Too hasty generalization a hindrance to the progress of medicine as a science : an introductory address delivered before the North London Medical Society, at the commencement of the session 1855-6 / by Charles J. Hare.

TOO HASTY GENERALIZATION A HINDRANCE TO THE PROGRESS OF MEDICINE AS A SCIENCE.

AN

INTRODUCTORY ADDRESS

DELIVERED BEFORE THE

North London Medical Society,

AT THE COMMENCEMENT OF THE SESSION

1855–6,

BY

CHARLES J. HARE, M.D. Cantab., L.R.C.P.,

PRESIDENT OF THE SOCIETY,

ASSISTANT-PHYSICIAN TO UNIVERSITY COLLEGE HOSPITAL, &c.

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INTRODUCTORY ADDRESS.

Gentlemen,

The annual re-union of the members of a Society such as ours, after the interval, which, during the summer months, occurs in our meetings, is a subject which naturally suggests to all of us many interesting reflections; and it is with much pleasure that I follow the example of my predecessor in office, and address you at the commencement of another Session of the North London Medical Society.

On the advantages which are offered by Medical Societies it is quite unnecessary that I should dwell at any length, for your presence here this evening is a proof that you fully appreciate them. It was, indeed, a due appreciation of them which led to the formation of our Society; for it was strongly felt by many of yourselves that a large district like this, where are congregated so many of our brethren, ought not to be without such advantages

near at hand. If larger and more central Societies have some points of superiority, local re-unions, like ours, possess theirs likewise: we find in the latter less of constraint, and, at their meetings, many speak, who, from various causes, will take no part in the discussions at larger Societies: with us, too, the friendly element (no small merit) predominates perhaps more, and a circumstance of not a little importance, practically, is, that the Society's Rooms, being comparatively near the residences of most of the members, the meetings can often be attended by those, who from the calls and exigencies of a professional life, could not absent themselves for an evening far from home. I trust that the number of members we possess is sufficient proof that the requirements of the neighbourhood were not wrongly estimated, while it is highly satisfactory to be able to state, that on no occasion since the commencement of the Society have we been without a valuable Paper ready for the evening's meeting. The Papers presented have been, indeed, no less varied in their subjects than interesting in themselves, and have embraced questions relative to Medicine and Surgery, to Pathology, Diagnosis, and Therapeutics, and to various important topics connected with our profession.

I hope, and indeed, believe, that the success which has thus far attended the North London Medical Society is but an earnest of its still greater

usefulness in future; and as its advantages become more generally known amongst our professional brethren in this neighbourhood, I think we may fairly anticipate a still further and very considerable increase in the number of our members. Within certain limits—and these are wide ones—the more numerous the members, the greater are the benefits which such a society confers upon each individual member, for on any given subject more thought, and knowledge, and experience will be brought to bear. If it be a difficult point which requires elucidation, or an obscure case which is mentioned, and on which it is desirable to have the opinions of others, with a view, perhaps, to more efficient treatment, the more likely is it that those will be met with who have paid especial attention to the point under consideration, that the difficulties of the case will be cleared up, and that a plan may be suggested likely, according to the nature of the circumstances, to alleviate the suffering, or to cure the disease.

Indeed, I hold it to be, with a medical man who has the opportunity, not a matter of choice whether he *should*, but of duty that he *ought* to belong to some Society such as ours: it is of advantage both to him, to his brother practitioners, and to those placed under his and their care. It is equally true of the mind and intellect as of physical substances, that friction tends to polish them, and by this

means hidden qualities may be brought to light, and many excellencies disclosed which would never otherwise be discovered: intercourse with those engaged in the same pursuits, whose objects are alike, and whose training and education have been similar, cannot but tend to maintain active in us that interest which might otherwise flag, and to keep alive that spirit of investigation which ought always to animate us. Those, indeed, who most regularly and zealously attend the meetings of such societies, are they who can most truly appreciate the benefits they offer, and bear witness to the amount of information and advantage to be derived from them. In a profession so practical as that of medicine, and which depends so much on observation, there is no one so learned that he may cease learning—none so inexperienced that he may not have more or less the power of adding to our stock of knowledge; and if he have the power, then the duty of doing so necessarily follows. Lord Bacon says: "I hold every man a debtor to his profession, from which, as men of course do seek to receive countenance and profit, so ought they, of duty, to endeavour themselves, by way of amends, to be a help and ornament thereunto." But there are many men stored with information, men of thought and experience, men ready against any emergency they may have to encounter, and who yet—either from their having but little leisure or from their dis-

like to writing, their love of retirement, or from some such cause—never give the profession the advantage of their experience, which might thus die with them did not opportunities and circumstances—such as our Society offers—call it out, and in this manner render it useful to others as well as to themselves. To this end the arrangements as regards our meetings are peculiarly well adapted, for at the commencement of each evening a certain time is allotted before the Paper is read and the more formal discussion which takes place on it ensues, for the reception of casual communications and friendly conversation on any topics connected with the profession. While the researches embodied in the written papers and the detailed cases are those, probably, most likely to advance the *Science* of Medicine, these oral communications are very valuable for our art, as in them the leading features of interesting cases are portrayed in a few broad outlines, the results of experience are concisely given, or suggestions offered which may lead to the best practical results: the observations dropped, indeed, in this casual manner, may often give rise to abundant fruit by suggesting new trains of thought and new ideas, by directing the attention of individuals to certain points of which they might not otherwise have thought, or by leading them to researches by which truths may be more clearly explained and more firmly established: or, what is not less

important, by which received fallacies may be exploded, so as to render the onward path of real science more free and certain.

I have alluded, then, to what are the principal advantages, besides the one of social intercourse and of the stimulus to which this gives rise, afforded by such societies as our own; and they may be briefly stated to be: the facilities they give for the communication of facts, for the promulgation and discussion of valuable researches, and for obtaining on any given point the opinions of others. Nor can we look upon these advantages otherwise than as great, for on Facts and Opinions the whole practice of Medicine is based, and these—or more properly speaking, Facts and true Inferences from them—are likewise the elements on which the advancement of our profession must alone depend. But if it be our desire that the Science of Medicine advance steadily and surely, it is above all things necessary that the facts upon which the whole superstructure rests should be well observed, well sifted ones; no doubtful assertions must be allowed to assume their badge, no mere matters of supposition permitted to place themselves in their ranks: the facts must be *real* facts, well ascertained, indisputable. If, moreover, we would advance to any beyond the simplest propositions, the observations on every point must be numerous too; and after all, having been collected in sufficient num-

bers, and compared with one another, a careful spirit of deduction must be brought to bear upon them, rigorously determined neither to go beyond or stop short of the point to which the data lead—neither to swerve to the right hand nor to the left of that narrow path—difficult, it may be, to keep in, and not always at first sight enticing to follow—but which alone can conduct us to certainty, and make us feel that we are progressing without danger of our road giving way as we pass along it.

If two or more *sufficiently extensive* series of facts on any given subject be observed with equal care by competent individuals, and the conclusions to which they really lead be carefully deduced, the results thus arrived at will generally, *cæteris paribus*, be similar. But instead of this unity being manifested, alas! what difference of opinion do we too often meet with! One who has seen a few cases bearing indirectly, or more or less directly, on the subject under consideration, states that such and such have been the results of *his* experience; while another, also appealing to *his* experience, is led to adopt conclusions of a very opposite character. Now what is the cause of this difference? Whence results the discrepancy?

Why, in the first place, each one does not always set clearly and distinctly before him the question at issue. Before a problem can be pro-

perly solved, it is necessary that its full import should be perfectly apprehended: before a proposition can be rightly reasoned upon, its terms and true signification must be perfectly understood, and it must not be supposed to affirm either more or less than it actually does affirm; while, on the other hand, the arguments brought in favour of or against a statement should have reference to what it asserts or denies, and to that alone. Without this clearness of view and precision of idea as to what is the object to be aimed at, argument is profitless, and instead of advancing science and real knowledge, tends only to mystify and confuse the mind.

Again, a fertile source of disagreement in opinion rests in this, that the cases brought in support of or against the point in debate, or from which the so-called experience is derived, are often not strictly comparable ones; the illustrations adduced in support of or in opposition to a particular view may be derived from instances of the same disease, but the circumstances under which it has occurred, the difference in the age, sex, or condition of the patient may be such, as to render the two or more cases brought forwards, though alike in some points, so dissimilar in others, that, strictly speaking, they are of little or no use in settling the question under consideration. It requires either that the cases adduced should be similar ones, as to all their con-

ditions and circumstances, or else that, by careful investigation, it have been *previously* proved that the points wherein they differ are such as not to render them unfit for comparison; this, however, must be proved—not taken for granted or merely assumed.

Another reason which may be assigned, is the peculiarity of individual minds, owing to which precisely the same object or case is seen by different persons under different points of view and with different impressions. The temperament of the individual often much influences the results of his observations, but the erroneous impressions left on the minds of some persons after the observation of any given circumstance or event may not unfrequently be traced to their being previously prejudiced in a particular way, and their liability in consequence to see only what accords with their preconceived opinions, so that facts are very apt, when they oppose any favourite theories, to be disregarded or even perverted. But a real determination to observe faithfully, and lengthened practice in so doing, will go far to remedy this source of fallacy; and unless investigations be conducted in a really unprejudiced manner, the probabilities are *not* small that the conclusions arrived at will be very wide of the truth.

But of all the circumstances which give rise to discrepancy of opinion, to the contradictory assertions we so often meet with, and to fallacious

theories, by far the most frequent is the habit of too hasty generalization. This has been the great bane to the progress of the science of medicine, almost from its earliest dawn to the present day. If you trace its past history, examine the phases it has undergone, and consider what has been the origin of its past extravagancies and of the absurd doctrines—Homœopathy, Hydropathy, Isopathy and the like—which, in the present day, while they screen themselves under the name of our science, are not less opposed to it than they are to common sense and to the results of experience, you will constantly meet with evidence of the working of the cause to which I now allude. In every stage of its progress there have been some bold speculators propounding theories which have been for the while enthusiastically received, but which, after flourishing for a time and for so long influencing practice, have then sunk to be heard of no more. Observe how these theories, holding men's minds captive for the time, and making them see every fact only in such a manner that it would fit in with their cherished doctrine, have and must have impeded the onward course of true science: and then consider how infinitely farther in the right path medicine would now have been advanced, had careful deduction from sufficient data been employed, instead of too hasty generalization from a few isolated facts.

It would be much out of place, if I were here, Gentlemen, to enter into anything like a history of medicine in its different phases, though I did so to illustrate the retarding influence which crude theories and wild speculation have had on its real progress and true advancement. It is sufficient that I remind you, for a moment, of the time and talent wasted or misused in supporting the dogmata of the different schools and systems which in different ages have been predominant—that I mention the Dogmatists, Pneumatists, and others of ancient times, and how, in comparatively recent periods, all the phenomena of vitality and every action of the body were looked upon as only so many chemical processes, while at another, and shortly subsequent period, every change and condition of the body in health or disease was deemed explicable on mechanical and mathematical principles alone. You remember how to these Iatro-mathematicians succeeded the sect of the Vitalists, whose opinions were at one time taught from the professorial chairs of almost every university, and who explained every act and change in the system by reference to the Archæus which had been created by the fancy of Van Helmont, or the presiding Anima which owed its existence only to the visionary mind of Stahl. Nor will I occupy your time by detailing the origin and fall of the systems of medicine which have since prevailed—with the bold

announcements of the Brunonian theory or the sweeping assertions of the Broussaian—nor do more than remind you of the extravagant lengths to which certain fashionable systems of medicine and theories of disease have been carried in times much more nearly approaching our own.

It is not by means such as these that medicine has advanced, nor should I have referred, even briefly as I have done, to so many discarded theories, were it not that they tend to teach, I think, a useful lesson, and that they have a direct bearing on the subject which I wish to illustrate. They have been propounded by men of talent and learning; have each for a time flourished and had numerous and devoted disciples, but each one in its turn has gradually disappeared, after having done so little to advance our science to its present forward state, that we can trace but little proof in it of their having existed, save in some terms of medical phraseology which still exist, and which took their origin in the different schools of the Chemists, the Mathematicians, and the Vitalists. Nor ought we, in reality, to be surprised at the disappearance of such systems, for, on examining the foundations on which they were constructed, we have seen that mere speculative hypothesis was at their base, or a few facts from which, with an unphilosophic spirit, too sweeping inferences and too hasty

generalizations had been made. Their inapplicability shows how finite are the boldest ideas and most comprehensive system which man can raise, compared with the infinite order, beauty, and perfection of those laws which govern the works of Nature; and how impossible it is rightly to approach the study of these, with a view to comprehending them, except the enquirer be "possessed with a simple and fervent desire to arrive at the truth, without any predilection in behalf of any opinion whatever, and without any other disturbing emotion of hope or fear, affection or dislike."*

I neither assert nor think—far be it from me to do either—that theories are useless: but in order that they may not be so, and that they may advance rather than retard the progress of a science—that they may be likely to stand their ground instead of being swept away, as so many of those have been which have hitherto encumbered the path of medicine—they must be founded on sufficiently accurate knowledge, and ought, before being woven into a *system*, to be rigidly tested by an appeal to repeated observation: while, instead of this, in the various systems to which we have alluded, reason gave place to imagination, and vague speculation prevailed over close thought and patient induction. In what bright contrast to these speculative theories do those discoveries stand which have been founded on another and a

* *Essay on the Pursuit of Truth.* S. Bailey.

better mode of reasoning; and how much more have they tended to advance and add lustre to our science. A glorious idea had entered the mind of Newton, but for sixteen long years did he lay aside the splendid hypothesis he had conceived, because, when brought to the test, facts and it did not seem to agree—because the results of direct observation on the velocity of the motion of the moon and of calculation deduced from the then supposed length of the earth's radius did not coincide. On what, under those circumstances, could be considered only a bare hypothesis, Newton had too truly philosophic a spirit to build a system, however beautiful the system might appear: but when the earth's radius had been accurately measured, and that same hypothesis was found to coincide with observation, he announced it as the grand law of universal gravitation—a law which, so nearly as the human mind can comprehend a subject so sublime, explains to it the means by which our Maker guides and regulates the motions of his universe. Influenced by such a spirit as this, and proceeding by a true inductive method of reasoning—such as Bacon taught and Newton followed—Harvey, after years of careful study and research, made out the true course of what Dryden terms,—

"Those circling streams once thought but pools of blood."

And another of our countrymen, after a quarter of a century of thought, labour and investigation,

announced that glorious discovery by which his name will be immortalised for saving thousands upon thousands of lives, and placing in our hands the means of almost—perhaps entirely—exterminating one of the most destructive of diseases. I may ask you what it is that has made the writings of Sydenham a standard of reference at the present day, while those of many who obtained, while they lived, as great a reputation as he, are scarcely so much as heard of now? What is it but that he contented himself, for the most part, with being, according to the phraseology of Bacon, "*Naturæ minister et interpres?*" What is it that has stamped value upon the works of Willis and Haller, of Laennec and Louis? It is that these men knew how to observe, compare, analyze, and how to draw unbiassed deductions from well ascertained facts. Speculation might have gone on to the present day, and left us still ignorant of the circulation of the blood, and of all the facts that have flowed from that discovery; it might still have been constructing systems of medicine, and have left us unarmed against the terrors of the small-pox.

It is, indeed, in proportion as men have been content to put aside vague fancies and speculations—misnamed "theories," for careful observation, just comparison, accurate analysis, and clear, unbiassed, legitimate deductions, that our knowledge has obtained precision, our data accuracy,

and that medicine has advanced as a science. These are the means by which it has attained its present forward position, and these means are necessary for its future progress.

But to obtain the results to which these means tend, thought, care, and labour are alike required; a sanguine temperament must not lead to too hasty conclusions, nor a desire for early reputation induce the assumption of greater discoveries than those to which the data actually lead: there must be clearness of understanding to sift evidence closely; there must be accuracy of perception to avoid being led astray by loose analogies.

I have laid considerable stress upon the importance of generalization from facts not being too hastily made, and have stated that for the results to be correct, the number of facts observed must be sufficiently extensive. The number of facts, however, required in order to attain accuracy varies much with the nature of the question to be solved, and must be great in proportion to the complexity of the latter—a very large number being necessary in some cases, while, on the other hand, there are points which a single undoubted fact, or which at most two or three facts may be sufficient to establish. Thus one observation might prove that gangrene of the foot may coexist with obliteration of the arteries of the lower extremity; that a certain peculiar eruption may

occur in the course of typhoid fever; or that the gums may present a particular purple appearance in an individual who has been exposed to the action of lead; while it would only require as many accurate observations as there were individuals affected, to demonstrate that cancer may attack several members of the same family, and that, too, for two or three generations in succession: but it would be exceedingly wrong to *assume from such data* that the gangrene of the foot depended upon the disease of the arteries; that the maculæ are important guides in our diagnosis and prognosis of fever; that the colour of the gums is one of the most marked symptoms of the impregnation of the system by lead; or, in the last case, that cancer is an hereditary disease. Each of these assumptions *might* indeed be true, but the above data would be totally insufficient to prove them to be so; and the evidence brought forward to support such a supposition must be based, not on one or two observations, the results of which might be purely a matter of coincidence, but on a large series of facts, by which the chance of error from mere coincidence would be eliminated in proportion to the number of observations of which it (the series) consisted.

Again, another mode, in which too hasty generalization leads to error, is the attempt not unfrequently made to draw deductions from facts which the nature of the latter (however important and

valuable the facts may be for the deductions of some other conclusions) renders them incapable of yielding. "Many things," said Dr. Reid, in his *Essays on the Intellectual Powers*, "called deceptions of the senses, are only *conclusions rashly drawn* from the *testimony* of the senses. In these cases the testimony of the senses is true, but we rashly draw a conclusion from it which does not necessarily follow. We are disposed to impute our errors rather to false information than to inconclusive reasoning, and to blame our senses for the wrong conclusions we draw from the testimony." Many examples illustrative of this, will at once present themselves to the mind:—A patient, let us suppose, comes for an opinion relative to some pulmonary complaint with which he believes himself affected; his chest is examined, and under one clavicle especially there is found dullness on percussion, increased fremitus, and vocal resonance with decided bronchial respiration, and we will suppose, if you please, no other physical tokens of disease. Suppose again that from these signs it was concluded the patient suffered under tubercles of the lungs, but that at a subsequent period it was proved that the patient was free from such disease, though there did exist consolidation of the part of the lungs indicated, from disease of another kind. Under these circumstances, were the observer to throw the blame of

the error in diagnosis on the physical signs, or to say that his sense of hearing had in this case deceived him, he would certainly lay the blame in the wrong quarter; for it was not the physical signs, as ascertained by the use of his senses, which told him there existed tubercles, but the wrong conclusions he came to from the true information which his sense of hearing afforded him: in reality, the dull sound elicited on percussion merely proved that *some* cause existed which prevented those sonorous vibrations of the parietes taking place which are usually produced by percussion over that part of a healthy chest, but it by no means shewed *what* this cause was; it *might*, indeed, be tubercles in the lungs, or it might be anything else producing consolidation of these organs, or it might be something between the parietes and the lungs. Further information, indeed, was given in the case supposed, by the other signs, and, being carefully analyzed, they might fairly lead to the inference that there *did* exist consolidation of the lungs, but they *did not* and *could not* of themselves prove that tubercles were the cause of the consolidation: that conclusion was arrived at, therefore, from reasoning of a different kind, probably from associating together the considerations that tubercles occurring in the lungs usually occupy that portion of them especially which in the above

case was found to be consolidated, and that, *if* tubercles existed there, they might give rise to such signs as were discovered in the case in question. Had it, indeed, been previously proved by repeated observation that no other conditions of the lungs, excepting the presence of tubercles in them, would give rise to the combination of signs alluded to—then, when these signs were met with, the conclusion that tubercles existed would manifestly be a just one; but such a position not having been established, the physical signs could, accurately speaking, merely be interpreted to indicate a certain physical condition of the lung, without proving what was the precise *nature* of that condition. Hence, in the case just supposed, it was not in reality the physical signs which were the cause of the erroneous conclusion arrived at, but it was the too hasty deduction from them of an inference which, when strictly analyzed, they evidently would not warrant.

But not only is there commonly a great tendency hastily to draw general conclusions, on the one hand, from data which really do not lead to them, and on the other from facts insufficient to establish them, but it not unfrequently happens that an observer is so much struck with some *single* fact or event which makes a strong impression on his mind, that it is apt in future unduly to influence his opinion. We can all of us, doubtless,

remember certain cases of different diseases which, —not from any peculiarity of their own, but in consequence of the attendant circumstances, or on account of the peculiar condition of our own minds at the time, or from some other such cause—are thus, more than any others, strongly impressed on our memory. Hence, we are apt to hold up, as it were, such cases to the mind's eye as types of the particular diseases—to consider some individual fact as the expression of the general rule—and any subsequent observation which we meet with, and which resembles the one that struck us so forcibly, is very apt to be remembered, while others, which are unlike it, are easily forgotten; so that, after a while, on reflecting upon our experience, and at the same time trusting to our memories alone, we shall be not unlikely to consider the former class as the one of most frequent occurrence, while, if the observations which our experience furnished could be recalled accurately to mind, it might prove that those which we considered to represent the general rule were in reality the exceptions. It is thus that we often omit to take into account cases which *ought* to have influenced, and which, had they been properly impressed on our memories, probably *would* have influenced our conclusions; and thus also can be explained many of the erroneous impressions which give rise to much of the diversity of opinion

often expressed as to the frequency with which certain complications are met with in any given malady—the occurrence or rarity of particular symptoms—the advantage of some plans of treatment over others on the duration of disease—and upon various other similar questions. As we are thus so liable to be influenced in our practice, by the impressions retained of individual cases, no stronger argument could be adduced to shew the desirableness of all our observations being made with care and accuracy. We must ever remember, too, that accumulated experience, of whatever extent it may be, consists ultimately of the observation of particular cases, and that unless these observations be made in the first instance faithfully and accurately, they vitiate whatever results may be deduced from them. We have here, then, another mode in which a too hasty generalization may give rise to and propagate errors—errors not only hurtful in themselves, but likewise injurious to the progress of Science; for it should ever be borne in mind that any false statement or erroneous theory requires proofs to be brought against it in order to destroy it, just as much as a right statement or a correct doctrine has required the production of proofs in order to establish it: so that as much talent and labour may have to be expended in exploding the one as in confirming the other.

If, therefore, we would rectify such errors as

those I have alluded to, and into which otherwise we are continually liable to fall, we must have recourse to a strict inductive mode of reasoning; our facts must, as I have said before, be certain ones, and our deductions from them accurate and impartial. Objections have indeed been urged against the application of the numerical method to medical subjects, and it has been said that such a plan is little or not at all adapted to questions connected with the science of medicine. True it is that, if misapplied or ill applied, the employment of numbers in the investigation of any subject whatever may give rise to most erroneous conclusions; but every mode of research is liable, more or less, to the same objection—an objection, be it remembered, which has reference to the misuse and wrong application, not to the proper use and right application of the method employed. Though the adoption of the plan alluded to may, certainly, in some instances be difficult, owing to the complexity of the question at issue, the principle must surely be held correct when we consider that the numerical method is one by which precision is given to our knowledge, and by which the results of our experience can alone be *accurately* analyzed. Indeed, all who appeal to their experience in support of their statements, and those very persons, even, who argue against the numerical method, are constantly —loosely or vaguely, perfectly or imperfectly—

employing it. For when any one asserts that he has "generally" found such and such to be the result of his experience, or that the plan of treatment which he proposes is the one which in his hands has been "the most successful," he manifestly compares numbers, however inaccurately he may do so, and however unconscious he may be of the mental process. It is by such a method that other sciences have been advanced; it is by such a one that we must look for the true advancement of our own. It is not thus, indeed, that a brilliant reputation is at once to be achieved, or that speculations which dazzle and allure can in a moment be arrived at; but we may rest assured that whatever progress is thus made will be of a permanent kind, and that ultimately views will be evolved by it far more beautiful and comprehensive than any theories which are the result of mere abstract speculations, because they will be the exponents of Nature's laws, stamped with her die, and marked with the impress of truth.

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