SEQUEL

TO

OUTLINES

OF

MEDICAL PROOF.

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LONDON:

PRINTED FOR

LONGMAN, BROWN, GREEN, AND LONGMANS,

PATERNOSTER ROW.

1849.

London:
Wilson and Ogilvy, 57, Skinner Street.

The following pages are reprinted, with some additions and corrections, out of the London Medical Gazette.

56, Wimpole Street,
January 1849.

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CHAPTER I.

Subject best continued in detached essays.—Reviewer.—Single facts considered 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 task of pursuing this important subject, which I wished to place in the hands of Mr. Green, as his right by pre-occupation of the ground, has been returned by him to me. Having made a commencement of that part of the subject,* which may be considered a sequel to Mr. Green's views in his "Mental Dynamics," I am unwilling that it should be altogether dropped. It is no doubt possible that there may be intellects so highly gifted, as to thread the mazes of medical proof without any rules or assistance. Nay, it is possible, that in

* Outlines of Medical Proof, by Thomas Mayo, M.D. F.R.S.

this, as in other pursuits, there may be intellects so constituted, as to work by a principle of natural dialectics more effectually than under cultivation. Such cases of each kind are, I believe, rare. It may with more reason be expected that time will be lost, abortive discussions engaged in, results erroneously assumed, experiments be confounded with observation, the abuse of theory mistaken for its use, and thus, vast piles of thought and inquiry be raised upon insecure foundations, if our attention should at no time be called to the specialties of our medical reasoning. And yet few will venture to assert, that this kind of inquiry had been carried out, when I propounded an essay on the Outlines of Medical Proof.

Now, with respect to the filling up of these Outlines, the task may be attempted by a methodical expansion of each of the heads of the subject, or by detached essays, in which medical subjects may be contemplated in relation to the rules and principles which I have there ventured to lay down. With respect to the first of these two ways, if great works did not exist in our language on the logic of induction and deduction, which render *my* task one of detecting specialties in our manner of proof, rather than of embracing the whole subject of proof,—if Sir John Herschel, Mr. Mill, and Dr. Whewell, had not laid out this whole subject, the more comprehensive and methodical procedure might be preferable. It might be requisite that the general inquiry should precede the particular one.

But with these works our task is limited in extent, though it remains of undiminished importance. For I have a right to expect that the works alluded to shall have been read in the course of the preliminary mental dynamics, so ably sketched by Mr. Green. And this consideration brings before me a remark, to which I should not otherwise have adverted, made by the writer of an article in the British and Foreign Medical Quarterly, in the heading of which my Outlines of Medical Proof appeared, as one of the works *reviewed.* On this work he observes, and it is almost his only remark respecting the "Outlines," that they are "deficient in a due appreciation of the most important part of the genuine process of induction, namely, its connecting idea." Now this involves a question, whether it is for the advantage of science, that its stores should in every work be so unfolded, that the work may be reviewed out of itself? Whether it may not be rather desirable, with a view to the progression of knowledge, that the author should accept and hand on the torch, without always walking back with his reader or reviewer, and shewing him all the passages, which he ought to have explored for himself before he joined company with the author? I presume my reader to possess such an antecedent knowledge of induction, as would comprise a due appreciation of the general importance of its connecting idea, and enable me to deal with induction only in regard to its specialties as arising out of the nature of medical research; and in this point of view, I am not aware that I have overlooked its connecting idea. What, for instance, is the distinction, which I have carefully illustrated, between induction by experiment and induction by observation, in reference to our pursuits, but a distinction between the kinds of connecting idea, which pervade these methods respectively? But, to return from this digression, I propose to throw my further remarks on the subject of medical proof into the form of detached essays.

The course of reasoning pursued by me in the Outlines, in its relation to pathology and therapeutics, supposes a series of facts used collectively for the purpose of establishing, or giving probability to, certain conclusions. The only exception to this view consists 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 no 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 through an effort of the imagination, which operates upon its collected stores of reading and experience. Meanwhile,

* Outlines of Medical Proof, page 20.

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 case or fact to the other involves an hypothesis as to the nature of their agreement or disagreement; while the less cultivated or 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 distinctions, can never reach the idiosyncrasy of the patient, or, at all events, must reach it only by accident.

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 instances. The fact of femoral and crural phlebitis having been succeeded in a given case by symptoms of cerebral disorder, no cerebral lesion being evinced on dissection, gives ample ground, in any fresh case in which such venous infraction may be detected, for the hypothesis of a 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 the heart, when the latter is manifested during life by its appropriate symptoms.

Now, in these two cases an explanatory hypothesis is suggested. We have seldom this advantage in reasoning from the effect of remedies; yet here a single case may be powerfully 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 afflicted articulation, has been placed by me for more than sixteen months on a plan of daily small doses of the Vinum Colchici, with very gentle aperients. During that time his general health has become very good, and he has remained entirely free from attacks of gout. I can offer no explanation of the *modus operandi* of colchicum in this case; and the accompanying system of gentle purgation may have largely contributed to its successful procedure. Besides, his diet has been more regular than usual during its course; yet, single as it is, when viewed in relation to the known influence of colchicum on gout, it affords a motive for similar treatment in a similar case. The pulse of this gentleman, I may observe, was naturally slow: I carefully modified the dose of colchicum, so as not to depress it below its normal standard, to which depression it was prone under any increase of 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 reasonableness of certain remedies in reference to vitious or defective processes in the primary or secondary assimilation under that disease. Meanwhile, an empirical procedure, which has scarcely given birth to the merest gratuitous hypothesis, has put us in possession of colchicum!

This use of cases is, in truth, a philosophical empiricism; and the instances which I have given strengthen the importance which I have attached on other occasions in this journal to a record of single cases. Our medical literature requires, indeed, a larger stock of single cases or monographs, not only in this empirical point of view, but 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 vitalized if his diagrams were changed into portraits! Meanwhile we 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 slight show 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 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 new case. 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 principles of classification, he tacitly, if not overtly, assigns to the case those specific differences which separate it from other cases of that class. A time, no doubt, arrives with most men, in which practical conclusions are arrived at with a rapidity which defies such analysis; but their character is not therefore lost, because its manifestations 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, as it were, 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 perhaps only crude expressions of what have been previously accredited and forgotten, *carent quia vate.*

The functions of single cases, which I have endeavoured to elucidate, will appear yet more important, when it is recollected that there are diseases recognized 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 generalization 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, 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 to 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 desirable that the abstractions necessary for general principles should be presented in close coexistence with realities, and identified with individuals, than in the philosophy of medicine. This consideration is rendered more necessary by its present position. Owing to the vagueness of dynamical speculation, as it was conducted during the last century, we have till very lately been indisposed to seek pathological truths, except where they might be most definitely localised, and, as it were, realised: hence the attractiveness of morbid anatomy. We are now returning (but without any tendency to neglect the latter source of information) in a far wiser spirit than heretofore to the dynamical manifestations of disease. If, in conducting these inquiries, we resolve to disembody properties, and so to view them in the abstract as to lose sight of them in the concrete individual, we shall find the spirit of inquiry again escape out of its legitimate circle, and wander back into that of Stahl, or Darwin, or Brown.†

* Introduction to the Principles of Human Knowledge.

† If any one wish to be made aware of the expediency of keeping the speculative principle under the influence of the practical one in our researches, he need only ponder over a few of the hypothetical expressions most employed in medical reasoning, and most necessary to our understanding each other:—stimulants, sedatives, tonics, irritants, irritation, &c. If in this meditation

he proceeds unaccompanied by images of actual fact, he is speedily lost: *e. g.* "Tonics," says an eminent medical writer, "are substances which neither immediately nor sensibly call forth actions like stimulants, nor repress them like sedatives, but give power to the nervous system to generate or secrete the nervous influence, by which the whole frame is strengthened." Now we are directly told in this passage no more, than that tonics are substances, which operate by enabling the nervous system to generate the nervous influence which gives strength.

I am the more willing to quote this example, because I can at the same time truly affirm that the author generally speculates most instructively, and with his case-book in his hand.

CHAPTER II.

Truth more readily emerges out of error than out of confusion—how far this 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 last winter, 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. The value, indeed, of that empirical procedure which I explained in my last chapter, 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 they may be suspended or prevented by our 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 and 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 in respect to boldness of pathological and therapeutical hypothesis. Now it is somewhat remarkable, that precisely *that* class of diseases in which nature, unassisted by art, 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 a morbid procedure tending to a spontaneous cure. Now, if this be the case, it may appear not unreasonable if 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 point I shall devote the following remarks.

I must refer to the general account which I have given in the "Outlines," of the relation of hypothesis to proof in our pathological and therapeutical deductions. Its foundation, I have there admitted, can rarely be laid in experiment; it must depend mainly upon observation. I may add, that, in its legitimate form, it may be said to spring out of observation, and to serve as a systematising principle, through which subsequent observations are arranged. It may thus be considered the result of an empirical induction, and the basis of a scientific induction, between which processes it thus holds a kind of middle place, derived from the one, and occasioning the growth of the other. 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 an explanation of the reference of the facts to each other. Now the gratuitous hypothesis has been extensively applied to fever; that is to say, where the nature of the disease, in its relation to the vis medicatrix, is most marked, we have unhappily considered ourselves most at liberty to stray out of the region of fact. I have noticed, in the "Outlines," the glaring deviations in this direction of the Brunonian theory. But it must be remembered that a barren theory may be nearly as mischievous as one which at once suggests wrong practice. Those into whom it is instilled may, in fact, not be aware of its sterility, or content with inaction. The wordy and unsubstantial nature of the hypothesis of spasm, as the cause of fever, is sufficiently shown in the readiness with which it takes any form which the imagination of its employers has been disposed to give it. Thus, while the first stage in 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—that is, of itself. Now, whichever of these views we adopt, it is obvious that we must regard it as having no proved objective sense, through which the therapeutics 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 treatment of the disease. Thus we find Cullen, Aph. 127, obliged, by the terms of his theory, 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 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 inductive hypothesis which assigns causes on proof being afforded of their reality, we must, for convenience, sometimes adopt the gratuitous hypothesis which assigns causes on proof being afforded of their suitableness, let us do so in the discreet manner of our great teacher, Sydenham. Speaking of the terms ebullition and fermentation as of frequent use with 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 imagination: his aim and object is discovery: while the physician, in those diseases at least which tend to a spontaneous cure, is in an analogous position to him only when the vis medicatrix is failing; up to that time he has to watch and pilot the patient on a theory as empirical and as unpresuming as he can devise.

Again, the hypotheses of the natural philosopher may be comparatively innocent, even while they are illusory, for they can be tested before they are applied to human use, while the discoveries of pathology and therapeutics can be tested only by application to man.

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 against them.

But our hypotheses, even where they deserve the epithet inductive, 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 hypotheses, 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 to us 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 much 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 thus suggested 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 obvious reference to observed facts as their bases, one of them distinguished in the highest degree by inductive precision: I allude to those of Drs. Armstrong and Louis. The debt of gratitude which we certainly 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 of those symptoms which connect them with crasis rather than

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

quantity of blood; and it is equally true that these practical difficulties in the application of his views, arising from this branch of science, would have been remedied had he left on record cases illustrating his practice under his theory of congestion. For the naked results of practice contain a source of information quite independent of the theory on which it may here 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 his views, which no subsequent mitigation 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 out were more definitely laid down. 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 consist in an increase of the heart's action, and of the 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 shown 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 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 conceived himself to embrace the entire 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 degree 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.

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

* Life of Dr. Armstrong. Vol. i. p. 124.

its just application. Thus an hypothesis may be framed to meet circumstances, under which its truth has to be assumed, not as having been proved, but as having become more probable than the contrary supposition, yet under which *some* hypothesis had become very desirable. 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. But the hypothesis selected on these grounds may involve much practical mischief if taken unreservedly. Such would be the working—such, indeed, I may say, has been the working—of the hypotheses framed to meet questions of epidemic or contagious fever. 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, and generally other forms of pyrexia, which recognise no such circumscription, 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.

Now it is perhaps theoretically right to assign to typhus, as some do, the first of these two descriptions. We perhaps cannot generalise on the subject of its spread with as much truth in any other way. Yet we may find reason to doubt our selection of this hypothesis when we see cases of typhus, which had before been endemically circumscribed, spreading from bed to bed when admitted into hospitals. 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 on the second assumption be extended to cases mainly of the first kind?

In these last remarks, I am aware that I am only unfolding and exemplifying principles which our best physicians have been for some time carrying out. But, though they have arrived at this point, it is right, in our speculations on medical reasoning, to consider through what perils men have passed while unenlightened by these sounder views, and to record them for the benefit of others. Thus we have been in danger of a removal of quarantine in reference to plague, while it was considered unphilosophical to admit the existence of contagion where an epidemic influence had been demonstrated. The fallacy on which such reasoning proceeds is indeed 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 observes that, *if it be unphilosophical to admit the agency of two causes in the explanation of the same phenomena*, the theory of a spontaneous generation of the poison is negatived.* Now I quote this passage, not in its relation to the doctrine which it conveys on the poison of typhus, but in relation to the logical principle conveyed in the terms quoted by me in italics. The excellent and learned writer of this passage should have 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 rare and singular 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 observed in the "Outlines of Medical Proof," when they have been obtained through experiment; and I have endeavoured there to prove that our inductions are mainly those of observation.

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

CHAPTER III.

Strict idea of word Cause—how far realizable in medical research—Idea of cause practically valueless, unless it assigns the manner in which, or the law under which, the effect takes place, *besides* its sequence—this condition overlooked in microscopical physiology; thereby merit of discovery erroneously assumed—Addison—Schwann contrasted with Liebig—how far such oversight mischievous.

In my last chapter I made an admission of very obvious truth, that the causes which we assign in pathology and therapeutics do not 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 mere condition or property, the cause must offer some explanation of the effect. Thus, to take an example from general physics, let us suppose an inquirer into the phenomena of dew to have arrived at the fact that bodies which radiate heat most are so far most readily bedewed on their surfaces. Now, observing that the radiation of heat is productive of relative cold to the radiating body, he will be justified in considering radiation of heat in bodies a cause of dew by chilling them, and thus producing on them a deposit of moisture from the surrounding air. Let him pursue his inquiry further, and he will find radiation of heat only a modifying circumstance in reference to the general laws of condensation of insensible vapour by cold, as the true cause of dew. But the extent to which his first conception on the subject has proved explanatory of the phenomena will have entitled him, according to the usages of language, to assign to it a causative agency. Neither does this supposition on my part imply any return to the justly exploded doctrine of efficient causes. It is, indeed, most true, that of the essence of causation we have no clear knowledge beyond the recognition of a sequence of phenomena: yet, in assenting, in these respects, to the limitation of Dr. Brown,* I may allege that the recognition of this sequence of phenomena in some cases involves a discovery of the manner in which, or the laws under which, the effect takes place; and that the idea of cause is farther limited to sequences of this latter kind. Such is certainly the sense in which we are said to comprehend the relation of an effect to its cause, in the fullest degree. And such is, I believe, the sense in which every language possesses a term corresponding to cause, and distinguishing a causal condition from all other conditions or properties; whether the idea be that under which *all* the antecedents to the effects are comprehended, or that more limited one in which, as I have observed, we are often contented to apply the term in medicine.

Now, it must be admitted, that the causes assigned on a gratuitous hypothesis will partake in the nature of that hypothesis; and that the explanation which such

* On Cause and Effect.

causes suggest will be fanciful. Their propounder, indeed, if he rightly understand their use, will view them, conformably to the expression of Sydenham, only as subservient to a more vivid illustration of his ideas.

I have observed, in the "Outlines of Medical Proof," that in this latter point of view a gratuitous hypothesis may be useful, or even essential, as an exponent of certain researches. "No definite idea," I remark,* "could be conveyed by description of the cogitata et visa of microscopical physiologists, either to themselves or others, unless in expressing them they had assumed a theory of uses and purposes." But, while I contend for the value of gratuitous hypothesis in such respects, I must express a suspicion that these philosophers are not always sufficiently cautious as to the extent of proof which they consider it to afford. Nor am I satisfied on this point by their occasional admissions of the speculative character of their researches. "It is by the special vital activity of individual cells," says Dr. Addison, "and of all the visible particles composing their structures, that the secretions are produced."† Surely some modifying terms are wanted here expressive of the total absence of all the really explanatory ingredient of causation, under which this assignment of

* Pages 19, 20.

† This passage is taken by me out of a long paragraph. My reader will readily ascertain, by referring to the original work, whether I have done it injustice in calling it a gratuitous hypothesis, in spite of the ingenious matter which accompanies it, in that and the preceding paragraph.—See Experimental Researches on Secretion, by W. Addison, F.L.S., page 22.

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 this combination. In the first of the cases adverted to, the existence of a formative power in 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 be possibly contributed *ab extra;* and an analogy is thus supplied 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; 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 thus quoted, serviceable as it is in giving a bond of union to vital processes? Merely in this—that its author assigns it a positive, and not a conditional, truth. So anxious, indeed, is he to maintain cell-structure in the possession of a formative power, that, in the experiment just quoted, summing up its results, he damages, if I mistake not, its real value as an analogical illustration of the manner in which, by a superinduced agency, cells *may* form a tissue or membrane, by using it as a direct evidence of the truth of a gratuitous hypothesis that cells *do* form

* See actual process of nutrition, page 18.

such membranes *proprio motu*. His expressions are—"It is evident the plasticity of the resulting membrane results from the rupture of the cells."* Herein he takes no account of the conceivable agency of the 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. The plastic or formative powers which are assigned to cells are not conditions involved 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 implied 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 an inductive 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 causation of secretions. Beginning with an admission of his hypothetical mode of proceeding, "the *unknown* cause presumed to be capable of explaining these processes in the cells *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 mole-

* See Experiment XIII.

cules, 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 or insensible distances : in the former sense, it is in relation to our globe, gravitation disposing all bodies to descend to the earth. In the other sense, it preserves the forms of bodies, modifies texture, gives spherical form to fluids, causes adhesion of surfaces, and influences their mechanical character; operating upon dissimilar particles, it produces their union. But in all these cases it operates agreeably to laws. It is for the microscopist to point out under what laws his 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 cytoblastema; 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

* Page 198. Microscopical Researches, published by Sydenham Society.

actions are reciprocal? Each, in fact, undergoes changes, though the contents of the cell undergo more frequent changes than the external cytoblastema. The movement of the cell 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 this fluid is boiled, be adverted to by Schwann as involving inactivity of the cytoblastema, why does it not also involve inadequacy in its materials to form parts of active cells? How unlike, in the important particular of referring phenomena to general laws, is Schwann's cell theory of fermentation, to Liebig's reference of that process to the 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 contagious decomposition of the cytoblastema.

In the above remarks, I have ventured to criticise important inquiries in a field, out of which pathological and therapeutical hypothesis of a valuable kind may eventually be raised. It is my consciousness of the importance of the subject, on these grounds, that has made me select it for these strictures. But I am far more confident in the importance of my subject than in my success in handling it. However this may be, I will suggest a few of the principal grounds for caution, against those errors in reference to presumed causation, which I have endeavoured to elucidate.

The simplest descriptions involve, in a degree, hypothetical language, and I have pointed out its peculiar demand in microscopical inquiries. But the objects of perception ascertained, and the order in which they are presented, being the truly important points at the present stage of the above inquiries, may be obscured and overlaid, as it were, by too ambitious hypothesis.

The progress of inquirers towards the ascertainment of inductive causes will be thus retarded, the discovery of such causes being assumed to have already taken place, while causes founded on gratuitous hypothesis, and explaining nothing, are adduced as inductive causes.

Finally, risk is incurred of some sterile hypothesis being drawn from these views, and engrafted on medical investigation.

The last of these considerations touches a subject deeply connected with our philosophical interests. The concurrent energy with which medical science is at present cultivated through Europe and America, places us on the threshold of great discoveries; and these may probably be expected *viâ* the prosecution of microscopy and chemistry, as giving us the completest information respecting structure and composition. I have suggested, on a former occasion, that chemistry has sometimes been over-daring in its application to medicine of causes founded on induction. From microscopy we are in some peril of receiving causes, having no foundation but words.

CHAPTER IV.

The present work must be left incomplete—why—contributions to it drawn exclusively from normal medicine—in consequence of abnormal medicine not being explored by us—grounds for extending research in that direction—Homœopathy, Mesmerism, Hydropathy, considered in reference to these grounds.

If at one time I designed to fill up the Outlines of Medical Proof, the experience which I have had of the nature of the task in putting together the preceding pages, would convince me of the hopelessness of the undertaking. To embrace all the forms of thought bearing upon proof in reference to my subject, is an impossibility. Every modification of proof, if carefully looked into, resolves itself into infinite subdivisions, from inquiring into which we are not 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 their new subject-matter. The profitable way of conducting an inquiry thus continuous and infinite, would be through successive instalments. Mr. Green has opened the whole subject. I have taken it up where it was left by him, and leave it in the full consciousness of its inexhaustible character.

But there some considerations in reference to future inquiries into medical proof which I am desirous to record. The principles which *I* have laid down, are drawn from, or have been illustrated from, normal medicine. But the deviations from them, and in some cases their application, is susceptible of illustration from other sources. Those pursuits which *we* term quackery, would afford an ample field for speculation on the use and abuse of reasoning, particularly in regard to gratuitous and empirical hypothesis. In their present state and shape, unrecognised as they are by the great body of our professors and students, I should have failed to obtain attention to any illustration or application that I might have drawn from them. The normal body of medical practitioners must have vouchsafed somewhat more inquiry into them than has yet been given, before such illustrations could be rendered intelligible.

Do these abnormal pursuits of medicine deserve such an inquiry? or do the public interests demand it? I answer this question unhesitatingly in the affirmative, at least in reference to those so-called irregular pursuits of medicine, 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 supposition, that a class of inquirers should exist, nay, that they should make useful and curious discoveries, without having pursued these curricula. This class of inquirers, together with some others, who have commenced their pursuits in union with us, but have afterwards partially or wholly abandoned the normal system of medicine, constitute that large mass of students and practitioners on whom the term "quack" is contemptuously bestowed.

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 normal body, (I use the term without any disrespectful intentions), is more numerous at present than it has ever been before known, at least in this country. On this ground alone they must force themselves upon our attention, even though 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 own cause is damaged in the estimation of the public, by our apparent want of candour in refusing that scrutiny. We strangely overlook the fact, that one of our best grounds for demanding for ourselves privileges or protection from the government of the country would be our diligence in sifting and investigating these abnormal views in medicine, 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 our deliberate inquiry. There are cases in which the refusing inquiry is more readily attributed to prejudice than to contempt.

The subjects to which these remarks are most applicable, 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 have most tended to discredit it, and which have naturally indisposed us to look farther 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 grant, either that venesection, combined with calomel and antimony, does not cure pneumonia, or that this combination of remedies possesses the power of producing pneumonia in a healthy person.

But neither this therapeutical assumption, nor their pathology, which finds a cause for all chronic disease in psora, syphilis, or 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 not involved in loose hypothesis, but 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 others.

Now we ought to view this part of the above subject as at that stage of induction through observation which precedes the formation of hypothesis. The antecedent improbability of the facts alleged should prevent our *granting* anything. 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 real in the extent to which it is asserted, should be the present 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 nick-name 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; that this might only be attainable through a commission of inquiry in countries in which homœopathic hospitals have been established. Can any kind of scientific object be conceived more deserving of assistance from the public purse than this would be?

With respect to hydropathy, the systematic inquiry to which it has a claim may receive a degree of assistance from pre-admitted and substantiated general principles which I have not allowed in the case of homœopathy. A court of inquiry might in some degree help themselves, in trying the facts brought before them, by the light which Curry has afforded us; but we are furnished with far more important means of distinction between cases to which hydropathy may be deemed appropriate, and those to which it is not, by the 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 may be included in the uric acid diathesis—as indigestion, bilious complaints, gout, rheumatism, and skin diseases. At Gräffenburg, in Austrian Silesia, under Priessnitz, 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 cachexia. Until Professor

* On Gravel, Calculus, and Gout, p. 54.

Liebig directed attention anew to the action of oxygen on the human body, the causes of success and failure were unknown. At Gräffenburg, 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 oxidation 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 should 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, but very serious consideration. The reality of those phenomena of trance which have been brought to bear upon the treatment of disease, and the removal of physical pain, are undeniable, however disposed we may be to exercise a chronic scepticism with respect to certain other transcendental phenomena of the mesmeric state. With respect to mesmeric therapeutics, beside other questions which would spring out of an inquiry, one question would arise of a very practical nature—namely, whether, a certain measure of beneficial results being conceded to mesmerism, the extent of benefit is commensurate with the contingent mischievousnss of the means employed. In reference to this point, I may call the attention of my readers to a case published in the October number of the *Zoist.** It is that of a lady, communicated by herself. In that statement it appears to me that "weakness remaining after an attack of fever," which constituted the complaint, is removed by the substitution of a kind of possession,† which might not unreasonably be considered more undesirable, than the weakness removed by it. The story is told with simplicity and candour, but the extent of induced influence is liable to be made subservient to the worst purposes. Is this extent unavoidable in some cases, or can the operator regulate the dose? Here, as in many other points which I could adduce, or which

* No. xxiii. p. 237.

† I have used this word advisedly, as expressing more correctly than the words "rapport," or "relation," the psychological influence established by the mesmerist 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 answer. The faculties of our physical and moral nature would indeed be circumscribed, if all were prohibited except such as never have been, or never could be, put to a vitious purpose or carried to a vitious extent. It would be a task worthy of the skill and caution of a philosopher 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 varying uses, from a very early period of history.

my reader's imagination may suggest, it should be remembered that the removal of physical evil may be effected by objectionable processes.*

Now the public has a right to demand, and to demand of *us*, some answer to the questions, whether the asserted removal of disorders on mesmeric principles has been truly effected—whether the objections above hinted at to their removal on these principles, may be over-ruled—whether, in regard to this latter point, a line can be drawn between a legitimate and an illegitimate use of the expedients of the science.

For great, indeed, is the curative effect held out by these practitioners, and held out with no slight degree of proof. The talents and high scientific position of Dr. Elliotson are well known. It would be superfluous, and, therefore, impertinent, to say, that his veracity is unimpeachable, but for the unscrupulousness with which charges of insincerity have been brought against professors of mesmerism. Now Dr. Elliotson has recently published a case of cancer, apparently absorbed under mesmeric treatment. Its cancerous nature had been recognized by Mr. Syme, Mr. Samuel Cooper, and Dr. Ashburner, as well as by Dr. Elliotson. But in fact, the cases of cure, less marvellous in kind than this of various diseases under mesmeric agency, are too nume-

* Dr. Feuchtersleben, in his searching and comprehensive view of this subject, which might stimulate our English science, wisely remarks (also) on "the cruelty of making experiments with mesmerism to gratify curiosity." "This," he observes, "is not employing it as a remedy."

rous to be put aside without inquiry. They are numerous to an extent which will induce the public to accept the *methodus medendi*, with *all* its presumable exils, unless we place it before them after investigation in a harmless form, if such a form can be devised, or convict the whole system of vice or imposture.

Such are the principal subjects, whether originated by unprofessional or professional inquirers, which have been placed out of the pale of normal medicine, as considered to bear the semblance of vice or folly, or imposture; but, in regard to which, the proofs adducible or adduced in favour of the respective doctrines, ought to be candidly and dispassionately weighed by the ruling medical body. An inquiry of this kind may no doubt terminate only in incertitude. In this case, if the requisite means have been taken to elicit truth, and to secure ourselves against error, we shall at least have done our duty. But it is conceivable, with respect to homœopathy, that as disease can arise from infinitesimal causes, so infinitesimal remedies may sometimes prove sanative: it is conceivable, with respect to mesmerism, that the influence of the trance and of the sympathy may be admitted by us to possess an extent of medical advantage, which may exceed the disadvantage of the peculiar kind of possession involved in this treatment. It is, again, not conceivable only, but quite certain, that a careful inquiry will enable us to recognize in hydropathy, not defects only, but great and manifold curative merits.

I have thus endeavoured to point out the road by which normal inquirers may penetrate into these regions, which are stigmatised by the absurd name of quackery. The occasions on which our interference is desirable must be determined in reference to the merits of the new system, or the extent to which it has possessed itself of public confidence. Such, it appears to me, are the claims of the country upon its recognised medical body. As far as the interests of that body are concerned, it appears to me most unwise that they should forego the character of arbiters implied in their position, as sanctioned and guaranteed by law, in favour of that of rivals and antagonists, whether the subject-matter before them be science or pseudo-science.

THE END.

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