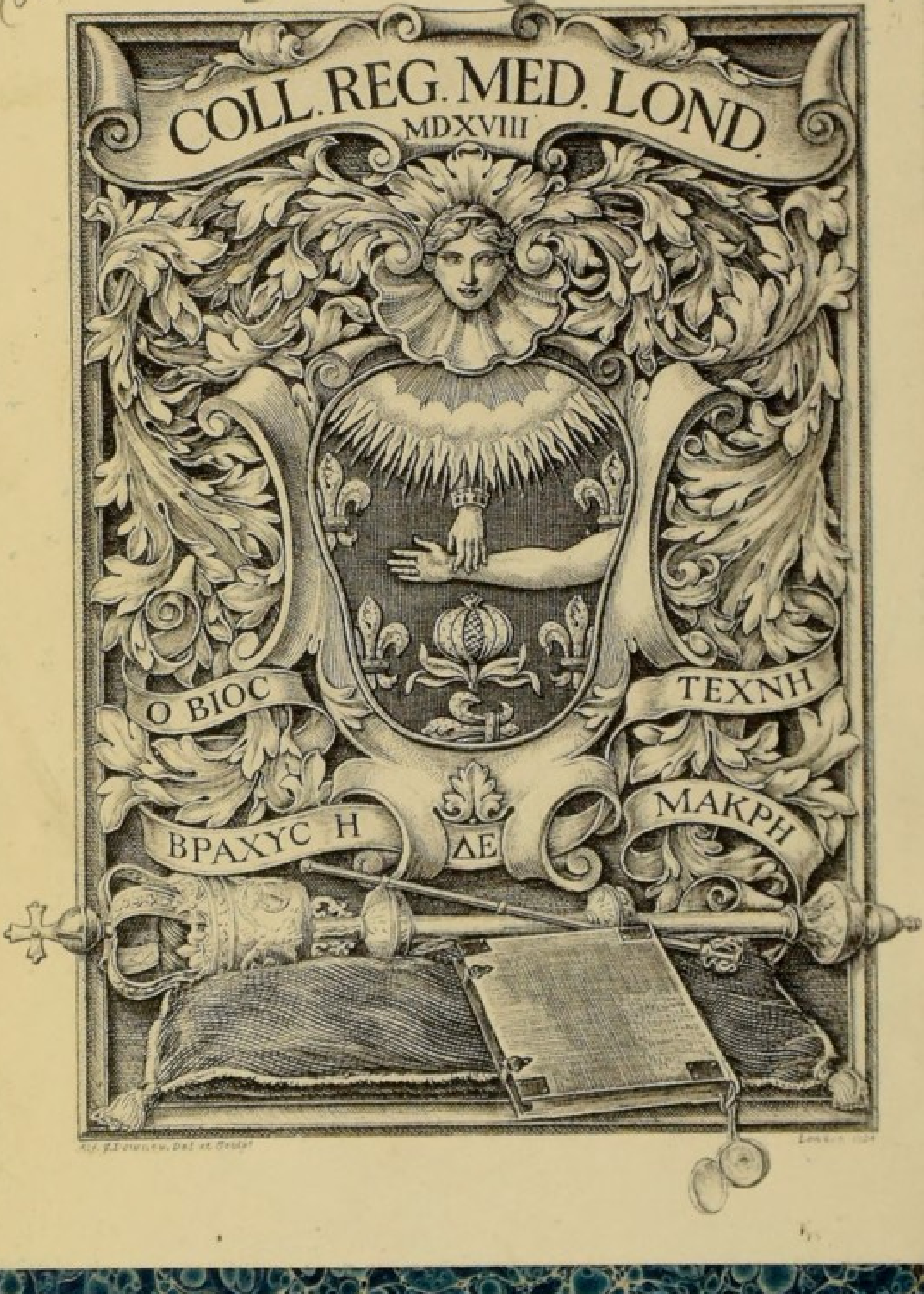
COLL. REG. MED. LOND.
MDXVIII
O ΒΙΟC
ΤΕΧΝΗ
ΒΡΑΧΥC Η
ΔΕ
ΜΑΚΡΗ

9a

sations, il en résulte, qu'elle n'a rien de plus certain que celle d'autres êtres, qui se manifestent également par leurs effets sur nous; et puisque nos observations sur nos propres facultés, confirmées par celles, que nous faisons sur les êtres pensants, qui animent aussi des corps, ne nous montrent aucune analogie entre l'être, qui sent ou qui pense, et l'être qui nous offre le phénomène de l'étendue ou d'impénétrabilité, il n'y a aucune raison de croire ces êtres de la même nature. Ainsi la spiritualité de l'âme n'est pas une opinion qui ait besoin de preuves, mais le résultat simple et naturel d'une analyse de nos idées et de nos facultés.'

# OUTLINES

OF

# MEDICAL PROOF.

REVISED AND CORRECTED,



WITH

REMARKS ON ITS APPLICATION TO CERTAIN
FORMS OF IRREGULAR MEDICINE.

BY

THOMAS MAYO, M.D. F.R.S.
FORMERLY FELLOW OF ORIEL COLLEGE, OXFORD.



LONDON:
PRINTED FOR
LONGMAN, BROWN, GREEN, AND LONGMANS,
PATERNOSTER ROW.
1850.



Price Two Shillings.

# OUTLINES

OF

# MEDICAL PROOF.

REVISED AND CORRECTED,

WITH

REMARKS ON ITS APPLICATION TO CERTAIN FORMS OF IRREGULAR MEDICINÆ.

BY

THOMAS MAYO, M.D. F.R.S.
FORMERLY FELLOW OF ORIEL COLLEGE, OXFORD.



LONDON:
PRINTED FOR
LONGMAN, BROWN, GREEN, AND LONGMANS,
PATERNOSTER ROW.
1850.

Wilson and Ogilvy, 57, Skinner Street, Snowhill, London

# INTRODUCTORY REMARKS.

"In contradictoriness and feebleness of reasoning the art of medicine exceeds every other." So said Michel de Montaigne; having certainly given much thought and attention to what had been said and written on the subject up to his time.

A few years ago, it occurred to me, at a well-known scientific club in London, to hear a distinguished member of it observe, that Dr. F—— was an eminent physician, and not that only, for that he *really* was a clever man. Time, thought I to myself, does not alter the tendency of society to look upon the medical profession with distrust. Eminence in it confers no presumption of intellectual power or capacity for reasoning. The medicus is still in juxtaposition to the magus, and professes a science which he can neither explain nor prove.

The considerations into which I have been led, while putting together the following pages, have recalled to my mind the tacit or open censure involved in the above expressions of opinion, with a mixed feeling as to what they are worth. They are unfair, and yet not ungrounded. Indeed, the apparently paradoxical remark of M. Littre, in the Révue des Deux Mondes, that the discoveries of one age are a source of retardation to the next age, unavoidably applies to medicine. For the occult nature of our subject matter implies that a new discovery must often be at variance with an old one, and not prove a development or addition to it. But there are other grounds for the above censure more within our own control; and these, I believe, would best be removed by our fashioning the minds of our junior members—first, on those principles of antecedent education which have been so admirably laid down by the present President of the Royal College of Surgeons; secondly, on those principles of objective inquiry and reasoning which I have endeavoured with more zeal than ability to shadow forth.

56, Wimpole Street,

Feb. 25, 1850.

# OUTLINES

OF

# MEDICAL PROOF.

## CHAPTER I.

Limitation and division of subject.—Sources of proof experiment and observation—Main source of medical proof properly so called; observation—Experiment how applicable; Sir Charles Bell, Dr. Williams—Induction; relations to it, quoad medical science, of observation and experiment—It may be non-hypothetical or hypothetical; general value of hypothesis, illustrated in the reformation of theory of cranial plenum by Dr. Burrows—Instances of non-hypothetical induction; Dr. Bright contrasted with Dr. Johnson, Dr. Jenner—Hypothetical induction of four kinds specially considered—Analogy—Instances of hypothetical induction, from cardiac disease, hepatic disease, reflex function—Inchoate inductions frequent; some worthless medical literature of that kind—Simple enumeration considered with relation to induction, decried by Lord Bacon as "res puerilis;" wherein useful.

The terms Pathology and Therapeutics are the most comprehensive expressions of the subject matter of medical proof; but these expressions indicate pursuits in which the physician participates with the general philosopher. The physician must, in different degrees, be anatomist, physiologist, chemist, and botanist. All these sciences may be possessed by the physical philosopher without his laying any claim to the character of a physician, from whom he differs in this respect, that these pursuits are followed by the physician with a view to an ulterior end, namely, the discovery, and the cure, and the prevention of disease. These remarks I make, in order to give intelligible limitations to the prospects which I may endeavour to lay open in the following pages. I do not concern myself with the proofs on which these, or any other science connected with medicine, rest, except so far as they concern pathology and therapeutics.

In the first five chapters I shall contemplate medical proof more peculiarly in its logical character, adducing its materials as illustrations; in the sixth chapter I shall offer some remarks on the distinctive character of the materials themselves, from which it may be most obviously drawn, or to which it may be most naturally applied.

---

Medical proof, or the proof applicable to the truths of pathology and therapeutics, must arise from mere observation, or from experimental observation, or I may say, for convenience, from observation or experiment.

Observation operates either on subject matter placed so far at our disposal that we can modify and alter it; or on subject matter, the phenomena of which we can collect and register, but cannot alter. Of the latter kind, the observations of the astronomer are instances. Those of the physician are obviously of both kinds; but while *he* possesses an advantage in his power of inducing changes on phenomena, he labours under the far greater speculative disadvantage arising from their inherent fluctuations.

Proof is said to be obtained by experiment, when we not only can alter and vary the subject matter, but also can investigate its properties by means of exclusions and rejections, per exclusiones et rejectiones. This "separatio naturæ," I use the language of the great philosopher of induction, duly carried out through a sufficient number of affirmative instances, enables us to draw conclusions, which *must*, if all the steps of the inquiry have been correctly made, be true. And it is to be observed, that these conclusions we can obtain only in the sciences, in which we participate with the physical philosopher, and which afford definite and fixed subject matter: whereas the clinical conclusions of pathology and therapeutics are subject to observation alone, and from the fluctuating nature of the phenomena must lay claim only to different amounts of probability.

Indeed, however desirable the kind of certainty produced by an experimental proof, it must be admitted that the method of observation is the main source of those principles which govern the practice of medicine, not only as obtained from the phenomena of disease, and the action of remedies, but as elicited from the auxiliary sciences above noticed. The very circumstances that these principles must be modified in their application by a reference to the mysterious laws of vitality, may account for this. It is difficult to obtain such a command over the living organism as to interrogate nature through the rejections and exclusions of experiments. Still we know that this has been occasionally done with success. The belief of Prochaska respecting certain functions of the nervous system, now called the reflex functions, was belief in a probability made out by observation. But the experiments originated by Sir Charles Bell tended to superinduce that certainty which belongs to their peculiar kind of evidence, on the above conjecture, by demonstrating the existence of the double roots of certain nerves, efferent and afferent. The "mera palpatio" of unmeaning and casual experiment, of course does not allow us to assume the credit of this kind of proof.

The proof that the second sound of the heart arises from an action of the aortic valves, is obtained by Dr. Williams through experiment: the separation and exclusion of every other part that can conceivably produce it without removal of the second sound, and the cessation of this sound on the action of the aortic valves being arrested, (supposing the experiment to be well conducted,) is final.

But the ultimate application to practice of our interrogations of nature, whether those interrogations are made through experiment or observation, must be of the latter kind; however the inquiry may have commenced, there it will terminate.

Now, when we are led to infer, from experiment or observation, the truth of a general proposition, the proeess which we have gone through is termed induction.* The inference obtained from an experimental induction is, as I have observed, final and conclusive; not through repetition of instances, but as soon as in any one instance, namely, the rejections and exclusions, have been realized. To the inferences from induction by observation we give admission proportionate in readiness to the number of instances by which it is supported, or to this element of proof combined with a consideration of the reasonableness of the inferences

* I do not enter upon a history of induction. My reader should have studied this in Herschel, or Whewell, or Mills, or in them all. I will, however, venture to suggest the following as, I believe, a fair exponent of its forces in relation to *my* subject matter.

INDUCTION BY EXPERIMENT.

The conclusion here arrived at, say, that B, *i. e.* some B, always exists in A, or is always present when A is present, owes its force to a process of exclusion and separation, whereby, a definition of A being assumed, everything has been abstracted that can be abstracted, A remaining. If once, exclusions having thus been made of all that can be excluded, A remaining, B is also left, then B always exists in A or is present when A is present.

INDUCTION BY OBSERVATION.

The conclusion here arrived at, that B always exists in A, or is always present with it, or the approximative proof of this, owes its direct force to multiplied and varied observations in which B is uniformly found coexistent with A, or present when A is present. I say direct force, because some expression is required to distinguish this ground of proof from that which a well selected hypothesis induces on it; as will be mentioned presently in the text.

on *à priori* grounds. These *à priori* grounds constitute what is called an hypothesis or theory.* In the first case, some conception of a law has been obtained; in the second case, the law is presumed to have obtained an explanation, and therefore an increase of probability. Not only a general fact, but the cause of that general fact, is suggested.

Let any one who desires to know how far the quality of the hypothesis, with which an investigation is set on foot, may determine its success, reflect upon the confidence with which we now generalise the pathology of dropsy, our observations being guided by the researches of Dutrochet; and then turn to the pathology of fever, formerly deduced from the indirect and direct debility of Brown. The hypothesis of Cullen was, perhaps, equally precarious with that of Brown; but *his* generalisations have a merit independent both of his hypothesis and his conclusions,—that, namely, of faithful description: his reasoning may be declined, but his semeiology will remain valuable. I may here observe, that it is in the structure of a good theory or hypothesis that we principally recognise the value of experimental induction. In this respect we are greatly indebted to Dr. George Burrows for his experimental proof of the state of the cerebral circulation, with reference to a presumed plenum of that organ, under certain conditions, which have been supposed to imply the absence of atmospheric pressure on the contents of the cranium. We are enabled to make an immense though negative use of these experiments in classifying cerebral disturbances; for we can now admit local plethora as an element of such classification, without any of the doubts and scruples which formerly beset us in assigning to it a place or influence. We are enabled to repose with our former confidence on the hypothesis of local fulness, as assigning causes to a form of cerebral disease.

But (to return from these general remarks) the inductive

* See note i. Appendix.

process by observation may originate in the discovery of common points in a series of phenomena, with a conception that this commonness of points is a law of the phenomena in relation to each other; or with an hypothesis which will involve both a conception of these common points, and the suggestion of a principle on which it depends.

We have an instance of an inductive process of the first kind commencing at once from observed facts, without any preconceived hypothesis, and contented with inferring an unexplained law, in the discovery of the Morbus Brightii. "It is now twelve years," says Dr. Bright, in 1827, "since I first observed the altered structure of the kidney, in a patient who had died dropsical. It was not, however, till within the last two years that I had an opportunity of connecting these appearances with any particular symptoms, and since that time I have added several observations." From this beginning a series of observations proceeds to the establishment of a very important practical division of dropsies, in relation to the principally affected organs.

It must here be admitted that a theory or hypothesis (in other words, a conception which presumes causes of some sort and is not contented with classifying,) is always adopted, if possible, as constituting the proposed end of an inductive process, and sometimes with some confusion as to relative degrees of merit. The discovery of Dr. Jenner belongs to the same category as that of Dr. Bright: it was immediately the result of his having had the good sense to pay attention to the young woman who came to seek advice, when he was pursuing his studies at the house of his master at Sudbury; and who, the subject of small-pox being adverted to in her presence, immediately observed, "I cannot take that disease, I have had cow-pox." The antecedent probability with which others and Dr. Jenner himself afterwards invested the discovered law, by finding out that small-pox belongs to a great family of disease pervading extensively the animal creation, pleased and satisfied many minds, who could not take up with the homeliness of non-hypothetical induction, but it did not assist, nor has it, I believe, since assisted in the smallest degree the actual verification of the discovery. The casual way in which this non-hypothetical induction sometimes commences, and often for some time proceeds, until some conception of common points binds the scattered facts together, may seem, unjustly indeed, to infer less of merit in respect to ingenuity than of good fortune, so far as the event of the induction is concerned. To return to Dr. Bright's discovery. The merit, indeed, mainly consists in the wisdom, which appreciated the importance of obtaining, by any means, a diagnosis between the organic concomitants of dropsy, the clear perception of certain concurring phenomena made known by autopsy, which many before Dr. Bright must have passed unnoticed, and the perseverance which converted a gradual process of observation into a great discovery. The affording by farther observations the explanation of such a discovery must generally rank after it: when the two attainments coexist in a given individual, they confer upon *him* the double merit attributed by Bacon to successful thought,—the being "luciferous and fructiferous." The merit of Dr. Johnson's suggestion as to the tightly-packed structure of the kidney under fatty deposit, as determining the altered secretion of urine in granular disease, (in the supposition that he has made out his case,) claims the first of these epithets; Dr. Bright himself has supplied a valuable instance of hypothesis in its relation to proof, when he accounts for the presence of chorea in pericarditis by the engagement of the phrenic nerve.

In the last paragraph I have noticed the use of hypothesis only in contrast with the non-hypothetical induction. I now proceed to consider it, for itself, as a main agent in that second form of induction, which I have also described in the paragraph. Now in regard to the value of hypothesis as applied to induction, many elements may conspire, or be wanting. The hypothesis which we propose to extend to a series of observations, may be in itself a previously acknowledged truth, perhaps experimentally established; or it may have the advantage of a strict and pervading accordance with the observations to explain and generalise which it is adduced; or it may be recommended by strong analogies between the series of phenomena, to which we would apply it, and other series of phenomena, to which it confessedly *does* apply. Again, the theory or hypothesis may be gratuitous; *i. e.* it may have no such extrinsic proof as in the first and third cases just noticed; and its accordance with the observed laws of phenomena may be partial and limited. With respect, however, to this last head, which constitutes, as will afterwards be pointed out, a very important one in medical reasoning,* we may remember with advantage the just remark of Hartley, that "any hypothesis which possesses a sufficient degree of plausibility to account for a number of facts, helps us to digest these facts in proper order, to bring new facts to light, and to make experimenta crucis for the benefit of future inquirers." "It must at the same time be granted," says Dugald Stewart, "that the probability of a hypothesis increases in proportion to the number of phenomena for which it accounts, *and to the simplicity of the theory by which it explains them.*" I quote this passage from Dugald Stewart's Elements, for the importance of the truth expressed in italics.

If I were here discussing not certain specialities of proof, but the general subject of it, I should have more to say on the subject of hypothesis; and far more than I propose to say on the subject of one element in the construction of hypothesis, which Dr. Whewell and Mr. S. Mills have carefully analysed,—I mean the argument by analogy. As this

* The strong necessity under which the physician is often placed to use this kind of reasoning, and the tendency thus generated to abuse it, may furnish, I suspect, the clue to those suspicions of the philosophical value of our pursuits, noticed in my introductory remarks.

is used by medical reasoners, I will venture one caution. Analogy is only useful towards the construction of an hypothesis when there is truly some essential point in common between the subject matter from which it is drawn, and that to which it is applied. A *fanciful* assumption of common points may produce a brilliant or witty illustration, but nothing will be proved, or even rendered more probable. Thus, when Bacon says that "virtue is like precious odours, most fragrant when they are incensed or crushed," the mind, pleased by the illustration, simply becomes more retentive of the idea illustrated. When Mr. Herbert Mayo applies Dr. Yelloly's fact of paralysed sensation occasioning under certain conditions an apparent but unreal loss of motor power, to the truly analogous case of the section of a branch of the fifth pair of nerves disabling to appearance the prehensile powers of the animal operated on, he converts what was before an assumption into a probability of the highest order. I do not mean to deny the rhetorical beauty and value of such analogies as I have quoted from Bacon, but merely to point out the *fallacy of illustration which is produced when similes are taken for proofs*. Of this dangerous, and I may add, seductive kind, is all that reasoning from the moral to the material world, which often intrudes itself on the fancy of the unwary theorist in medicine.

Cardiac disease offers, in the present day, two instances of hypothetical induction in course of fulfilment. I allude to the hypotheses explanatory of bruit in endocarditis, and of rubbing sound in pericarditis. The latter hypothesis serves, indeed, admirably both to identify and to explain the disorder. It accords with the other phenomena of the disease, in so much that the fact itself, the rubbing sound, seems unsusceptible of any other explanation than that which the hypothesis assigns to it. The endocarditic bruit of Dr. Latham is also explanatory as to the manner in which the fact is brought about. This criterion, however, does not possess the same amount of probability, from accordance with phenomena, as that alluded to in pericarditis; for the endocarditic bruit must be susceptible of another explanation, inasmuch as it is present in certain anæmious cases without any ground for suspicion of valvular lesion or deposit. Still, this discovery of Dr. Latham, in combination with other symptoms of cardiac disease, is most important.

The ingenious argument of Dr. Budd, in favour of the causation of hepatic abscess through pus absorbed from ulcers in the large intestines, presents another instance of an induction explanatory as well as collative of facts; *i. e.* hypothetical. The weight to be attached to it must depend upon the extent to which it may be considered to fulfil the conditions above laid down, of analogical probability of that hypothesis or number of instances to which it is accommodated. Dr. Annesley's supposition, that in some cases the abscess is consequent to the dysentery, in other cases the dysentery is the consequence of disease of liver; while in a third order of cases, the disease of the liver and that of the large intestines are coeval or nearly so; is the best exponent of the present state of the question.

Perhaps the best illustration that can be adduced of hypothesis fortified by experiment being rendered subservient to induction by observation, is one which I have already adverted to in speaking of Prochaska. The reflection of sensorial into motor impressions was inculcated by him. Some years after, the double roots of the nerves appropriated to these impressions were experimentally made out by Sir Charles Bell and his coadjutors. I need not add, that the subject has been since followed out, through a series of valuable observations, by Dr. Marshall Hall.

It must be admitted that in both the heads of induction above illustrated, much has been left by eminent authors in a very imperfect and unapplied state. It cannot as yet be said that the numerous series of collated, and arranged, and ably discussed facts for which we are indebted to M. Louis, to M. Andral, to Bouillaud, to Broussais, and to our own

Annesley and Abercromby, have either by these distinguished men, or by others, been rendered subservient to all the purposes of classification or explanation to which they ought to prove available.

A large proportion, indeed, of our most valuable and, I may add, of our most worthless medical literature, consists of *inchoate* inductions through observation having various amounts of support from collected instances or from hypothesis. And of these it must be observed, that all which have a good basis in facts, or are supported by plausible hypothesis, have a certain value, provided the author is himself aware how little as well as how much they are worth, and shews that they sit lightly upon him. Of the merits and demerits, indeed, of the gratuitous hypothesis, to which these are allied, I shall speak presently. The logical circumspection which appreciates the just value of this kind of writing is nowhere more observable than in the Notes and Reflections of Dr. Holland, in which he throws out suggestively abundant theories, without ever failing to let us see that he justly appreciates the amount of proof which he has to bestow upon them.

But there is a kind of suggestive essay writing not uncommon in our profession, which bears some resemblance to the last-named composition, just sufficient to produce an occasional identification of the two kinds. In the works which I have alluded to, there is little or vague reference to general principles, but much exhibition of a kind of tact which undoubtedly forms an important part of the medical character, but which cannot be conveyed in this way through the press. It is desirable that the writers in question should exchange their present plan for that of recording distinct cases. Their remarks thus realized, as it were, and perhaps corrected, would then have a specific value, instead of the vague character—half philosophical, half gossiping—which they at present wear.

The good which gratuitous hypothesis is capable of effecting in medical research, may well be contrasted with its contingent evils. Being presumed to have no extrinsic proof, and depending for its admission only on a partial and limited accordance with the phenomena of which it pretends to explain the relation to each other; it is liable to give influence and credit to an induction having a fiction for its basis. In this noxious character it may be traced from the earliest history of medicine even to the present time. The ancient physiological conjecture that a certain πνευμα is transmitted from the lungs to the heart, thence by the aorta into the system, and eventually returned to the lungs by the pulmonary veins, is not more objectionable for its deficiency in objective truth, than Maximilian Stolt's hypothesis explanatory of that form of rheumatism which it pleases him to call bilious, in persons: "quibus acris et biliosa materies maximam partem ex ventriculo resorpta et ad corporis superficiem delata in vasculorum exhalantium orificiis hæsit ibique vellicando dolores rheumaticos concitat." A free use of illicit hypothesis has indeed up to a late period vititated our views. "Pathologists of a recent date," says Dr. Watson, in his History of Dropsy, "speak of a want of tone or energy in the absorbing vessels, of the superfluous fluid not being taken up adequately by the enfeebled absorbents, meaning thereby the absorbents properly so called." Now the philosophical error of these pathologists consisted in their resting satisfied with a hypothesis of absorption, not otherwise proved to involve a truth, than as it suited observations, as far as they had been carried. Wanting the support of analogy of direct proof, or of consistency with an extended series of observations, their hypothesis was a mere form of words, from which, however, they reasoned with as much confidence as if it rested on the phenomena of heterogeneous attraction.

With respect, on the other hand, to the more advantageous results of gratuitous hypothesis, it is to be borne in mind, that relating as they do to a subject

"Quod latet arcanâ non enarrabile fibrâ,"

our inductions must often lose the benefit of such lights as theory throws upon investigation, unless we are contented at first, and sometimes at last, with an hypothesis which possesses a sufficient degree of plausibility to account for a number of facts, though not so based upon analogy or experience as to be other than gratuitous. One form I must notice, which certainly demands more consideration than it has hitherto obtained. It may be called the extemporaneous hypothesis, applicable, as it is, when a case falls under no recognised law, and the mind craves, in the absence of such law, some intelligible ground of immediate practice. It is the faculty of thus extemporising, which perhaps more than any other distinguishes an able physician, provided it be combined with a just appreciation of the value of such hypothesis, and a readiness to abandon it in the presence of contravening facts. A capacity and readiness in executing this process is indeed sometimes a source of reproach to us, as practising a merely conjectural art, by those who are unable to distinguish the results of luck from those of sagacity: and sometimes physicians, with a false modesty, humour the imputation. Although in its immediate application conjectural, the power which I speak of demands an original talent, and is never successfully carried into practice except by men of large acquired knowledge.

"It was a frequent and favourite remark of Dr. Cullen," we are told by Dugald Stewart, "that there are more false facts current in the world than false theories." If this remark is correct, it tends to justify a ready use of theory. That it probably *is* correct will perhaps be admitted, when we consider that facts captivate the attention specially by strangeness and novelty, theories by suitableness and fitness; and that truthfulness lies nearer to the latter than to the former qualities.

Independently of the value which gratuitous hypothesis may occasionally claim as a source of proof, it is in some cases useful, we might almost say essential, as a means of description. No definite idea could indeed be conveyed of the "cogitata et visa" of microscopical physiologists, either to themselves or others, unless they had gratuitously assumed a dynamical theory with respect to them. And it is to be observed that a plausible hypothesis thus originated may ultimately turn out the true one, as harmonising with *all* the phenomena; though it may rarely be susceptible of so direct a clinical application as that of hepatic venous, and portal congestion, brought out by the researches of Mr. Kiernan. Irrespectively of such good fortune, the tendency of theories such as we are considering, to afford light and to open out prospects, is of immense value. They are "luciferous," if not immediately "fructiferous."

The sense in which I have discussed inductive proof through experiment and observation, has been in substance that which Lord Bacon has laid down as characterising "true induction." I am unable entirely to subscribe to the depreciatory distinction which he makes between this and another process, having, as it appears to me, many claims to attention. On this process he makes the following remarks. "Inductio, quæ procedit per enumerationem simplicem, res puerilis est et precariò concludit, et periculo exponitur ab instantiâ contradictoriâ, et plerumque secundum pauciora quam par est, et ex his tantummodò quæ præsto sunt, pronunciat." Now, I will venture to suggest, that if Lord Bacon's far-darting eye had reached the present age he would have seen this "enumeratio simplex" applicable to the most important uses. He wisely, indeed, suggests the defect to which it is most obnoxious—that, namely, of insufficient number of instances; but he would have recognised the fact, that where the enumeration is sufficiently extensive, the danger from a "contradictory instance" is averted or ceases. The existence, indeed, of contradictory instances is so far from endangering the argument, that it is implied in its construction, as must be observed in the calculations of our insurance offices. Now, from this source many valuable conclusions are obtained in the science of medicine; the term "puerile," used by Bacon, being rather applicable to the occasional purpose of the reasoning than to the reasoning itself. Thus, when the subject-matter of the enumeration is such as renders definition impossible, the conclusion arrived at can only deceive. Such are many of the medical enumerations of the Registrar-General, when an average is supposed to have been struck on cases of given disease—*e. g.* enteritis, pleuritis, asthma; without any evidence existing that the registers of these cases were kept by persons who were agreed as to the definition of the terms, or who would make the same application of them to actual instances.

But the objections made to the above process, as a form of induction, are essentially inapplicable. It is, in truth, no induction at all, but a sorting and colligation of facts, and as such, well calculated to supply or to suggest materials for induction.

## CHAPTER II.

Inquiry into the value of single facts, why postponed, in order, to the consideration of collective facts—Single facts estimated in their relation to proof—As embodying general principles—As enabling us to act in the absence of general principles—Their relation to abstraction considered.

The course of reasoning pursued by me in the last chapter in its relation to pathology and therapeutics supposes a series of facts used collectively for the purpose of establishing or giving a probability to certain conclusions. The only exception to this view consisted in my brief reference to the application of extemporaneous hypothesis to medical reasoning. This application may be made, and often is very effectually made, through the medium of even one well-selected and well-appreciated fact. Indeed, the consideration of facts or cases, as implying proof, when used singly or with a reference to their aggregate effects, demands a place here, inasmuch as it illustrates one of the most distinctive qualities of the medical mind. Those who tread the safe path of practical medicine, however carefully they may bear principles in mind, act immediately from facts remembered, or conjured up by an effort of the imagination, which operates upon its collected stores of reading and experience: meanwhile they are aware that no two clinical facts are alike. And herein they are distinguished from less safe and less enlightened inquirers. In their hands indeed the deduction from one fact or case to another involves an hypothesis as to their agreement or disagreement; while the less cultivated or less gifted person adopts *his* prototype whole and unbroken. His practice accordingly being founded on the common points, or the assumed common points, of cases, without reference to their differences, can never reach the idiosyncrasy of the patient, or at all events must reach it only by accident.

If I am asked why I have postponed these considerations to that of facts in their cumulative state, I answer, that the cumulative value of facts is much more easily understood than that which they possess individually. We can perceive how largely a series of instances contributes to proof, before we are distinctly aware how much (or how little) value each of them possesses.

Indeed, it is not always understood what a volume of proof may be contained in the limits of a single case judiciously applied. Of this it would be easy to multiply examples. The fact of femoral and crural phlebitis having been succeeded in a single case by symptoms of cerebral disorder, no cerebral lesion having been evinced on dissection, gives ample ground in any fresh case, in which such venous infarction may be detected, for the hypothesis of a similar functional origin of any cerebral disturbance that may arise in the course of the case. Again a single case of well-marked cerebral symptoms, which ending fatally shall have exhibited pericarditic inflammation without any structural disease of the brain, will powerfully assist a diagnosis referring any future cerebral disturbance to disease of heart, when the latter is manifested during life by its appropriate symptoms.

Now in each of these two cases an explanatory hypothesis is suggested; and thus empiricism becomes philosophy. But we have not always *this* advantage in reasoning from single cases on the effect of remedies; yet in the absence of it a single case may be highly suggestive of practical measures. A gentleman, aged 70, of a powerful frame and strong constitution, who had laboured for many years under attacks of gout in the ankles and hands, with permanent thickening, nodosity, and imperfect use of the affected articulations, was placed by me for a continuous system of eighteen months, on daily doses of the Vinum colchici, with mild aperients. During and in the course of that time his general health became very good, and he has been entirely free from attacks of gout. Before this plan commenced his diet had for some time been compulsorily regular: during its continuance, I allowed a nutritious diet, with a very moderate daily use of brandy. A year has elapsed since the termination of the plan; during which, however, he has used every second day a mild aperient, containing a form of colchicum, but resumed a much freer and more liberal use of alcoholic stimulants. No attack of gout or general diminution of health has supervened. I can offer no adequate conjecture of the modus operandi of colchicum in this case. Yet, single as it is, when viewed in relation to the known influence of colchicum in gout, it affords a motive for similar treatment in a similar case. The pulse of this gentleman, I may observe, was naturally firm but slow. I carefully modified the dose of colchicum, so as not to depress it below its normal standard, to which depression it was prone whenever I increased the doses. The pride of science may be lowered and its industry stimulated by considering the present condition of the treatment of gout. Inductive inquiries have demonstrated the appropriateness of certain remedies in reference to vicious or defective processes in the primary or secondary assimilation under that disease. The practice thus made out applies to gout generically, as one of a large series of disorders. The patient is benefited, though obscurely and uncertainly; but the disorder still makes its occasional or periodical visits. The remedies, in fact, which have been used, would do good to him, whether gout were present or not, on the hypothesis on which they are given. In short, as far as the disease is concerned, our philosophical inquiries have led us only to a practice equally uncertain in its effects and in their duration. Meanwhile, an empirical procedure, which has scarcely given birth to the merest gratuitous hypothesis on the subject, has put us in possession of a specific in colchicum.

These considerations tend to confirm the importance which I have in another work endeavoured to give to "a record of single cases."* Our medical literature, rich as it is, requires indeed a larger stock of monographs, not only in this empirical point of view, but also as embodying the varieties of nosological generalisations, so as to afford the modifying influences of constitution, temperament, &c., by observance of which our treatment is individualized, and the idiosyncrasies of the patient receive attention. How unimpressive, and therefore uninstructive, are the "varieties" of Sauvages, stated as they are in the abstract! and how immediately would they be vitalised if his diagrams were changed into portraits! Meanwhile we actually accumulate in our reports exceptions and not examples, as if a perfect acquaintance with the latter ought not to precede an enumeration of the former.

It may be alleged, with some shew of reason, that cases expressing all these varieties would be interminable, and might mislead us out of the more philosophical road to successful practice—that, namely, which lies through general principles. I have already suggested, that facts are, after all, the medium through which we apply, as well as construct, our general principles; but I may further assert, that principles can be applied through no other medium; and that all practice is resolvable into the application of a fact conceived in the mind or remembered, however large or limited may be the principle which the fact illustrates. Let him who doubts this remark test its accuracy by examining the operations of his own mind, as applied to a case in practice. The place assigned to it by nosology will not satisfy him; he views it by the light of his experience,—in other words, he determines its pathology and treatment either in direct reference to some other cases, or with a tacit recognition of the kind of practice which a similar case has before required; and thus, while he is applying the general prin-

* Clinical Facts and Reflections, by Thomas Mayo, M.D.

ciples of classification, he tacitly, if not overtly, assigns to the case those differences which separate it from other cases of that class. A time no doubt arrives with most medical men at which practical conclusions are obtained by them with a rapidity which defies such analysis; but their character is not therefore lost, because these stages have become too rapid for observation. And it is expedient to give the medical mind that pabulum, through well-recorded facts, which may be digested into such conclusions. With respect to these empirical stores becoming oppressive, no apprehension need be entertained on that score. At present, for want of such records, the *normal* is but partially known; and we are constantly finding ourselves in a false position as apparent discoverers of new facts,* which are only crude expressions of what have been previously observed, substantiated, and forgotten "carent quia vate."

The functions of single cases, which I have here endeavoured to illustrate, will appear yet more important when it is recollected that there are diseases recognised in nosology in respect to which our knowledge is at present so far inchoate, as only to exist in the shape of examples; in which no general expression of their character can be made, no diagram can be offered, and we must be contented to recognise the disease in its portraits,—that is to say, its cases. Thus in hysteria, there is no generalisation on the subject of it, which advances us a step; no description of it, except such as is embodied in cases, will enable us to deal with it in practice. And, I believe, it remains one of the "opprobria medicinæ," mainly because we are not sufficiently aware of that fact, and have not sufficiently enriched our records with monographs indicating its varieties. I know no work on hysteria which is so useful, because it is thus enriched by cases, as that of M. Louvet Villermay.

The fact, that many practitioners make a bad use of cases,

* This fact, unappreciated, often leads the public to wrong judgments. He to whom everything is new will have an imposing air of originality from that circumstance.

and convert their experience into a source of error, is unquestionable. A generic instead of a specific affinity is often accepted as justifying the use of the precedent; nay, there are practitioners whose measures can generally be traced to the *last* case of the disease that they have seen. It is hoped that the above remarks may tend to prevent this abuse of observation, by pointing out the real value of the *ομμα της εμπειριας*.

It will be observed that I have here been dealing with facts in reference to the aid which they individually afford in the *application* of principles. There is another more remote though not less important doctrine connected with the relation between individual facts and general principles, to which I may advert in this place. It is singular that we should be indebted to Bishop Berkeley for objecting to the philosophy of abstraction prevalent before his time, whereby the formation of an abstract idea was presumed to imply "the abstracting and cutting away of all those circumstances and differences which might determine it to any particular existence." In no pursuit is it more important that the abstractions necessary for general principles should be presented in close coexistence with realities, and therefore identified with individuals, than in the philosophy of medicine. Startled at the excesses of Stahl, of Darwyn, and of Brown, in disembodying properties and viewing them in the abstract, the present generation of medical thinkers has, till very lately, been indisposed to seek pathological truths, except where they might be most definitely localised, and, as it were, made tangible; hence the attractiveness of morbid anatomy. We are now returning (but without any tendency to neglect the latter important source of information) in a far wiser spirit than heretofore to the dynamical manifestations of disease: that is to say, we are again disposed to draw our indications of treatment from vital actions, contemplated as such, rather than from their results or their presumed causes in structure.

## CHAPTER III.

Hypothesis farther considered in reference to the dictum, "Truth more readily emerges out of error than out of confusion"—How far this dictum holds good in medical research—The avoidance of confusion through hypothesis examined in the history of fever—Different shades of hypothesis, how far safe or useful—Brown, Cullen, Sauvages, Sydenham, Rush, Louis, Armstrong, Williams.

During the winter of 1848, in the able lecture delivered by Dr. Whewell at the Royal Institution, it was maintained that false theory had proved more advantageous to science than the absence of theory: in other words, agreeably to Lord Verulam, that truth more readily emerges out of error than out of confusion. I am not disposed to contest with Dr. Whewell his general proposition, but it deserves to be very attentively considered in its bearings on medical science, so far as it may there be accepted as a basis of reasoning. I have observed in the last chapter, that there are diseases recognised in nosology in respect to which our knowledge is at present so far inchoate as only to exist in the shape of examples, implying in this inchoate state an empirical pathology and treatment. The occasional necessity of an empirical procedure derives additional evidence from the very peculiar nature of the curative operations which are constantly proceeding in some diseases irrespectively of our plans, except so far that these curative operations may be suspended or prevented by interference. The progression of some disorders to a successful issue, if left absolutely to their own course; of others, again, if the critical efforts of the system are modified or called out by art; while of other disorders the course is altogether and uniformly mischievous if left to itself, impose very varying duties upon the physician. It becomes necessary that we should make our choice in relation to the above differences, between a more empirical procedure in those disorders in which our knowledge is immature, or in which a system of comparative non-interference is desirable, and a more hypothetical procedure in disorders better known, and such again as pursue a course altogether and uniformly mischievous if left to themselves. To engage with a phlegmasia, a disorder of the latter class, without having before us some hypothesis or explanation of it, on which our treatment may rest, would appear quite unequal to the exigencies of the task which the physician undertakes in attempting to grapple with it. Here, then, hypothesis is as desirable* as empiricism either with no hypothesis, or with an extemporaneous one may be under the other circumstances adduced. But it is somewhat remarkable that precisely *that* class of disorders in which nature unassisted seems most powerful to cure, has been the very class in which theory, or hypothesis, has been most active, and, I may add, most intrusive. I allude to fever: the ordinary forms of which certainly afford instances of morbid procedure tending to a spontaneous cure. Now, if this be the case, it may appear not unreasonable that I select that class of disorders as affording appropriate subject matter for some more extended inquiry into the uses and abuses of theory. To this object I shall devote the following remarks.

I must here refer to the general account, which I have already given, of the relation of hypothesis to proof by induction in our pathological and therapeutical inquiries. To this account I may add, that in its usual form hypothesis may be said to spring out of observation, and to

* Where it can be obtained. The treatment of the Morbus Brightii is necessarily empirical (and most unsuccessful) in the absence of any hypothesis.

serve as a systematising principle, through which subsequent observations are classed. In its less legitimate form, which I have termed gratuitous, it is to be found classifying observed facts in reference to some principle presumed to pervade them : I say presumed, because its existence is taken for granted, on the ground that it offers a partial explanation of the reference of the facts to each other. Now gratuitous hypothesis has been extensively applied to the subject of fever. I have already noticed the glaring deviations in this direction of the Brunonian theory, the apparent barrenness of which, *as it now presents itself to our eyes,* by no means lessened its noxiousness. It must, indeed, be remembered, that a barren theory may be as mischievous as one which logically suggests wrong practice. Those into whom it is instilled may, in fact, not be aware of its sterility, or content with inaction. Again, the wordy and unsubstantial nature of the hypothesis of spasm as the cause of fever, rendered it only the more ready to take any form which the imagination of its employers has been disposed to give it. Thus, while the first stage of the febrile paroxysm is assumed both by Cullen and Sauvages to consist in spasm, according to Sauvages this hypothetical state involves a constrictive force, whereby the blood is propelled so as to conquer a stasis or obstruction. Cullen, on the other hand, having borrowed spasm from Sauvages, himself assigns it two functions. It is, according to him, both the source of the obstruction, and the agent in the removal of the obstruction. Now, whichever of these views we adopt, it is obvious that we must regard it as having no proved objective sense, through which practical measures may be determined. Yet will this hypothesis of spasm lend itself with dangerous readiness to many views which a sober empiricism would discard from the pathology and consequent treatment of the disease. Thus we find Cullen, Aph. 127, obliged, by the terms of his hypothesis, to admit the use of antispasmodics as a method of taking off the spasm of the extreme vessels, which appears to be the chief cause of the violent reaction. And thus the cautious and moderate Dr. Cullen might place the fever patient as mischievously under a hot regimen, as his ignorant and conceited pupil Dr. Brown.

If, in the absence of that legitimate hypothesis which assigns causes on proof being afforded of their objective reality, we must sometimes adopt the gratuitous hypothesis which assigns causes on proof being afforded of their partial suitableness, let us here do so in the discreet manner of our great teacher, Sydenham. Speaking of the terms ebullition and fermentation as of frequent use with the physicians of his day, he observes, that he has himself no objection occasionally to use this language, provided it be perfectly understood that these (hypothetical) expressions "have no other purpose in his treatise, than a more vivid illustration of his ideas." We may, indeed, permit the natural philosopher to help himself freely out of the treasures of his fancy; his aim and object is discovery; and his hypotheses are comparatively innocent even while they are illusory, for they can be tested before they are applied to the human subject. We have noticed cases in which this bolder procedure of thought is warranted in the physician; but in those diseases which tend to a spontaneous cure, he is in an analogous position to the natural philosopher only when the vis medicatrix is failing; up to that time he has to watch and pilot the patient on methods as empirical and as unpresuming as he can devise.

Between the nominalism, if I may so use this term, of the gratuitous hypothesis, and the realism of the inductive hypothesis as applied to fever, we may assign a place to a kind of hypothesis which we meet with, in which really existing conditions are assumed as its basis, but the connection between these conditions and the disease of which they are predicated is vague and illusory. Such, in some of its heads, is the hypothesis of fevers laid down by Pinel. Thus in the mucous or pituitous fever, and the gastric fever, the specific relation between states of the mucous or pituitous secretion in the one and gastric irritation in the other, to the fevers ranged under these heads, is eminently unsatisfactory. If, however, terms of this import and this relation to their subject matter are rightly appreciated, not as explaining diseases, but as directing inquirers into modes of investigation, I have no disparaging remark to make of them.

But the hypotheses of a far better sort which have been applied to fever have not always maintained that caution which befits us in dealing with a disease whose course will generally be more favourable in the absence of all hypothesis than under the guidance of any other than the most carefully selected. The safest hypothesis, in fact, which we can apply to this subject, is one which we may glean from the history of fevers transmitted by Sydenham. His observations enable him to establish the general fact of a change of their type occurring in successive periods, and in this way authorise us to expect a corresponding variation in treatment.

It is to be regretted that the admitted value of this hypothesis has not made it more influential in the inquiries of subsequent pathologists. In our own day, two hypotheses, each utterly irrespective of the principle laid down by Sydenham in respect to fever, have widely influenced the practice of this country; yet each of them far removed from the gratuitous hypothesis in their extensive reference to real facts as their bases; one of them distinguished in the highest degree by inductive precision. I allude to those of Dr. Armstrong and Dr. Louis. The debt of gratitude which we owe to Dr. Armstrong for establishing a form of fever congestive in its first stage as requiring certain depletory measures, would have been more freely paid him, and his memory would have commanded a larger share of fame, had he imitated the circumspection of the pathologist* who immediately preceded him in these views. It is true that the chemical inquiries which have been carried out since the publication of Dr. Armstrong's works, into the constitution of the blood, have given to the symptoms by which he recognised congestion in the *above* sense, a new significancy, by suggesting causes which connect them with crasis rather than quantity of blood; and it is equally true, that these practical difficulties in the application of his views would have been remedied had he left on record cases illustrating his practice under his theory of congestion. For the naked results of practice, honestly given, contain a source of information quite independent of the theory on which it may have been founded. I am, indeed, the more desirous to attract attention to the views of Dr. Armstrong, because his want of precision, and the undue extent which he at first gave to them, which no subsequent modification could undo in public opinion, have left them in abeyance. Cases are frequently occurring of well-marked typhus, in which depletion taking place at an early period has obviously tended to give a successful termination. Other similar cases occur, in which a similar measure would probably prove equally successful, if the principle on which it may be carried were more definitely laid down, or, in default of this, if instances were given of which empiricism might avail itself in consequence of their fulness. Still, in the absence of this practical character from Dr. Armstrong's speculations on congestive fever, I believe that his merits are truly and faithfully set forth by his friend and candid admirer, Dr. Boott. "It was commonly supposed," says Dr. Boott, "on the prevalent authority of Dr. Cullen, that the stage of oppression always attends fever, and that this was uniformly succeeded by one of reaction; fever, in fact, being made to

* I allude to Dr. Rush. That Sydenham of America never omits to record the epidemic periods to which his views relate.

consist in an increase of the heart's action and of animal heat,—excluding, therefore, the unmixed congestive form entirely. But Dr. Armstrong has proved that in many cases there is no congestion, and in others that there is no reaction; and he has more explicitly shewn how the state of excitement arises,—that it is sometimes direct or indirect in its origin, as well as the cause, and occasionally the effect of inflammation."*

Conformably with the French system of pathological inquiry, which has become perhaps too exclusively popular among ourselves, Dr. Louis prefers the localised to the dynamical view of fever, and has based his hypothesis of the cause of typhus on the ulceration of Peyer and Brunner's glands. In common with Dr. Armstrong, he has neglected the example of Sydenham, and seems to conceive that he embraces the whole disease when he has made good the phenomena of one epidemic period. Accordingly we find him committing the errors which belong to unripe generalisation, and assuming that the debility of typhus is the effect of the glandular ulceration, while it can in fact exist in its highest degrees in cases of which this symptom forms no part. If the views of Dr. Louis contrasted with those of Dr. Armstrong, in being less dynamical, are less comprehensive, and so far less philosophical, they far exceed both Dr. Armstrong and almost every other pathologist in inductive precision. Both, however, of these writers are, as I have observed, of the right kind in obtaining respectively their hypotheses from real grounds, and affirming as such truths, not plausibilities. Theory cannot be said to *intrude* into *their* views of fever. They each contemplate a variety of it, closely approximated in their view to the character of a phlegmasia. The one sees cerebral congestion, the other intestinal ulceration. So far as these states are concerned, antagonism, not palliation, is required; and to antagonise a

* Life of Dr. Armstrong, vol. i. p. 124.

disease without a hypothesis or theory, gratuitous or otherwise, is impossible.

If comprehensiveness of views and careful observation are requisite to the framing a sound hypothesis, judgment and discretion are often requisite in a high degree for its just application. Thus an hypothesis may be framed to meet circumstances under which its truth has to be assumed, not as having been well proved, but as having become more probable than the contrary supposition,—*some* hypothesis, either negative or affirmative, on that view of the subject being urgently wanted. In short, circumstances may have arrived in which the risk of confusion without an hypothesis has become a greater evil than the risk of error with one. That in a particular place, at a particular time, fevers spread through a population circumscribed by local limits, which they do not pass by conveyance through infected persons, is apparently most true. That there are other fevers which recognise no such circumspection, and which occur so frequently on an infected person being brought near some one else, who thus appears to receive the fever from him, as to imply transmission, is equally true. And if we let these two considerations serve as broad statements of a general probability, we use them discreetly according to the present state of our knowledge.

Dr. Armstrong's speculations led him early to place typhus in the latter of these classes: he ended by considering it non-contagious; and perhaps we cannot generalise on the subject of its spread with as much truth in any other way. We may, however, find reason to doubt the epidemic hypothesis, when we see cases of typhus which had before been locally circumscribed, spreading from bed to bed when admitted into hospitals remote from that locality. Still the usefulness of the theoretical distinction is not in the least impaired by these qualifications if rightly understood, but a great misdirection of precautions is averted. This usefulness consists in its tendency to solve the practical question—the general distinction between epidemic and contagious influence being assumed—how far and in what instances should the precautions demanded in the second assumption be extended to cases mainly of the first kind? Asiatic cholera is subject to similar reasoning in reference to the question of its epidemic or contagious diffusion. It is epidemic; sometimes indeed so circumscribed as to deserve the expression endemic: but we must close our eyes to evidence, which we accept in other diseases, of contagious transmission, or of a transmission which cannot practically be distinguished from contagious, if we entirely refuse it the latter character also. Still, for practical purposes, the hypothesis of an endemic or non-contagious origin remains the preferable one. The preventive efficacy of measures adopted against a presumed contagion would be far exceeded by their mischievousness. The squabbles about epidemic or contagious diffusion might often have been solved or avoided, had it been remembered that as far as our present knowledge on the whole subject extends, these attributes are perfectly compatible, and may coexist. The fallacy, indeed, on which such squabbles are founded is still influential, or has been so to a recent date. Speaking of the poison of typhus, as "either at all times diffused in the atmosphere of some regions, or capable of being spontaneously generated in the human frame," the late Dr. Williams has this observation: "*If it be unphilosophical to admit the agency of two causes in the explanation of the same phenomenon*, the theory of a spontaneous generation of the poison is negatived."* Now I quote this passage, not in relation to the doctrine which it conveys on the poison of typhus, but in relation to the logical principle conveyed in the terms quoted in italics. The excellent and learned writer of this passage should have

* Elements of Medicine, vol. i. page 33.

remembered with what meaning we must often be content in our imperfect science to use the word cause, if we choose to use it as he has applied it in that passage. The merit of being the vera causa in the scientific acceptation of the term, in which it is presumed to contain *all* the essential antecedents to the effect, can rarely be challenged by medical causes, except, as has been above observed, when they have been obtained through experiment; and I have there endeavoured to prove that our inductions are mainly those of observation.

## CHAPTER IV.

Erroneous estimate of causation induced by some gratuitous hypotheses—Strict idea of the word "cause" seldom realised in medical research—A cause must at any rate suggest the manner in which, or the law under which the effect takes place, beside its *sequence*—This condition overlooked in the assignment of causes by microscopical physiologists; thereby merit of important discovery erroneously assumed—Addison—Schwann contrasted with Liebig—How far the oversight above noticed mischievous, as also the assignment of "essential characteristics" by microscopists, *e. g.* in regard to cholera recently.

A WELL-CONCEIVED hypothesis suggests a cause, and the induction founded on that hypothesis establishes (or refutes) the claims of the cause to be considered as such. In the last chapter I made an admission of very obvious truth, that the causes which we assign hypothetically, or which we afterwards pretend to establish inductively, in pathology and therapeutics, do seldom fulfil the strictly philosophical idea of the word cause; that they rarely pretend to contain the whole antecedents to the effect. Still, in order that the name may be assigned in a sense distinguishing it from a mere condition, the cause ought to offer some explanation of the effect. It is, indeed, most true, that of the essence of causation we know nothing beyond an unvaried sequence or precession in phenomena; yet in assenting in this respect to the limitation of Dr. Brown,* I may allege that the recognition of the sequence and precession of phenomena, in certain cases, involves a discovery of the manner in which and the laws under which the effect takes place; and that the idea of cause is limited to sequences of this explanatory kind. Such certainly is the sense in which we may be said

* On Cause and Effect.

to comprehend the relation of an effect to its cause, where this exists most closely, *i. e.* when the vera causa has been made out; and such, I believe, is the sense in which every language possessess a term corresponding to cause, and distinguishing a causal condition from all other conditions, whether the idea be that under which *all* the antecedents are comprehended (vera causa), or that more limited one in which I have observed we are often contented to apply the term pathologically and therapeutically. But if in subject matter directly of these latter kinds, in which our causes must sometimes be assumed from a very limited knowledge of antecedents, we are bound to use this condition in an explanatory sense, this necessity becomes much more stringent, when our subject matter can be dealt with according to the laws of physical science. Thus our inquiries into physiological causes must not be subjected to the same short-comings in this respect, as are often compulsory when our subject is strictly pathological. Now I have observed in another part that a gratuitous hypothesis may be useful, or even essential, as an exponent of certain researches, which undoubtedly belong to the former kind. "No definite idea," I remark, could be conveyed by description of the 'cogitata et visa' of microscopic physiologists, either to themselves or others, unless in expressing them a theory had been assumed of uses and purposes." But while I contend for the value of gratuitous hypothesis in such cases, I must express a suspicion that these philosophers are not always sufficiently cautious as to the extent of proof which they consider it to possess. Nor am I satisfied on this point by their occasional admission of the speculative character of their researches. "It is by the special vital activity of individual cells," says Dr. Addison, "and of all the suitable particles composing their structures, that the secretions are produced."*

* Experimental Researches on Secretion, by W. Addison, F.L.S., page 22.

Surely some modifying terms are wanted here, expressive of the total absence of all the really explanatory ingredients of causation, under which this assignment of a cause to the secretions labours. Compare this passage with the important experiment No. xiii. by the same author,* through which he enables us to conjecture analogically how a formative power may be generated in certain corpuscles, by observing them in contact with liquor potassæ, and witnessing the tissue formed by the combination. In the first of these cases, the existence of a formative power in the cells is begged by the use of terms which presuppose it; in the second case, we are taught by a well-devised experiment how such a power may possibly be contributed *ab extra*: and an analogy is thus suggested which may at some time suggest the organic cause of such tissues. Such is the difference between the gratuitous hypothesis first stated and the experiment last alluded to, suggestive as the latter is of a real cause; yet both are given by the ingenious author with the same apparent confidence as to their value.

But wherein, I may be asked, consists the harm of the gratuitous hypothesis first quoted, serviceable as it may be in giving a bond of union to vital processes? Merely in this, that its author assigns it an absolute and not a conditional truth. So anxious, indeed, is he to maintain cell structure in full possession of a formative power, that in the experiment just quoted he damages, if I mistake not, its real value as an analogical illustration of the manner in which cells *may* form a tissue or membrane, by using it as a direct evidence of the truth of a most gratuitous assumption, that cells do form such membranes *proprio motu*. His expressions are, "it is evident that the plasticity of the resulting membrane results from the rupture of the cells." Herein he takes no account of the conceivable agency of the

* Actual Progress of Nutrition, page 18.

liquor potassæ, not only in making them discharge their contents, but in modifying the product.

Thus it happens that a description is confounded with a process of reasoning, or converted into it. The plastic or formative powers assigned to cells are not conditions implied in the relations in which those cells and molecules are witnessed through the microscope whether combined or in successive development. The relation of cause and effect, inferred by Dr. Addison, is unproved at present, and awaits the discovery of a real power, as it would be called according to the doctrine of efficient causes, or of a sound hypothetical explanation, as we should venture to term the deficient element.

The plastic or formative power of cells forms the basis, in Schwann's admirable work, of much reasoning seductive, as it appears to me, from the real mode of obtaining truths on the construction of tissues and the production of secretions. Beginning with an admission of his hypothetical mode of proceeding, "*the unknown* cause presumed to be capable of explaining these processes *may be called* the plastic power of the cells," his reasoning proceeds absolutely and authoritatively, as if a true cause had been eliminated. In the first place, there is a power of attraction exerted, at the commencement of cell life, in the molecules, which occasions the addition of fresh molecules to those first observed. Now let us consider what explanatory force this word attraction may possess.

"Physical attraction is said to act at sensible and insensible distances: in the former sense it is in relation to our globe, gravitation disposing all bodies to descend to the east. In the other senses it preserves the forms of bodies. modifies textures, gives spherical forms to fluids, causes adhesions of surfaces and influences their mechanical character; operating upon dissimilar particles it produces their union." But in all these cases it acts agreeably to laws. It is for the microscopist to point out under what laws *his* form of attraction acts in the cases referred to. This he has not done.

We next find a metabolic power, or a power of originating changes, attributed to cells; and vinous fermentation is adduced by Schwann as an instance of this. "A decoction of malt," he observes, "will remain for a long time unchanged, but as soon as some yeast is added to it, which consists partly of entire fungi, partly of single cells, the chemical change immediately ensues. Here the decoction of malt is the entoblastema; the cells already exhibit activity; the cytoblastema, in this instance even a boiled fluid, being perfectly passive during the change."* Now is not this a game of words? Would it not be as easy to say that the activity of the cells is itself occasioned by the cytoblastema the decoction of malt, or that the actions are reciprocal? Each, in fact, undergoes changes, though the contents of the cells undergo more frequent changes than the external cytoblastema. The movement of the cells is no proof that they originate motion, neither is the apparent quietness of the decoction of malt a ground for assertion that it is not influencing the cells. If the fact that the fluid is boiled be adverted to by Schwann as involving inactivity of the cytoblastema, why does it not involve inadequacy in its materials to form parts of active cells? How unlike, in the important particular of explaining phenomena and thus affording causes, is Schwann's cell theory of fermentation to Liebig's reference of that proof to the *quasi* contagious influence of chemical action—a law so widely instanced in the decomposition of substances held together by weak chemical forces. By this law, truly a chemical one, we are enabled to accept the primary influence of the cells as being in a state of chemical action, and the consequent decomposition of the cytoblastema.

But it is not merely in assigning causative agency to the

* Page 198 Microscopical Researches, published by Sydenham Society.

objects of microscopic perception that the philosophers of that school give themselves undue latitude; they are equally perilous in their reasoning as to essential characteristics. Witness the presumed identity of the atmospheric with the intestinal appearances, which has recently been so prolific in hypothesis! The recent disintegration of the microscopic theory of cholera, by the experiments reported by Drs. Baly and Gull, may possibly indeed render this class of inquiries more cautious and unpretending. The so presumed identity, depending for proof not upon a chemical investigation of properties, nor on a physiological ascertainment of structure, but upon outward resemblance, entirely vanished when the observations of Mr. Marshall had assigned to the small annular bodies an equal degree of resemblance to substances received as food into the intestinal canal, under artificial digestion.*

In the above remarks I have ventured to criticise important inquiries, in a field out of which pathological and therapeutical hypotheses of a valuable kind may eventually be raised. It is my consciousness, on these grounds, of the importance of the subject that has made me select it for these strictures; if I may succeed in averting difficulties of corresponding magnitude in its application to our science. Its difficulties in the way of description are not easily surmounted; they are, indeed, so great that the discoveries of the microscope are liable to be confined within the circle of experts, so as not to be tested by the opinion of the scientific public. Again, with respect to immediate practical uses, which some appear to expect from morphology, I may suggest one caution. Seeming to penetrate into the ultimate recesses of structure, it may also seem to offer to medical science the largest amount of truth that can be obtained from structure, short of decomposition. But the microscope, we must

* Report, page 27.

remember, deals with structure under circumstances in which, in the present state of our knowledge, it has no relation to semeiology. It is true that a finer perception of symptoms than we at present possess may accrue to us through attentive observation of dynamical phenomena ; but meanwhile we must beware how we strain the results of such observation so as to force them into fanciful relations to a microscopico-physiological system.

## CHAPTER V.

Defects incidental to the application of hypothesis, even when the general proposition contained in it has been substantively proved true, through a forced accommodation of it to the series of phenomena which it is proposed to explain—Chemical hypotheses thus abused.

I HAVE laid much stress on the imperfections of hypotheses, as these may afford by their gratuitousness a delusive explanation of observed phenomena. I proceed to consider a faultiness of an opposite kind, and one perhaps more seductive to philosophers. The error above noticed involves a defect in the structure of the hypothesis itself. That, which I proceed to notice, consists in the far-fetched or strained application, as an hypothesis, of a well-established truth. A forced accommodation of an inductive truth, as an hypothesis to a series of observations, is one of those errors into which a tendency to generalise is apt to seduce us; and I think I have observed instances in which the philosopher has indemnified himself by almost heedless precipitancy in this work of application, for the pains which he has really taken in establishing the substantive truth of the hypothesis itself. The following remarks, from the valuable work of Dr. Owen Rees, indicate the defective logic in this kind to which the science of chemistry is exposed in its relations to medicine. After certain observations on the oxalate of lime deposit, he proceeds,* "Farther and more correct observations are needed in the condition of patients labouring from oxalate of lime deposits, than any we yet possess. We find that chemistry is at no loss, however, to devise theories for

* Dr. Owen Rees on Urinary Diseases, page 147.

the transformation of several organic principles into oxalic acid; and whether it be derived from sugar, urea, or lithic acid, we can make our formula by adding or abstracting oxygen, as the case may require. All this, however, must be looked upon as a display of ingenuity on the part of the chemist; and we should wait till accurate and long-continued observations, conducted on the urine of patients, help to better evidence on which to form a conclusion. Unfortunately, the addition or subtraction of oxygen necessary to some of these theories has not yet been proved or even rendered probable; and no good reason has been given in most cases for transferring one proximate element more than another for the formation of diseased product. It is often the case, that more than one proximate element could answer the purpose, owing to the similarity of composition. The profusion with which the chemists are in the habit of adding or taking away oxygen, by as many atoms as it may please them, still farther lessens the difficulty that may at first appear to stand in the way of effecting an explanation."

These admirable remarks on the theory of metamorphosis in the production of oxalic acid, suggest the two sources of primary and secondary assimilation from which it may be derived. Nor, it must be confessed, is it unreasonable, if imperfect assimilation in the primary digestive organs be the source of it, that this should be made the general ground of treatment.* But if the supply of oxalic acid may be as large or much larger, through imperfect combinations with oxygen in the secondary assimilation, then a general proscription of sugar, in all cases of oxalate of lime deposits, becomes unphilosophical, prior to a diagnosis being obtained between these two sources, in relation to the presence of oxalic acid; that is to say, it becomes amenable to Dr. Owen Rees's objection against those chemists who make an arbitrary choice of one proximate element rather than another for the formation of a diseased product. Now we are in fact

* See Dr. Prout on Stomach and Urinary Organs.

made aware of a much more copious source of oxalic acid by M. Liebig's remark, that it may be produced from uric acid, whenever it is subjected to the imperfect action of oxygen; and Dr. Aldridge, of Dublin, has shewn that lithic acid, by the addition of the elements of water in varying proportions, may be theoretically converted into oxalate and carbonate of ammonia, hydrocyanic and formic acids, according to the circumstances of decomposition. Under this *embarras de richesses* supplied by the secondary as well as the primary assimilations in the formation and deposit of the morbid product, chemistry is misapplied if it lead to the assumption of the hypothesis, and that hypothesis the least adequate one, as a basis of treatment for the group of cases, without the most explicit admission that this assumption is partial and imperfect. At all events, the *empirical* question of diet has a right to consideration under these and analogous circumstances, before *a change* is made. Here, indeed, empiricism may prevent or correct the errors of science, aided, as it often is in a cultivated mind, by a prompt hypothesis suggested by the occasion. In truth, the physician is often less purely empirical than he thinks himself. Conjectures, which appear to him mainly of that nature, are not wholly so. A forgotten process of inductive thought has often left in his mind the idea which afterwards occurs irrespectively of this process; and he may neglect the conclusions of his judgment, if he at once rejects what assumes an unphilosophical aspect, as not being connected with any remembered process of proof.*

Speaking of the progress of his own science, M. Liebig

* Empirical practice has often proved infinitely less delusive than imperfect science, and has itself been justified by more complete investigation. Thus the use of alkalies in many cases of an alkaline condition of the urine had been long empirically found palliative; it is *primâ facie* condemned by chemical reasoning. It has been finally placed on a scientific basis, by the suggestion of Dr. Owen Rees that in these cases the abnormal state of the urine results from a condition of the lining membrane of the urinary apparatus, which alkaline remedies will remove.

observes: "It is universally felt that we are as far from a true animal chemistry as the anatomy of the last century was from the physiology of the present day." This sentence may be true, but it is gratifying to collect from it a prophetic anticipation of an improvement in chemistry corresponding to that of the science with which he contrasts it. With a view to the practical results of this scientific improvement, it is of incalculable importance that the use of hypotheses in chemistry should be as cautious as the experiments by which they are obtained. Instances, however, of that departure from sound logic which I am illustrating, may be found even in M. Liebig. The following example may be considered in point. By experiments proving that "animals deprived of life and subjected to artificial respiration cooled rapidly, notwithstanding the blood appeared to undergo the unusual changes in the lungs," Mr. Brodie had disproved the opinion then generally received, that animal heat is dependent in warm-blooded animals on the changes produced in the blood by respiration. M. Liebig's experiments proceeded to remove entirely the causation of animal heat from combination of inspired oxygen with carbon contained in the blood, and to place it in the metamorphosis, through combination with oxygen of living tissues formed from the blood. It is next argued by him, that in the healthy subject a quantity of carbon must be introduced as food into the system corresponding with the quantity of oxygen introduced. But the quantity of oxygen inspired in a given volume of air is affected, he observes, by the temperature and corresponding density of the atmosphere. Now in applying these important views as an hypothesis to pathological observations, he instantly quits his inductive circumspection. *Assuming* the capacity of the chest to be a "constant quantity,"* he argues that "at every inspiration an amount of air enters,

* Liebig's Organic Chemistry, p. 16, 17.

the volume of which may be considered as uniform." But its density, and consequently the quantity of oxygen which a given volume contains, is not uniform. "Air is expanded by heat and contracted by cold, and therefore equal volumes of hot and cold air contain unequal weights of oxygen. In summer, moreover, atmospheric air contains aqueous vapour, while in winter it is dry; the space occupied by vapour in the warm air is filled up by air itself in the winter. On this account, also, "atmospheric air contains for the same volume more oxygen in winter than in summer." Meanwhile, "in summer and winter at the equator of the poles we respire an equal volume of air."

From these data, "the oxygen taken into the system by inspiration being given out in the same form in summer and winter," M. Liebig infers that we expire more carbon in cold weather than we do in warm weather, and "we must consume more carbon in our food in the same proportion in Sweden than in Sicily, and in our own more temperate climate a full eighth more in winter than in summer."

Now, as a comment upon this dietetic generalisation, it may be observed, that the volume of inspired air is not *necessarily* uniform, and that the capacity of the chest in the corresponding sense is not a constant quantity. This requires further proof. What is the phenomenon of gasping for breath, when a high elevation has been obtained, but an endeavour to take in a larger volume of air, probably in order to compensate the diminished amount of oxygen in a given quantity of it? I have a right to suppose, previous to disproof, that this is only an exaggerated degree of a process of accommodation which may be constantly taking place on a smaller scale. Whence comes that choice of farinaceous food which the exigencies of his nature seem to dictate to the Hindoo, if his warm climate make a smaller quantity of carbon necessary to him in his food than to the inhabitant of a colder climate, and if equal volumes of air are always inspired by each, agreeably to M. Liebig?

I am drawn into these remarks incidentally, and with no disposition to criticise great philosophers further than is requisite for the illustration of an important logical error. It must, however, be admitted that the immense physiological consequences arrived at through the last precipitate deduction, give a practical interest to my remarks. M. Liebig himself deals somewhat severely with physicians in *their* specific character as pathologists. "All the new facts daily ascertained by the chemists, are," he says, "regarded by the pathologists as exactly those for which they have no use; because they have no clear idea of what they require; because they are unable to connect with these chemical discoveries any questions to be solved, or to draw from them any conclusions."* *If* they are unable to draw conclusions from these discoveries, the above remark at least admits the supposition that they are conscious of their inability. Let M. Liebig remember that it is far better to ponder over great inductive truths, even for many years, and deliberately to reduce them to practice, than to spoil their application, and to prejudice the public, by making them subservient to rash generalisations.

* Researches on the Chemistry of Food, page 3.

## CHAPTER VI.

Subject matter of medical proof; a review of it cannot be exhaustive or systematic—*Physical* subject matter—Semeiology—Obtained from phenomena of dead or living structure—Medical inquiry most valuably directed to dynamical laws of a vital kind—*Psychological* subject matter; question, whether accessible to experiment or observation?—Dr. Wigan's duality of mind illustrates a misconception on this point—Farther observations on sound mind aided by physiology wanted, before the laws of the unsound mind can be developed—Difficulties arising from selfishness and sympathy in estimating mental unsoundness, quà insanity, considered; case of Dyce Sombre — Idiocy — *Therapeutical* subject matter, discoverable through experiment and observation, applied through observations—Contemplates vital forces, or structural composition—Primary importance of the first element; namely, vital forces—Remedies, appropriate to these, classed as partial and specific; why are the latter so few in number?—Hints for an inquiry into therapeutical value of continental spas.

At one time I designed to fill up these outlines. The experience which I gradually obtained of the nature of such a task, convinced me of the hopelessness of the undertaking. Thoroughly to unfold the forms of thought bearing upon proof in reference to my subject, is an impossibility. Every modification of proof, carefully looked into, resolves itself into infinite subdivisions, from inquiring into which I should not be exempted by their minuteness. For this is unreal. That which is small at one period of the inquiry is great at another; it enlarges under the microscope of thought and on the discovery of new facts. Hygienic and medical principles, which are now scarcely nascent, will eventually be of the same dimensions with those on which we at present act, and the laws of experiment and observation will be modified in relation to then new subject matter. The profitable way of conducting an inquiry thus continuous and infinite would be by successive instalments. Mr. Green has opened the whole subject. I have taken it up at a point of interest suggested by his previous views; and I shall leave it eventually under a full conviction of its inexhaustible character.

Accordingly, in the considerations on which I am now entering, I shall not pretend to be either exhaustive or systematic. If the ever-shifting horizon with which our inquiries are met forbids the first of these endeavours, the second is scarcely less warranted by the progress hitherto effected. The materials of medical proof are as wide as the whole circle of our knowledge, physical and physiological, and yet have hitherto been made the subject of little more than the most suggestive discussions in their relation to that proof.

I have hitherto contemplated medical proof more peculiarly in its logical character, adducing its materials as illustrations. I proceed to consider the distinctive character of the subject matter from which it may be most obviously drawn, or to which it is most naturally applied.

In respect to the data of our clinical reasoning, we naturally look to semeiology for their earliest and most ready supply. From this source of proof the mind travels onward to presumed structural changes, and endeavours to read them by experiment and observation in the living organism, or by observation in the dead body. But reasonings based on the phenomena of the living structure have an advantage over such as depend on cadaveric inquiry, in the nature of their results. To explain the living from the dead structure exposes us to the fallacy of extracting conclusions from premises which do not contain them. If such inductions as Dr. Beaumont's sagacity and perseverance effected, operating on the peculiar case of St. Martin, were often in our power, we should soon arrive at high degrees of certainty, and escape the imputation to which we are open in our autopsies, of endeavouring to reason out the principles on which the battle of life has been fought, by examining the field of battle, and the bodies of the slain. The information obtained from morbid anatomy tends also, it must be confessed, to localise all our conceptions of disease, and so far tempts us to beg the very important question, whether change of structure is the proximate cause of disease, or whether dynamical disease precedes and originates change of structure. In these remarks I am influenced by no tendency to disparage one of our most important sources of medical proof; for such, morbid anatomy must ever be considered.

But the researches of the present century are beginning to assist us in the application of dynamical laws, through the phenomena of the living structure, either primarily or in relation to its immediate products. Here we are indebted to the labours of chemistry, and perhaps in this direction more than any other we may expect medical science to be progressive. Such observations as M. Franz Simon, and others of the same school, afford us, concerning an altered state of the blood in various diseases, and under the use of given remedies, *e. g.* the diminution of the quantity of fibrin in a case of phthisis, when cod's liver oil had been largely used,* forming an exception to a law of the disease also ascertained by the same inquirers; such observations, I say, open to us large vistas of thought. In truth, the mere advance from speculations on the place and quantity of circulating blood to its crasis is a very important step in the right direction. John Hunter felt the want of inquiries pushed into that region, but does not seem to have clearly seen the medium through which they should be made. This at least I must presume, from his mode of adverting to the subject. "The mode of examining the blood, when out of the body, enables us," he says, "to observe whatever relates to its spontaneous changes and separation, together with the apparent properties of each component part. Its chemical properties become known likewise by this mode; though without throwing light on the nature of the fluid itself." The great intellect of John Hunter had, no doubt, conceived profound thoughts respecting the *nature* of the

* If we accept Poisseuille's experimental proof that hypinosis implies deficient permeation of capillaries, vascular congestion and hemorrhage becomes a conceivable consequence of the use of cod's-liver oil.

blood. After all, however, we must be at present contented with ascertaining its physical laws: and it is improbable that more truth will be realised respecting it by any line of research than is made visible in the distance through the operations of chemistry.

On the inquiry into microscopic objects, viewed in its relation to pathology and therapeutics, I have, in the present day, to suggest cautions rather than to apply stimulants; and this I have endeavoured to do, with whatever success, in the 4th chapter, when examining the logical idea of causation in reference to certain errors of microscopists.

The subject matter of medical reasoning, which I have hitherto contemplated, and from which all my illustrations have been drawn in the preceding pages, has been entirely physical, or viewed in physical relations. But though the mind has a physical organ, we have a philosophical right to contemplate it in a certain primary sense, and as possessing properties on which we may reason as such; just as we reason on extension and solidity as properties of cerebral substance. Though we should accept Dr. Faraday's revival of Boscovich's theory, and resolve the material world into centres of power, we shall still be met by this distinction, and compelled to admit its expediency. For at all events the domains of thought and sensation are so placed with respect to each other, that each will be practically best estimated when their distinctness is kept in mind. Thus a disease may have its moral or physical phase, according to the position from which we survey it.

The question first to be considered, in reference to our present inquiry, is, whether the proof applicable to the phenomena of mind, and its laws in health and disease, be that of experiment or observation, or whether, in common with matter, it participates in both these kinds of proof. And that question may, I believe, be decided in favour of the latter; that is, of observation. In a passage which I readily quote, as bearing upon the general distinction between observation and experiment, a distinguished writer in the Edinburgh Review remarks: "By experiment we generally acquire a pretty correct knowledge of the causes of the phenomena which we produce, as we ourselves arrange and distribute the circumstances on which they depend; while in matters of mere observation the assignment of causes must always be in a great degree conjectural, inasmuch as we have no means of separating the preceding phenomena, or deciding, otherwise than by analogy, to which of them the succeeding event is to be attributed."

"Now it appears to us," the reviewer proceeds, "to be pretty evident, that the phenomena of the human mind are almost all of the latter description."*

This distinction is far from implying, that the properties of the human mind are placed out of our reach, so that we have no power of controlling, of mitigating, or of antagonising one property by exciting the action of another. I am, however, induced to dwell upon the distinction, by the tendency of philosophers to overlook it, and to reason upon these properties as if they were the subject of experiment; as if we could practise upon them the "separatio naturæ," which discovers causes, instead of contenting ourselves with the process of observation, which must often content itself with developing laws. Much of the indefiniteness of the science of mind has arisen from this appetency of a knowledge beyond our present means of attainment.. If any one doubt this, I advise him to peruse Dr. Wigan's able work, in which he professes to prove "the duality of the mind." The knowledge that we have two brains has long been in the possession of physiologists, and the subject has been discussed by them. That Gall knew one brain might be insane, the other healthy, has been pointed out by Dr. Elliotson. Tiedemann relates the case of a man who had one side of the brain deranged, and who observed the derangement with the healthy side. Bichat has some curious

* Edinburgh Review, vol. iii. p. 275.

remarks on this subject: "If one of the hemispheres," he observes, "is better organised than the other, more developed in all its points, consequently capable of being more strongly affected, then I maintain that perception will be confused. Therefore, if we could squint with this organ as we can with the eyes,—that is, receive impressions with but one hemisphere,—we should then be masters of our intellectual exertions." Now these physiologists have made good by observation the capacity of the mind, whether double or single, to energise with one brain, under defect or destruction of the other; and they conjecture its liability to suffer disturbance in its operations from a want of harmonious action of the two brains; but they do not assert the ulterior fact, that we have *two minds*, because they are logically aware that the nature of their proof does not reach this proposition, which demands a "separatio naturæ,"—a power of witnessing the two minds in a state of distinctness, neither at present cognizable to us, nor, indeed, conceivable. No such proof is adduced by Dr. Wigan; and all the proof that he *does* advance is explainable on the assumption of a single mind, viewed in relation either to antagonism of faculties, or to want of harmony in the two brains, or to disease of one of them.

Our acquaintance with the normal phenomena of mind must precede our acquaintance with the abnormal. This, when stated, is sufficiently obvious; yet the truth is practically less felt in mental than in physical disease. We are indebted to Reid and Stewart more than to any other philosophers not also physiologists, for that modest course of observation which may lead to the establishment of laws in the science of mind. So far, indeed, as the inquiry can proceed advantageously without the assistance derivable from a consideration of structure and its functions, they have done well.

No eminent philosopher, with the exception perhaps of M. Comte, has given the deserved credit to the views of the phrenologists in this direction. Every error of diagnosis that they have committed, has been brought to bear upon them by their adversaries, as if subversive of their system: no candid inquirer has suggested on this point that their proof being drawn from quantity and form of the cerebral substance, irrespectively of quality, would be truly suspicious if it seemed to be uniformly successful. It must be confessed, however, that the phrenologists have been incautious in their management of the startling part of their system; namely, their map of the cranium. They have never, as far as I can find, recorded a distinct admission that this same map is a purely speculative hypothesis, intended only to be such an approximation to the results of their observations as to afford aid in applying them to practice. To assert categorically, for example, that the properties of hope and conscientiousness are separated from each other by a straight line, inclining or slanted from the occiput to the forehead, and without some such modification, is an absurdity. And this absurdity is repeated in kind through every page of their history of organs. As an instrument for carrying out their researches, their chart of the brain is judicious enough; viewed as a "fait accompli" in discovery it is laughable; and more than anything else has retarded their attainment of their just rank among the philosophers of mind.

When observation shall have gone further towards the development of the laws of the sound mind, we shall become more fitted to appreciate its unsound states. But there are many difficulties, created alike by the selfishness and the sympathies of our nature, which will evermore beset this subject. Thus, according to the predominancy and mode of action of either of these classes of emotions, we may be unwilling or willing to admit the proof of mental unsoundness, when the proof, if admitted, will control the liberties or the fortunes of the presumed patient without any delinquency having been committed on his part. The influence thus produced on us may be one of three kinds. We may be unwilling to realise the imputation of mental unsoundness against a person whom we have fondly loved or greatly respected. We may be unwilling to impute this state to our own blood and family, by admitting its justice in a member of that family. On the other hand, we may be tempted to accept or to adduce proofs of mental unsoundness in cases in which we thereby acquire rights over the management and enjoyment of property from nearness of kin to the suspected person.

Such is likely to be the play of sympathy and selfishness in blinding judgment, where the question of insanity takes a civil aspect. But when we are called upon to define, or to accept a definition of, insanity in reference to criminal acts, and may be enabled to screen a delinquent through that plea, our sympathies will generally prove more active than the rational selfishness which might suggest that we are remotely protecting ourselves while we are protecting the public. The chance that our own turn may come next, if we readily extend the plea of insanity to crime, is generally remote; the gratification of sympathy, immediate.

Fully to meet these evils, a much more searching inquiry into the nature of both moral and intellectual unsoundness of mind is required than has hitherto been effected, or, if effected, recognised as such by the public. The idea at present conveyed to the public mind by moral unsoundness, in its relation to crime, has a very prejudicial effect. I have endeavoured in another work* to point out and define a state of moral unsoundness, which need not and ought not to be used as exculpatory, involving, as it does, a very complete competency on the part of the patient, if not to recognise moral distinctions, at least to recognise the penal conditions of his conduct, and to be withholden from acts by fear of this penal condition being realised. I have thus endeavoured to reconcile the views of the psychologist, as to the *abnormal* nature of extreme depravity, with the practical conclusions of the jurist and the man of the world. Not less important

* Elements of Pathology of the Human Mind. 1838.

is it to the public interests, in relation both to civil and to criminal cases, that the nature and definition of insanity, *properly so called,* should receive a more complete exploration than it has hitherto obtained; and I would venture to suggest, that this want is principally felt in regard to the question of insane delusions. In the hearing given in the Court of Chancery, during last year, to the petition of Colonel Dyce Sombre, the want of a clear idea on the subject of delusion was constantly visible. Delusions were admitted in his case; but the plea was set up in favour of his mental soundness, that he remained competent to manage his personal property in spite of those delusions. In order to support or invalidate this plea, it was necessary that some tests or criteria should have been applied to the (admitted) delusions, whereby their insane character might be substantiated or disproved. In the absence of this procedure, a constant *ignoratio elenchi* pervaded the hearing of the cause.

The evolution of those principles, on which an inquiry into insanity must be conducted, no doubt forms only a part of the subject of unsound mind in its relations to coercion or penal infliction; to the protection of the patient on the one hand, and the protection of society on the other. *Deficiency* of intellect may render the patient incompetent to manage his affairs, and may possibly render him dangerous to society; though the idiot is, I believe, rarely mischievous unless also insane. This subject is a large one. It is questionable whether our knowledge thereon, that is, our acquaintance with mental unsoundness as dependent on weakness, not perversion, is as yet reducible to laws. At present, every case is apparently tried and settled on its own grounds; the appeal being made to the ομμα της εμπειριας, or empirical sagacity.

The use of remedies is an element of medical inquiry, in which our reasoning both by observation and experiment is of the utmost importance. It is the *champ de bataille* of empirics, who take refuge, in the vagueness which it admits, from the more stringent and exact principles of pathology. By ourselves it is often treated with an appearance of indifference, as compared with pathological inquiry, which is not justified either by the amount of knowledge attainable, or its usefulness when obtained. But the tendency to depreciate remedies, or to delight in finding out that they are convertible, and that a disorder gets well just as readily under one treatment as under another, belongs to second-rate minds. To persons who lean in this direction, I beg to suggest the perusal of the following case. "A lady," says John Hunter, "of what is called a nervous constitution, arising in some degree from an irritable stomach, often troubled with flatulence and what are called nervous headaches, with pale urine, at these times uncomfortable feelings, and often sinkings, had a tumor removed from the breast and likewise near the arm-pit. Nothing appeared uncomfortable for a few days, when very considerable disorders came on. She was attacked with a shivering or cold fit, attended with a feeling of dying, and followed by a cold sweat. It being supposed she was dying, brandy was thrown in, which soon brought on a warmth, and she was relieved. The fits came on frequently for several days, which were always relieved by brandy; and she took in one of the most violent of them about half a pint. While under these affections she took the bark as a strengthener; the musk occasionally as a sedative, in large quantities; camphorated julep frequently; and towards the last, valerian in large quantities. But whatever effect they might have in lessening the disease on the whole, they certainly were not equal to it without the brandy. Brandy removed these dying fits, and I thought they were less violent after taking the valerian. A question naturally arises, would the brandy alone, if it had been continued as a medicine, have cured her without the aid of the other medicines? The other medicines, I think, certainly would not have done it; nor do I think the brandy could have been continued in such large quantites as to have prevented their returns. If so, the two methods were happily united; the one gradually to prevent, the other to remove immediately, the fits when they came on."* Now the main object of this case, as adduced by John Hunter, was pathological; while the carefulness with which he analyses the treatment illustrates his opinion of its relative importance as a subject of observation. In contrast with the above, I may refer my reader to a very large proportion of the valuable cases in Dr. Abercrombie's works on diseases of the brain and spinal cord, and on diseases of the stomach and abdominal viscera, wherein treatment is so slightly and generally given, that a question repeatedly arises, whether the phenomena really illustrate the laws of the disease under which they are described, or the effects of remedies used, or in what proportion they illustrate either.

I am disposed to think, that our pathological inquiries might in some cases be advantageously commenced from the therapeutical side of the subject, and the order "dato morbo invenire remedium" be so far reversed. The question, what shall I do for the removal of this group of symptoms? would often receive an answer virtually assigning to the group its nosological place and name. The truths which had escaped us in speculation, often come vividly before us when the mind is thrown into a practical condition. This method of dealing with a difficulty is of course not meant for application to the neglect of the more scientific procedure, but merely in cases in which the latter has failed to convey light.

With respect to the kind of proof applicable to the discovery and use of remedial agents, in the first point of view it admits of experiment; in the second point of view, it depends upon observation alone. A confusion between the analytical processes which discover, and the observations

* Hunter on the Blood, Inflammation, &c.

founded on experience, which apply, will be full of mischief. The question whether quina in its combinations is convertible with cinchona bark, as a remedy, was at first often begged through this oversight very much to the disadvantage of the public. Morphia in its relations to opium is similarly circumstanced; in both cases, the experimental procedure of the chemist must be made subservient to the experience of the physician.

In considering the principles on which our therapeutical experiments and observations must be founded, we must return to the subject of pathology, and decide for ourselves the question, whether, as the means of obtaining a healthy state, it is our purpose to influence and remedy disordered actions, or the results of disordered actions; namely, altered states and conditions of structure. It may be presumed that we propose to amend and regulate the organism in its fixed conditions, as the ultimate results of our measures; but do we contemplate the vital actions or the stable conditions as the subject to which our measures have a definite and ascertainable relation? Can we operate on the latter except through the former? If I am guided in answering this question by the best efforts made hitherto by others in the direction of therapeutics, I must reply, certainly *not*. The general names and titles of remedies are, in fact, all dynamical, and all assume as their character the power of operating some change in the *actions* of the system.* Thus, we hope that pericarditic alteration of structure may be prevented by the depletory remedies which we employ; but their direct purpose is to lessen the flow of blood to the part. We adopt similar measures under presumed hypertrophy of the heart, not from any hypothesis of immediately altering its structure, but in expectation of

* The object of these remarks will be in a great degree attained if they stimulate attention to the works of Dr. Paris, Dr. Pereira, and Dr. Golding Bird; all of which indicate or possess the requisite conditions for the advance of therapeutics.

establishing a diminished or altered arterial action. If on the same fluid, the blood, we operate through laws that assume a disordered crasis and constitution of it, we place it under the influence of vital actions which our remedies excite or occasion, as in the use of the preparations of iron, or in the ordinary methods of improved nutrition in anæmia. Indeed, if, as I have observed, we willingly accept names and general descriptions, for the remedies which we employ, indicating vital actions as their purposes, these actions must also be our purposes in using them. The change in vital statics which may be effected by vital dynamics must, of course, engage our attention. But the legitimate object of our practice is that in which our perceptions and experience assure us that we can work changes and modifications,—namely, functions and energies. This may appear a truism; but if the principle did not require to be enunciated, and thus recalled to attention, we should not find it so frequently overlooked. Thus, under the singleness of aspect which the structural termination of a disorder often assumes, contrary remedies applied to relieve, or rather prevent it, will appear to him who contemplates structure as the direct object of his practice, not only contrary, but also contradictory. Meanwhile, though this structural termination may be single, or at least suggest no differences on which diversity of treatment can be built, the indications drawn from actions of the system, on which a more judicious practitioner bases his treatment, may be both various and varying; as is often observed in the simultaneous or alternate use of depletion and stimulants.

If, indeed, in our systems of practical medicine, the fact had not been in some degree overlooked, that the dynamical exhibitions of disease, and not their structural effects,* are the subjects of treatment, our indications would by this time have become at once more precise and more numerous. From this misconception, and not from any absence of interest in the

* See Appendix, note ii.

patient's welfare, it too often happens that a consultation closely resembles an autopsy in its topics. The symptoms which bear upon that possible event of the case have in fact been more skilfully detailed by pathologists, than those, obtainable from an acquaintance with the vital actions which, properly estimated, might lead to its cure.

It might be presumed, and it is also true in fact, that dynamical laws of treatment would be afforded by the physiology of health and disease, and by the sciences of chemistry and botany. But although this has happened, it is somewhat singular that the value of remedies should not bear any direct proportion to the degree of science required or exhibited in their discovery. In fact, those remedies, which are the great desiderata of science,—namely, specifics—have rarely found their number increased through the intervention of science. Compare, or rather contrast, with the certainty of good derivable from colchicum when temperately and timely administered, the uncertainty in remedial force of agents applied according to such scientific observations as the following, by Dr. Prout:—"During feverish derangements, in which the functions of the hepatic system are particularly involved, the lithate of ammonia is not only supposed to be derived from the imperfectly assimilated chyle, but also from the deranged secondary assimilation of the albuminous textures of the body." So far even as this remark concerns the primary assimilations, I need not say that the dynamical laws educed are imperfect and uncertain. But, how, when gout is presumed to depend on the mal-assimilation of the albuminous *textures*!* Meanwhile, the discovery of colchicum does not appear to have been led to by any scientific procedure. Its operation as a specifically curative agent is not based on any one of its sensible properties, or any influence on one vital force more than another.† We knew it first in the present day as a

* Page 110.

† The doctrine, that it operates by exchanging the urates for urea, is a rare instance of an approximation towards explaining a specific.

dangerous resource of charlatanism, and have tumbled into its present salutary use.

Among the modes of classifying dynamical treatment,—a subject which may indeed be surveyed from many sides,—we might institute a division of it into two heads. One of them might contain the small tribe of specific remedies, as illustrated in colchicum. The other, for want of a better term, I would call partial. By the first term I designate remedies which fulfil, or pretend to fulfil, the true meaning of the word specific, by applying to the whole group of actions which really constitute the disease, *i. e.* to the whole essence of the disease, not operating obviously through one of its phenomena more than another. By the latter term, partial, I mean to designate remedies which seemingly apply to those actions alone which it has in common with other disorders; remedies which are so far likely to have a less marked and definite operation in its cure. Let me illustrate the distinction by another reference to gout. The remedies or dietetic rules applied to the morbid actions of gout under such dynamical laws as that which assumes the mal-assimilation of albumen as a cause of these actions, being partial in the above sense, seem to palliate and modify the disease; while colchicum appears to cure it, or at least to have a direct curative tendency. It is probably the antidote to the essential principle of the gouty diathesis; the abstraction of which, or its neutralisation, may leave the system impassive to the usual exciting causes of gout, such as the deposit of the lithates may be considered to indicate.

That the remedies which most potently cut short or antagonise diseases should be thus distinguished, that they should be felt by the entire malady without telling upon its separate morbid actions, is as singular as, I believe, the fact is true. And this their property constitutes in part the reason why they are few. Colchicum, mercury in syphilis, sulphur in psora, cinchona in ague, and perhaps cod's liver oil in certain pulmonary affections, almost exhaust the scanty list. Associated with no obvious effects or symptoms, and not depending for their virtue on any definite perceptible action, specifics are not got at by that process of scientific induction which gratifies the aspirations of intellect, or may be commanded by patient thought. They occur accidentally —often through unscientific observers. Perhaps the scientific way of getting at the class is hitherto precluded to us, lying, as it necessarily would lie, through an intimate knowledge of the essence of disease; a point of knowledge which we at present are far from possessing. The homœopathists fancy that they have indeed solved the problem, and have consummated a principle which may guide us as to specifics by their law that "similia similibus curantur;" and in conformity with this principle they gravely announce that it is a proprium of cinchona bark to cause as well as cure ague! It would be worth while to inquire, in a spirit more humble than distinguishes that sect, how far this law really would carry us. We want specifics in the limited sense in which I have used the word; but a specific generalisation, a law of a specific character embracing large masses of disease, would be a God-send indeed. Our acceptance of this one will not be promoted by such examples as the above, neither by our being informed that squills is pointed out as a cure for pneumonia by its (assumed) tendency to cause that disease.

I have advisedly endeavoured to draw the attention of the student who may read these pages to the study of vital actions from that of structural changes, in reference to treatment, under the impression that the medical mind has leant too much of late years in the latter direction when this question is mooted. But on the other hand, let it be remembered that those morbid actions require interpretation as well as attentive observation; and that although the indications derived from them cannot be superseded, they may be explained and rendered intelligible by just views of structure. Again, that although we aim our measures at some vital action, our selection of the vital action to be operated on must be governed, if possible, by prospective considerations, carrying the mind on to resulting changes. When none of these modifying views have been estimated, the crash of opinion between dogmatists who happen to meet in consultation, having been accustomed to view the subject each from his own side of it, is highly mischievous.

Under the head of treatment, and truly under that of dynamical treatment, I would invite attention to one large subject, which has not met with adequate consideration from the English medical public, as to the kind of proof on which it rests, and of which it is susceptible. Our communication with Europe and through Europe has been such during the last forty years, that remedies local to other countries have been a part of the subject with which our therapeutics are justly expected to be conversant. Indeed, a necessity has arisen that we should place ourselves in a more complete acquaintance with the continental spas, in their application to English habits and constitutions, than is comprehended in our present course of studies. Knowledge on these subjects is almost confined to the physicians practising at their respective spas, instead of being a part of the great system of European medicine. The instances in which benefit is undoubtedly derived from a very copious introduction of diluted neutral salt with gaseous impregnations into the system, and the cases in which this may be prejudicial; *e. g.* the effects for good or evil of the bath-sturm, with its resulting phlegmonous abscesses,—these are weighty considerations as yet if not unexplored certainly not exhausted. At present, a patient is sent precariously to a spa, by one who knows nothing about *it*, and there he falls into the hands of one who knows nothing about *him*. Of course, I put this down as a liability, not as a rule.

## CHAPTER VII.

Abnormal theories of medicine; why deserving of inquiry—Homœopathy—Hydropathy—Mesmerism—Gratuitous hypothesis of homœopathy as to origin of chronic disorders; infinitesimal doses; its therapeutical principle dangerous from its exclusiveness; *possible* grounds for adopting it as one head of a more comprehensive system, considered—Hydropathy; inquiry into it urgently needed; valuable principles in regard to its abuse enunciated by Liebig, Dr. Bence Jones—Mesmerism; phenomena of it proved true or rendered probable, as facts, may be examined in relation to medical uses—Mesmeric clairvoyance declined in reference to this test—Physiological, pathological, and therapeutical grounds adduced for inquiry into first mentioned phenomena—Moral objections to the practice of mesmerism; religious objections propounded by the Rev. Dr. Maitland, estimated.

My remarks on the subject matter of medical proof have hitherto related to inquiries which are unquestionably normal. They open, however, vistas into what we ought to consider a part of our present subject; namely, into other pursuits, which might either be brought into the sphere of practice wholly or in part, or which, having attained celebrity and importance on inadequate grounds, may afford even in their apparent errors a salutary and instructive lesson.

But do these abnormal pursuits of medicine deserve inquiry? or do the public interests demand that we should give it? I answer both these questions unhesitatingly in the affirmative; at least, in respect to those theories of the above kind which are most prevalent in the present day.

We have supposed in the previous speculations that a certain curriculum both of antecedent and professional education should be required, at different periods, of the medical student. It is not inconsistent with this demand that a class of inquirers should exist—nay, that they should make useful remarks and curious discoveries—without having pursued these curricula. This class of inquirers, together with some others who commenced their pursuits in union with us, but have since partially or wholly abandoned the normal system of medicine, together, also, with the charlatans both of normal and abnormal medicine,—these, I say, constitute that mass of practitioners and students who are somewhat unfairly classed under the common title of quacks.

Now it certainly belongs not to any comparative indolence of the normal body of the profession in the present day, but to the immense stimulus existing everywhere to bold and free inquiry, that this abnormal body is more numerous at present than it has ever before been known,—at least in this country. On this ground alone they might force themselves upon our attention, even if it should be denied, which denial I consider impossible, that their efforts often tend to useful discoveries. But if this is the case, surely our conduct in relation to them is singularly unfortunate. Our uninquiring opposition to them deprives the public of the advantage derivable to it from their doctrines passing through the ordeal of our scrutiny: and our cause is damaged, in the estimation of the public, by an apparent want of candour in refusing that scrutiny. We strangely overlook the fact, that one of our best grounds for demanding for ourselves privileges and protection from the government of the country, would be our diligence in sifting and investigating these abnormal views in medicines, which we wish, under some circumstances, to be empowered to restrain. Nor will the hacknied argument be accepted in excuse for such non-performance of a duty, that we are unwilling to give importance to empiricism by making it the subject of deliberate inquiry. There are cases in which the refusal of inquiry is more readily attributed to prejudice than to contempt.

The subjects to which these remarks are most applicable in the present day are Homœopathy, Hydropathy, and Mesmerism. I shall venture some suggestions in regard to each, in the hope that they may stimulate investigation.

Those circumstances in the homœopathic system which most have tended to discredit it, and which have naturally indisposed us to look further into it in spite of its prevalence, are, first, the singular gratuitousness of its hypothesis, both pathological and therapeutical; secondly, the strangeness of the infinitesimal doses; thirdly, the presumptuousness with which it advances exclusive pretensions. For instance, if we admit the demands of homœopathy upon our assent, we must also grant either that calomel and antimony combined with or without venesection do not cure pneumonia, or that this combination of remedies possesses the power of exciting pneumonia in a healthy person.

Yet it is perfectly consistent with the phenomena of curative treatment, that remedies should act in many cases by setting up a morbid action, which may supersede the existing disease. Nor is there anything so irrational in the supposition that this new morbid action may in some forms of disease be similar in kind to that which it supersedes,—that we should absolutely set aside the authority and testimony of Dr. Hahnemann and his followers on this subject, though we may be reasonably disposed to *limit* our acceptance of it.

But neither this therapeutical assumption, nor their pathology, which finds a cause for all chronic disease in psora, syphilis, and sycosis, have so much indisposed us to inquire into the claims of homœopathy, or so much excited a derisive feeling, as the intense dilution of their medicines: and yet it is in relation to the effect of these very infinitesimal doses that the attention of the normal body of physicians is most directly applicable to the subject. For here, if we would give our attention, we should find ourselves grappling with asserted facts, and should with certainty be rewarded by arriving at practical conclusions, either negative or affirmative. And let it be remembered, that this practical question is of immediate import to society, as these remedies are in course of extensive application, and generally to the exclusion of other remedies.

My reasoning on medical logic has hitherto assumed the existence of a code of medical ethics; and the application of hypothesis has been allowed under a tacit supposition that the subject to be discussed controversially has been treated with good faith. But, on the theory now before us, the suspicious improbability of the facts alleged should prevent us granting anything, previously to examining them. The bare question, to what extent the cure, or the mitigation, or the aggravation of symptoms, may follow the exhibition of homœopathic remedies, and whether the dilution of these remedies is true to the extent to which it is asserted, should be the present and immediate subject of such an inquiry as I recommend.

Very unfair arguments are sometimes used to satisfy us that the homœopathists may justly be treated with contempt, in respect to their facts, as well as their reasonings. We are sometimes told, that, in truth, they use many remedies in full doses, just as we do. This may somewhat impugn their sincerity or their logic, but it is to the credit of their good sense; and, I fear, if allopathists, (the nickname which they give us,) perilled the truth of their systems whenever they supported them disingenuously, their edifices would be in a very tottering state.

It is true that the unaided efforts of a College of Physicians might not avail to the obtaining full information on these points; and that this may be attainable only through a commission of inquiry in countries in which homœopathic hospitals have been established. Can any kind of scientific inquiry be conceived more deserving of assistance from the public purse than such a one? But it is even now open to all of *us*, though we may decline to accept homœopathy in its present state, to endeavour to profit by the presumed errors of that theory, or to avail ourselves of the suggestions which it may supply, sometimes in an opposite direction to our own habitudes. The therapeutical law which they propound is truly a dynamical one; and laws of this kind are wanting to *our* practice. However unduly the homœopathists have generalised their law, this is a contingent, not a necessary defect of it. The homœopathists, as I have already observed, *need not* have affirmed that all morbid states are curable on the principle of similia similibus,* or that every disease has a type of its cure in the causes that produce it: an affirmation which puts them to the trouble of adducing mere accidents of the disease, as if they were *propria*, with a view to elicit from them appropriate remedies, and again to strain and distort remedies in order to adapt them to those propria of the disease which the effects of the remedies must resemble. Thus, squills becomes in their hands a remedy for pneumonia, because it is productive of some bronchial irritation in a healthy person; an assumption on which two remarks may be made,—one, that if squills has had the latter effect, this has been accidental; the other, that squills does not *cure* pneumonia, but merely tends to relieve it in its latter stage, when truly appropriate remedies have removed the direct distinctive characters of pneumonia.

The following remarks point to a method of dividing the whole subject of therapeutics, which would, perhaps, assign to the homœopathic theory of treatment, if substantiated, its proper place. Vaccination occasions a disease having many common points with variola, which for life, or for a term of years, renders the system incapable of receiving variola. A given morbid action here supersedes a similar one. The disease, pneumonia, is successfully antagonised by venesection, with or without calomel and antimony, or by calomel and antimony without venesection. A morbid process is set up, I contend, by these remedies, totally distinct

* See Appendix, note iii.

in kind from that of pneumonia, but curative of it. Here, then, we have a dissimilar disease conquering a given one. Thirdly, there are many instances of erysipelas in which cinchona bark, with or without mineral acids, cures the disease effectually, without inflicting any morbid action whatever. Now, on these three procedures thus exemplified, two of them conquering a disease through the medium of another disease,—one conquering it directly and without any concurrent morbid action,—supposing them severally substantiated in a few cases, an hypothetical arrangement of therapeutical objects and agents may be conceived that may serve as a basis for an inductive process, confirmatory or otherwise, through *repeated* instantiæ, of the hypothesis. And this hypothesis will have the characteristics which we ascribe to a sound one: for it will not only be explanatory of the instances from which it is drawn, but also, if the instances are allowed, it becomes a general truth, by virtue of these accumulated instances.

Whatever may be the value of the distinctions here suggested, they have enabled me to illustrate remarks made as to the nature of a well-chosen hypothesis, and its connection with that subsequent inductive process which converts a hypothetical suggestion into a generalisation. In this point of view they may not be thrown away, even though the hypothesis itself should be eventually declined. With this humbleness of purpose, in this point of view, and with these precautions, the greater is the number of sides from which the subject of treatment is contemplated, the greater will be the opportunity afforded us of obtaining rationalised practise.

With respect to the subject of Hydropathy, the systematic inquiry to which it may lay claim will receive some assistance from preadmitted and substantiated general principles, which I have not assumed in the case of Homœopathy. And we are furnished with important means of distinction between cases to which hydropathy may be deemed appropriate, and those to which it is not, by principles accessible in Liebig's works. "In Austria," says Dr. Bence Jones, "a mode of treatment has been revived which, in those who can endure it, is most beneficial in the diseases which are included in the uric acid diathesis,—as indigestion, bilious complaints, gout, rheumatism, and skin diseases. At Graffenberg, in Austrian Silesia, under Preissnitz, the action of oxygen is promoted to a most beneficial extent in these diseases, but to a no less disastrous one in the opposite class of diseases which arise from too much action of oxygen on the body, as in phthisis and scorbutic cachexy. Until Professor Liebig directed attention anew to the action of oxygen on the human body, the causes of success and failure were unknown. At Graffenberg, which is among the mountains near Frieburg, the greatest possible action of the skin is produced by baths. Large quantities of water are required to be taken: by these means the action of oxygen on the body is promoted to a very high degree, and death ensues, if ever the system is no longer able to furnish matter to resist the action of oxygen." The practical cautions suggested in this passage are invaluable, and are, I suspect, in substance greatly neglected. The process of oxydation may give to the ill-selected patient great temporary relief, while it is taking out of him what he cannot afford to lose as well as what he can. The curious on this subject may visit the hills of Malvern. If calomel had produced the energy which many of the cachectic patients there exhibit, they would have regarded their own improvement with distrust.

The position of Mesmerism with respect to the public demands not jesting and abuse,—the treatment which it has met with from uncandid adversaries,—but very serious medical consideration. The reality of those phenomena of trance, &c., which have been brought to bear upon the treatment of some diseases, is undeniable, however disposed we may be to exercise a chronic scepticism with respect to certain transcendental phenomena of the mesmeric state. It appears to me that the public has a right to demand of us, whether the asserted removal of disorders on mesmeric principles has been truly effected, and whether the application of the remedy is safe, and not undesirable on general grounds.

In suggesting and aiding this inquiry, I shall beg the question, whether those phenomena of mesmerism with which it is at present concerned, are true, as facts, whatever may be their merit as remedies; and I shall venture to assume that they possess that full amount of evidence from well-sifted testimony, by which we are accustomed to be guided in the acceptance of facts; assuming also, that this evidence is proportionate to their antecedent improbability. The mesmeric trance, the anæsthesia attending it, the influence of the mesmeriser's will, and the sympathy, however established, between the agent and patient, are the phenomena to which I apply these remarks. Strange and marvellous as are these phenomena, they not only are supported by abundant evidence, but have their types, however remote, in well-known laws of the human mind. Spontaneous somnambulism has long been recognised as a fact; and with respect to its being occasioned ab extra, a significant passage which describes that *mesmeric* process is as old as Plautus, and indicates its habitual use.* In this respect both ancient and modern mesmerism are conformable with the acknowledged influences which lead to sleep, and its comparative anæsthesia. The phenomena of a powerful will, in relation to one of less power, and the mysterious influence so well termed by Lord Herbert of Cherbury, "magica sympathiæ," involve laws of undisputed generality. The latter principle, in Dr. Smith's Theory of Moral Sentiments, becomes the basis of all the ethical actions and reactions of social life. Mesmerism might thus far be described as a form of sympathy

* Qui tractim tangunt, ut dormiat.

produced under some physical laws at present in course of discovery and development.

With respect to the transcendental phenomena of mesmerism, on these I shall offer no opinion in their relation to medicine. The immense antecedent improbability of pathological and therapeutical clairvoyance, explained as it is by the lady professors, or rather obscured by their explanation, renders me unwilling to advert to it, or recommend it to the consideration of those whom I address. I am, indeed, not in possession of such evidence in its favour, as ought to be adduceable by me, before I press improbabilities of that amount on the attention of others.

Let me now proceed to a few suggestive remarks on the rationale of those changes from disease to health, of which the Zoist, and other works, contain so many instances, apparently most authentic, and certainly not open to any suspicion of mistake, under the presumed influence of the mesmeric trance, the mesmeric sympathy, or the mesmeric will. For my own part, I must admit that when once I had made myself acquainted with the general power, physical and moral, of the agent employed, I lost all tendency to regard as improbable in a high degree those imputed changes from sickness to health, from the abnormal to the normal state, which appear to have shocked the belief of persons, for many of whom I have a very high respect. In regard to these changes, they admit of two hypotheses,—a moral and a physical one (used separately or conjointly); a moral influence operating through the laws of sympathy or volition, *i. e.* willing; a physical influence operating through an imponderable substance conformably with the discoveries of Reichenbach. That there exists a physical agency in the case is an hypothesis well suited to and almost required by the results of mesmerism. But its only direct proof is that afforded by the experiments of Reichenbach, who obtained evidence of the objective reality of the colours made perceptible to persons under mesmeric influence, and, prior to those experiments, to them only. In what relation the imponderable thus gained may be placed to the influence of sympathy and the power of will, it is impossible to conjecture. But a little reflection will convince us, that an imponderable agent controlled and directed by such powers is conceivably adequate to the production of physiological changes.* The wonderful descriptions afforded by the discoveries of the microscope of ultimate structure, authorise some suggestions of the rationale of mesmeric cures in the above point of view. An imponderable has much more conceivable power of affecting, by permeation, or otherwise, the microscopic cells and molecules than a ponderable agent. It is thus adapted to relations with the organism under circumstances in which it is likely, or may at least be conceived, to come into contact with disease in its rudimental forms. And if disease, as is not improbable, commences thus in ultimate structure, we have here a subtle agency adapted, as such, to influence that structure under the very incubation of disease. In estimating the question, whether mesmerism thus hypothetically supported deserves to be put upon its trial by the medical public, on the ground of direct evidence, it is first to be observed, that all its *primâ facie* manifestations are those of comfort, ease, and a feeling of consciousness of improvement. Much reason, in truth, is afforded by *this* view of the subject, why we should accept with some favour the agency of mesmerism in medical treatment. Whoever has witnessed the serenity and repose occasioned by it, even where the state before has been one of extremest anguish, may easily conjecture that the relation and harmony of parts, the smooth play of the human machine thus produced, *and indefinitely continued at will*, must be admirably co-operative with the agency of that power which we call for convenience, vis medicatrix naturæ, and which analogy leads us to suppose in continuous exer-

* The gymnotus discharges an electrical battery by willing.

tion. But the strongest ground that I can obtain from the annals of mesmeric treatment in favour of its receiving a fair trial, is that they contain a long and most remarkable array of cures in nervous disease. We admit our own failures and difficulties in the treatment of this class of cases; and we cannot but be aware that some of our most remarkable, though very accidental, cures are psychological. On both these grounds, mesmerism lays claim to a trial. I have endeavoured in another part of this work to assert the value of single cases; I know no single case so pregnant with important inferences as that of cancer cured under mesmerism, as reported in the Zoist. Its cancerous nature had been recognised by Mr. Syme, Mr. Samuel Cooper, Dr. Ashburner, and Dr. Elliotson.*

Strongly recommending, on the above-stated grounds, an inquiry into the curative value of mesmerism, I regret to be obliged to reprint in full some remarks which have appeared in my first publication of these Outlines, but which I would have wished only to have alluded to in this reprint; in the hope that these remarks having done their intended good might reasonably be left out. But they have been met in a spirit which compels me to repeat them, lest I should be supposed to have conceded the point at issue. Mesmerisers

* I think it desirable to subjoin that portion of Dr. Elliotson's narrative in which he describes the patient such as she was when the treatment commenced. "On the 6th of March, 1843, a very respectable-looking person, of middle height and age, fair, rather slender and delicate, and with the sallow complexion of cancer, called to solicit my advice respecting a disease of her right breast. I found an intensely hard tumor in the centre of the breast, circumscribed, moveable, and apparently about five or six inches in circumference; the part was drawn in and puckered, as though a string attached behind the skin at one part had drawn the surface inwards; and upon it, on the outer side of the nipple, was a dry, rough, warty-looking substance, of a dirty brown and greenish colour. She complained of great tenderness in the tumor and the arm-pit when I applied my finger, and said that she had sharp stabbing pains through the tumor during the day, and was continually awakened by them in the night."

Cases of this kind do not recover spontaneously.

certainly have not been always sufficiently careful to furnish with a satisfactory answer a question which meets us on the threshold of mesmeric therapeutics, whether a certain amount of beneficial results being granted to mesmerism, the extent of benefit is commensurate with the contingent mischievousness of the means employed. In reference to this point, I called the attention of my reader to a case published in the October number of the Zoist, 1848. In that case, it appeared, and it still appears to me, that "weakness remaining in a lady after an attack of fever" (the attributed indisposition), is removed by the temporary substitution of a kind of possession, which might not unreasonably be considered more undesirable than the "weakness" removed by it. The story is artless and well told by the lady herself, and conveys a very favourable impression of the character of the narrator; but the extent of induced influence is liable to be abused to the worst purposes. To this I subjoined the question, whether this extent of treatment is unavoidable in some cases; and whether the operator can regulate the dose; and I added a warning, that "the removal of physical evil may be effected by processes ethically objectionable." I have been injudiciously blamed in the Zoist for my remarks on this case; I say injudiciously, because I am thereby compelled, in self-defence, and for the purpose of explanation, to subjoin some extracts from the séances of this lady and her mesmeriser;* and my reader may possibly think certain particulars, which I must give, necessary adjuncts of mesmeric treatment, whereas, I believe, they are contingent, and avoidable. Dr. Feuchtersleben, in his searching and comprehensive view of this branch of pathology, which might stimulate the sordid utilitarianism of English science, wisely remarks "on the cruelty of making experiments with mesmerism to gratify curiosity." "This," he observes, "is not employing it as a remedy."

* See Appendix, note iv.

Some writers in the Zoist display unnecessary sensitiveness about my use of the word "possession," in reference to the state of the mesmerised individual. If it awakes new associations in their minds, I can assure them that they need not be alarmed. I used the word advisedly, as expressing more fully than the words rapport, or relation, the psychological influence established by the mesmeriser over his patient: if the word suggest an association between this practice and certain modes of illicit influence, to which the same word has been formerly applied, the mesmeriser has a ready and obvious answer: the faculties of our physical and moral nature would be very needlessly circumscribed, if all were prohibited except such as never have been, or never could be, put to a vicious purpose, or carried to a vicious extent. It would be a truly philosophic occupation to trace that vein of psychology which has of late years widened into mesmerism, through its previous course, as I believe it might be traced, under different names and with various uses, from a very early period of history. Thus, mediæval witchcraft certainly requires for its explanation, considering that it was admitted as a fact by such men as Lord Hales, some conjecture, such as Miss Martineau ingeniously applies to the conjuror at Cairo; namely, that he was in some cases an unconscious mesmeriser; or, to apply this to the case of witchcraft, that in some cases *its* phenomena were neither jugglery nor absolutely false allegations; but that a relation of a mesmeric kind was occasionally established between the so-called witch and another person. On a basis of this kind laid (partly) in fact, a superstructure, either of lies or of the fictions of a heated imagination, might easily be raised.

But the Rev. Dr. Maitland has adopted an hypothesis, and supports it learnedly and ingeniously, that in the course of its march through the early ages of history, mesmerism has, in some of its parts, incurred a Divine prohibition and condemnation, contained in the writings of the Old Testament, and having relation to the Gentile world as well as to the Jewish people. Dr. Maitland grounds his supposition on the etymological force of two Greek words, by which, in different places, the Septuagint translates the Hebrew word rendered in our version of the Old Testament, "a consulter of familiar spirits." One of these Greek versions signifies "one who wills;" the other "one who ventriloquises." To these Greek expressions there is no explanatory context: their conversion by the English translator into "a consulter of familiar spirits," seems to have been quite arbitrary. It is to be observed, that if our translators had taken the original Hebrew, and not the Septuagint version, their expression would have been, "one that asks." But taking the Septuagint version, on which Dr. Maitland grounds his scruples, the utmost that can be affirmed of these scruples is, that some persons, who exercise a peculiar and undescribed power of will over others, are forbidden to exercise this power; and also, some persons, whom it is difficult to render into English, except by a term which scarcely carries with it a ground for prohibition, namely, ventriloquists.* Now this specification is too indefinite and too remote from the subject of mesmerism to authorise us in proscribing and prohibiting a power apparently of very wide diffusion, and capable of beneficial results. To the question whether mesmerism is divisible, which Dr. Maitland annexes to his own presumed proof of the forbidden nature of some of its properties, I will venture to give an affirmative answer, though on a different assumption. It is divisible in that sense in which all our mixed properties are divisible, namely, according to the lights of reason and conscience, directing its application, either in kind or degree, according to tendencies and motives. Without entertaining any suspicions of a Divine command prohibitory of its operations, I recognise a strong ground for human vigilance and circumspection in the management of it.

* The Greek terms are *θεληται* and *εγγαϛρομυθοι*. See Enquiries relating to Mesmerism, by the Rev. Dr. Maitland.

The nature of my present purpose confines me in my remarks on mesmerism to those of its phenomena which may concern the treatment of disease. I do not embrace in my estimate its transcendental phenomena; either the impartment of sensations, thoughts, and feelings, to persons en rapport, or the so-called clairvoyance,* or the phrenological excitement of organs. Now in regard to all these points, I strongly advise a further prosecution of inquiry; but this, in my opinion, for reasons which I think will occur to my reader, ought to be non-medical. Such an inquiry must be beneficial; it must either evolve new laws of a very important class of phenomena, or if it terminate in discrediting them, it will afford a wholesome warning, as to sources of error, in the admission of evidence.

The credulous and the incredulous alike require to be reminded of some preliminary considerations before they enter upon such an inquiry. The first have to guard against the love of the improbable, incidental to minds "that long to be deceived," wherever the subject matter stimulates the imagination. The incredulous may be reminded, that it is probable that some improbable phenomena should turn out to be truths, if ever it should be given to us to fathom the laws to which the phenomena of mind are subjected.

* See Appendix, note v.

# APPENDIX.

---

## Note i. (p. 5).

In medical language these expressions are used almost convertibly. An hypothesis might perhaps justly be called an inchoate theory. According to Mr. S. Mills, it is any supposition which we make, either without actual evidence, or upon evidence confessedly insufficient, in order to endeavour to deduce from it conclusions in accordance with facts which are known to be real; under the idea that if the conclusions to which the hypothesis leads are known truths, the hypothesis either must be or is likely to be true."—Page 10, Vol. II. When proved to be either true or highly probable, by having been subjected to a process of induction, it is called a theory: and thus a general proposition may be the theory established by one induction, and the hypothesis to a second one; *e. g.* the experiments of Poiseuille on the permeability of capillary tubes by fluids, tend to a theory that it bears a direct proportion to the viscidity of the fluids. These experiments and their results may be used as an hypothesis, affirming the tendency of such remedies or diet as diminish the fibrin of the blood, whereon its viscidity depends, to promote cerebral extravasation by increasing the vis a tergo impressed on the circulation.

## Note ii. (p. 57).

The distinction between these terms applies more to the manner of treating the subject than to differences in the subject. Both methods may, indeed, be applied to the same subject. That which is contemplated by one physician principally in relation to the vital actions concerned in its production may be viewed by another principally in relation to its own independent nature and composition, as if appropriate treatment would some way be suggested by this latter view being kept mainly before him. The structural termination of peritonitis may be thus regarded, either in reference to the question whether the disease has arisen from sthenic or adynamic causes, or in reference to the morphological character of the deposit thrown out in and upon the peritonæum.

## Note iii. (p. 66).

In the following case, which I have extracted from Dr. Balfour's admirable detail of cases treated under Dr. Keischman on homœopathic principles, we see good evidence, both in the symptoms and the event, that it was wrongly arranged under this head; that it required antagonism, conformably with what the homœopathists call the allopathic system. It conveys a serious warning.

"Pneumonia.—A. T., a stout-looking man, aged 46, admitted May 21st. Same day, at visit, stated that he had caught cold, and had now great pain in breathing, accompanied by a slight cough. He has not expectorated since admission. *Percussion everywhere normal ; respiration vesicular.* Pulse full, strong, and accelerated. Aconite, third dilution, four times daily. May 22d.—Expectoration rusty blood-stained; percussion anteriorly on left side normal, on right side dull as high as centre of mammary space; posteriorly on left side normal, on right side dull as high as centre of scapular space. Dulness likewise extends round over the lateral and infra-lateral spaces; elsewhere, vesicular. Pulse still full and bounding. Aconite stopped, and phosphor given, third dilution, four times daily. May 23d.—

Not examined, on account of evident dying state." The symptoms of a moribund person are then given. He died in the course of the day. No autopsy. Now this case would scarcely be a pardonable failure in any one acquainted with the use of antimony, calomel and opium, with or without bloodletting, under pneumonia. A case commencing in such a subject, with such symptoms, and treated thus early upon the principles which we uphold, would according to all rational expectation be certain of a successful issue. As it was, the symptoms ran the rapid course of unchecked inflammation in a full subject.

## Note iv. (p. 73).

The following are the phenomena of three séances undergone by Miss A., on the 14th, the 18th, and the 19th of June. The mesmeric treatment had commenced on the 7th, and was continued to the 21st, when the patient went to the sea.

"14th.—Mr. N. was later than usual in coming, and we had almost given him up, when I felt a slight mesmeric influence seeming to draw me forward, as it were, and I remarked to my aunt, that I was sure he was coming up to the house; and accordingly, in two or three minutes he made his appearance. I was speedily under the mesmeric influence; my body and senses subdued and under control, but my thoughts as usual free and clear. The mesmeric passes made at a few yards distance seemed to possess almost a greater influence than when close to me. This evening I followed my mesmeriser unerringly through the room, with closed eyes; and answered correctly to pressure over several of the organs of the head. When an organ was touched over, I felt irresistibly impelled to follow the indication, though perfectly aware of what I was doing: for instance, Mr. N. meaning to touch firmness, happened to press venĕration, and I fell on my knees, my thoughts turning to God and heaven. When firmness was really touched, I was compelled to draw myself up to my full height. When benevolence is pressed, I feel unutterably calm and happy. I cannot express any of the emotions in words unless the organ of language is excited, and then my tongue is loosened and I speak, knowing what I say, but saying it entirely from impulse. Imitation makes me follow most ludicrously Mr. N.'s words and gestures. By making passes from my knees to the feet, the latter became so chained to the ground that by no effort could I move them, or stir at all. When, in this state, Mr. N. left me, my anxiety to follow him became both painful and absurd. I could be thus chained to the ground with equal facility when I was otherwise free from mesmeric influence. All that I have mentioned is common to many patients; but from my mind remaining in its normal state, I am able to give a distinct account of my sensations, which I believe is not very usual."

"18th.—I awoke about two o'clock this morning (a very unusual thing for me) with a restless feeling, and my thoughts full of mesmerism, and a strong conviction that Mr. N. was passing within a short distauce of me. On my afterwards asking him, he said that he had passed the house at that hour, on his way to a patient, and in passing had bent his thoughts strongly upon me, willing me under his mesmeric influence. It was some time after this before I could compose myself to sleep, which when it did come was dreaming and confused. This evening, while sitting after tea chatting with my aunt and a friend, the mesmeric spell came over my eyes, limbs, and voice; and it was with difficulty I roused myself so as to escape observation. However, Mr. N. soon arrived, and owned that as he came from his own house to this, he had been mentally mesmerising me. Nothing very new occurred in my séauce of this evening. Excitement of ideality gave me the power of speech, but caused no other manifestation. The contact of my mesmeriser's hand with my throat had the same effect, giving back power to the organs of the voice. I followed Mr. N. with closed eyes, as unerringly as if I could see him, never feeling any doubt as to the path that he had taken; and I stood for at least ten minutes with my arms extended at right angles from my body, without feeling it an exertion.

"19th.—As I was sitting this forenoon under a tree, reading a book of argument with deep attention, my eyes closed, and my limbs became fixed and powerless, my mind, as usual, remaining free. I was in this state from five to ten minutes, and then gradually returned to my normal state. Mr. N., as I afterwards learnt, was at this moment bending his thoughts strongly on me. My séance was of its usual character this evening. I was in constant action for nearly an hour, and yet felt no fatigue. When Mr. N. sat or stood near my aunt, I had a wish to follow him, and yet felt a fear and reluctance to approach. When my mesmeriser is near no one I can follow him, and even his wishes, unerringly; but when it is otherwise, I become confused and distressed, and less under his influence. After his chaining me to the ground, I stood, as usual, unable to move; suddenly my feet were loosened, and I felt impelled to walk up to him. He had mentally ordered me to do so. He caused me a great feeling of distress, by making repulsive passes towards the region of destructiveness and combativeness. But the same passes directed to the front of my head had no such effect. I have lately felt a great increase of strength, which I attribute entirely to mesmerism; and the more exertion I use while under its influence, the more benefit I seem to derive from it."*

I have willingly concluded the above extracts with a passage indicating the improvement which Miss A. considered herself deriving from mesmerism; and I doubt not, however experimental as well as sanative the treatment appears, that her mesmeriser may have left *her* benefitted

* Zoist, Vol. vi. page 240 et sequent.

without any admixture of evil. But I conceive that there is much in these extracts to justify my opinion, as expressed in the text, that "weakness remaining after fever" scarcely warrants all this subjugation to an influence so intense as the above.

I had wished to have confined myself to the strictures conveyed in the text, and thus far commented on in the present note: but Dr. Elliotson, in his indiscreet regard for mesmerism, has himself chosen farther to withdraw the veil from its defects, and I am compelled to follow him. Now I prefer to do this in the words, as well on the principles, of Dr. Maitland, from whose work I subjoin the following important remarks. "My object," Dr. Maitland observes,* "is to direct the reader's attention to mesmerism as a matter of fact, and not to discuss the consequences which may arise from the use or abuse of it. Still, it is impossible to avoid taking notice of an uneasy and anxious suspicion which must have arisen in the minds of some readers. I think it will be sufficient, and doing justice to all parties, to quote the deliberate opinion of Dr. Elliotson, as recorded by him in the Zoist, for April 1845. I have invariably observed, says Dr. Elliotson, without a single exception, in all my mesmeric experience, from the time of the Okeys, in 1837, to this very day, that the mesmeric state has, even if characterised by affection, and the most intense affection too, apparently nothing sexual in it, but is of the purest kind, simple friendship; and, indeed, exactly like the love of a young child to its mother,—for it seems characterised by a feeling of safety when with the mesmeriser, and of fear of others. Those who think they have seen anything else must have seen with the eyes of a prurient impure imagination, *unless* the unjustifiable experiment of mesmerising amativeness has been made."—*Zoist*, No. ix. page 55.

* Illustrations and Inquiries relating to Mesmerism, page 40-41.

"Could we be sure," Dr. Maitland continues, "that persons who have unjustifiable designs would abstain from unjustifiable experiments, this would be perfectly satisfactory —but what an unless! Are not those who have the worst ulterior objects the least scrupulous in the selection and use of means? It will be quite obvious that the subjugation of the will may be used for other ill purposes besides those which are likely first to occur to a reflecting mind. These seem to me to be very important questions, and such as should be considered by those whose position in society renders them in any degree responsible for its well being."

In answer to Dr. Maitland, the Zoist contends, that his remarks on the "*unless*" would set aside in parity of reasoning, steam engines and lunatic asylums, *unless* the first never burst, and the second never were employed for unlawful coercion." Undoubtedly they would, if upon inquiry into the failures of these two discoveries in these respects, it was found that they overbalanced their advantages. This is the question with respect to mesmerism; on which neither Dr. Maitland nor I pronounce an affirmative opinion.

## Note v. (p. 76).

I am tempted to make one suggestion on this strange subject, in its relation to the pathology and treatment grafted upon it by some followers of mesmerism. The only way in which the mind is able to form any conception, I do not say any probable one, of clairvoyance, is by a supposition that the clairvoyant becomes cognizant, not of the contemplated object, but of the idea of the object existing in the mind of some one else, with whom he is en rapport in the mesmeric state: *e. g.* that she becomes aware not of the internal state and functions of her patient, but of the conception formed by the patient of his state and its manifestations. Now in this case, it will be observed that the pathological opinion of the clairvoyant will be only a rechauffée of the vague impressions of the patient; and the practise enjoined will be either such as has occurred to the patient's own mind, or to the clairvoyant's excited imagination, or it wiil be a mixture of the two.

THE END.

Wilson and Ogilvy, 57, Skinner Street, Snowhill, London.

Dand 8/83

5/9/79 D

26/6/

7/56