any part of the body. On the other hand, of animal poisons, it is remarkable that even those which are most deadly when inserted by a wound, may often be swallowed without any injurious effect.

13. Many drugs act powerfully upon more organs than one; and the more there are of points of contact between the drug and the disease, the greater is the confidence with which it may be prescribed. For example: For a feverish headache belladonna may be given as a remedy. If, in addition to the headache, there is an inflamed conjunctiva, the belladonna may be given with increased expectation of good. If further, the throat is inflamed, the probability of success is still greater. If, to all these symptoms there is added a scarlet rash, the probability becomes almost a certainty; provided that the inflammatory condition of these several parts is not complicated with grave symptoms of some other kind, which may, in fact, be such as to change essentially the character of the case.

In like manner, a patient suffering from colic, or spasmodic pain in the bowels, may be relieved by nux vomica. If there are also cramps, or twitchings in the extremities, it will almost certainly succeed. So, in cases pointing to ipecacuanha, if, in addition to the other symptoms, there

is nausea or sickness, it may be given with great confidence. Again, some affections of the heart are cured by spigelia; the cure will be more likely to be effected, if there is also in the case neuralgic pain about the eyes.

14. These remarks cannot be concluded without the expression of regret that many provings of drugs have been published, in which a great want of seriousness and distinctness of object is apparent. Others are disfigured by a multitude of insignificant sensations and observations, calculated rather to bring disgrace upon the physician than benefit to the patient. The undertaking is one which should be promoted by every member of our body, but it should be warily done, with a clear purpose in the mind, with thoughtful gravity, with active suspicion of error, and with freedom from bias towards any foregone conclusion.

We are now able to answer the question with which this Essay commenced. How is the action of drugs to be discovered? Not in any of the ways hitherto commonly pursued; but in a new way. First, by experiments made upon ourselves and our friends while in a state of health. By these experiments we learn the power which drugs possess to *disturb* the health of the different organs of the body; and also how each drug may be characterised, and distinguished from all the rest. And secondly, by giving them to the sick under the guidance of the results thus obtained in health. In this way we learn their *healing* powers.

The only difference, in respect to the drug, between the experiments in health and the prescriptions in sickness, is the quantity or dose to be taken. In the experiments the dose must be large enough to produce symptoms of disturbance in some organs of the body. In the prescriptions the dose must be small enough to avoid causing such symptoms, but large enough to act curatively, though peacefully, upon the diseased parts.

May I without presumption conclude this Essay by addressing a few words of encouragement to those of my medical brethren who have toiled through a practical trial

of homœopathy, amidst much discouragement and obloquy; and also to those who are beginning to find themselves mistaken in the condemnation of homœopathy in which they have hitherto joined?

Our medical authorities, and the great bulk of our colleagues, headed by the late Sir Benjamin Brodie, have not been ashamed to speak of homœopathists and to treat them as impostors or fools; forgetting that they are men educated like themselves, that they have investigated Hahnemann's system with a free spirit and in a practical manner, and have conscientiously adopted what in it they believe to be true. It is a great and unjust indignity, and, unless it is repudiated, it will one day recoil with heavy severity upon themselves.

Let us not desire the evil day. Let us rest assured that it is sufficient for ourselves to know that the self-denial, discomfort, and pain undergone, and the amount of time and thought expended on such experiments in health, as were begun by Hahnemann, and have been carried on since by many others, are a sufficient testimony of our honesty; and that the successful results with which these labours have been crowned, are a good proof of at least an average amount of intelligence and sense.

While many have thus spoken disparagingly of the labourers, they have not been afraid to appropriate the labours. But the attempts which have been made, during the last few years, to introduce homœopathic remedies as new discoveries, or accidental observations, are surprisingly puerile. The authors must have forgotten the declaration which we have on the highest authority, "There is nothing hidden which shall not be known."

Happily, the tide is now turning. Some of our eminent men, who, through unacquaintance with the facts, were carried away by the strong current of condemnation, have gained this information, and are beginning to acknowledge their mistake.

These, I doubt not, will be followed by others, and in the end, useful truth will prevail and patient conscientiousness will triumph.

This useful truth may not be Hahnemann's homœopathy unaltered, but a system of Therapeutics springing out of it—a system matured by degrees, freed from all hypotheses, and founded upon a sure basis.

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