On the signification of fact in medicine, and on the hurtful effects of the incautious use of such modern sources of fact as the microscope, the stethoscope, chemical analysis, statistics, &c; / by Wiliam Seller.

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INCAUTIOUS USE OF SUCH MODERN SOURCES OF FACT AS THE MICROSCOPE, THE STETHOSCOPE, CHEMICAL ANALYSIS, STATISTICS, &c.

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SIGNIFICATION OF FACT IN MEDICINE, &c.

It is perhaps true that there cannot be two opinions as to what constitutes fact, either in medicine or in any other department of science. Whatever is termed fact, every one is ready to exclaim, must be an absolute truth, unadulterated by any admixture of error. Of truths of this kind, beyond all doubt, the actual order of nature is made up,—of truths which are facts in the arrangements of the universe and of all its parts, inorganic and organic. But it is not less true, that it is from a mere abstract conception of truths of this kind, in the economy of nature, that our principal idea of the term fact in science is drawn; and this being the case, is it not plain that we come to apply a term framed on our conceptions of the perfection of nature to appearances more or less contaminated with the imperfections of human observation? Thus, what are called facts in human science, do not necessarily represent with exactness the facts of nature. Our representations of the facts of nature, though termed facts by courtesy, may, and often do, involve errors of very material moment. Notwithstanding the proverbial authority of the direct observations of sense, there were not wanting unsuspected errors in what were upheld as facts, even in those early times when the interrogation of nature was carried on solely by the unassisted use of the senses; and therefore there would be less advantage than might be expected, were the term fact confined to observations made in this simple manner. But, in the actual state of science, it is indeed impossible to limit the term to phenomena so easy of determination. As knowledge advances, nature must be scrutinized by more refined methods than belong to its earlier progress, and then truths, or supposed truths, which are exactly parallel, and in every way strictly analogous to the immediate observations of sense, come to be ascertained,—some, for example, by the assistance of a more or less complex apparatus,—others by inference from a larger or smaller succession of phenomena,—others by numerical methods of research,—others, again, by deduction from a previously-established general principle or law. Truths so de-

termined, though currently termed facts, can seldom be at first unmixed with error. Organic nature, above all, with which almost exclusively medicine has to deal, illustrates, in particular, the difficulty of reaching truth of fact otherwise than by successive approaches. But in every department of science, it is only the most gross and palpable truths that are discovered at once without any admixture of error. Whenever there is complexity, the approach to truth must be gradual, even when that truth is most unequivocally a fact. What can be more purely a matter of fact than the place of a fixed star in the heavens?—yet of how many errors must the earlier determinations be cleared before its actual position be ascertained with precision? No doubt it might be said that such truths as the place of a fixed star are, from the beginning, facts in the order of nature, but that these do not become facts in science till determined beyond the reach of error. This is to introduce a distinction between facts in nature and facts in science, which would be a source of endless confusion. It is better to take things as they are, and to speak of what are plainly facts in nature, as facts in science also,—yet, with this reserve, that though parts of science, they may stillfall short of being absolute truths.

Much confusion, however, could not fail to result, if effects referred to a cause were included under the head of facts, as sometimes happens from inadvertence. The distinction is for the most part easy. The phenomena of inflammation, as determined by the microscope, namely, the narrowing of the capillaries and the more rapid motion of the blood, followed by the dilatation of the capillaries and slow motion of the blood; its oscillation and final stagnation; and lastly, the exudation of the *liquor sanguinis* through the walls of the capillaries,—are plainly facts, or supposed facts. But it is not a fact that inflammation is cured by blood-letting: it is the expression of an effect referred to a cause. In like manner, it is not a fact that colchicum relieves gout and rheumatism, or that sulphate of quinine cures agues; nor is it a fact that the stimulus of the blood makes the heart contract. All these are not facts, but alleged effects. The vesicular breath-sound in the healthy lung, the friction-sound in pleuritic inflammation, the crepitant rattle in inflammation of the substance of the lungs, pectoriloquy in excavation of these organs, are simple facts under the use made of them in practical medicine; though, when in the theory of the stethoscope an attempt is made to account for them by pointing to the several physical circumstances corresponding to each, they immediately become effects. Yet by becoming effects, they do not cease to be facts; since, when things truly stand in the relation of cause and effect, the cause

is a fact, and the effect is a fact, though the relation between them is not a fact, being the result, not of a knowing but of a judging faculty. But to continue. It is a fact that the colourless part of the blood-clot is the same substance with the fibrine of the muscular tissue, circuitous as is the process by which this is determined. It is not, however, a fact, but the alleged effect of a cause, that this same fibrine is a principal part of the nutriment of living bodies. It is a fact that more males are born than females. It is not a fact, but only an alleged effect, that ergot of rye dilates the uterine orifice, or that chloroform abolishes the pangs of child-birth. It is a fact that John and Peter must die, while it is only an alleged effect that exercise and temperance are favourable to longevity.

It is not, indeed, very material by what name a piece of knowledge is called, provided its truth be beyond question. And the only real use of such views as I have been insisting on is to put us on our guard against being misled by words,—to keep before the mind the possibility of statements loosely passing for facts, containing a greater or less amount of error.

It must be plain, then, from the instances just referred to, that modern medical facts are apt to differ widely from the simpler facts of early times, being of a much less absolute character, and free from error only in proportion to the perfection of the methods of research employed. Hence the necessity, so much overlooked, of a continual retrospect to the so-called facts forming the foundations of medical theories, for the purpose of amending their deficiencies, and pruning them from error. And hence also the necessity, to borrow an illustration again from the language of astronomy, of taking pains to understand the theory of our instruments of research, whatever these may be, so as to become acquainted with their laws of error, and thus to anticipate the corrections necessary on their results.

In proceeding to consider the hurtful effects of the incautious use of the several modern sources of medical facts before referred to, I wish to call the attention of the Society not merely to the evil of mistaking imperfect observations for absolute truths, owing to defects inseparable—at first, at least—from such instruments of research, but in particular to the much more mischievous consequences of an overweening estimate of the practical value of the knowledge derived from these sources, as compared with the accumulated truths of medicine throughout past ages. I am very far from underrating the importance of any one of these methods of research as regards the future progress of medicine. Each of them is plainly capable of making incalculable additions to medical science. But though the

seed be sown at present by many labourers, the harvest is far distant. And what we have to complain of is, that the more ardent partizans of these several methods continually mistake this distant prospect of harvest for a great present advantage. They contrast their mere conception of what medicine is to be, ages hence perhaps, when it shall have exhausted all the aids of their chosen methods of research, with its present imperfections, and ask us triumphantly to adopt at once a new phraseology, new doctrines, and new methods of treatment, as if the medicine of past times contained nothing worthy of any further serious attention.

It is already apparent that microscopic medical facts are slowly changing the whole aspect of pathology. And it is not easy, I freely confess, to overrate the benefits which the microscope promises to confer on medical science. But the greater its capacity for good, the greater also is its tendency to evil. The microscope takes up anatomy, healthy and morbid, where the simple use of the scalpel fails us. It would be superfluous to insist on the inestimable services which anatomy, normal and pathological, has rendered to medicine. And as an instrument for the enlargement and extension of this kind of knowledge, the microscope is justly upheld as of unrivalled utility. But if common morbid anatomy had been all along confined to its proper sphere, the other important aids of pathology being permitted to retain their due influence, medicine would have proceeded more steadily in an onward progress. Morbid anatomy for a time usurped exclusively the name and office of pathology; diseases were seen only through this medium, and the rules of practice were bent into conformity with its crudest suggestions. It was a common error—one not yet wholly exploded—to mistake the effects and occasional complications of a disease for its actual essence, thus rashly converting the proximate causes of the old pathology into sensible alterations of tissue. Inflammatory action was for a while recognised as the root of maladies in general; fevers were idiopathic inflammations; hemorrhages were inflammatory; dropsies were inflammatory; asthma was inflammatory; dyspepsia was inflammatory; the gastro-enteritic tendency was the ruling morbid character; while the lancet, leeches, cupping, and saline purgatives were the almost exclusive weapons of the practitioner; narcotics, astringents, tonics, and especially chalybeates being regarded with horror as little short of instruments of assassination. Microscopic morbid anatomy, if left uncontrolled, threatens us with a similar train of erroneous views on the subject of the essence of diseases, and the like deviations from sound precepts of practice. But it would be inexcusable if, having just felt our escape from the

trammels of views entailed upon us by the incautious application of common morbid anatomy to the investigation of diseases, we should plunge into errors in every respect analogous by a like blindness in the use of microscopic knowledge of the same kind. While, then, all due weight is given to microscopic discoveries, let us insist that every innovation shall be rigidly scrutinized before being adopted in contradiction of any long-received view of the old pathology. And in making this scrutiny, let it not be forgotten—1, that admirable as the microscope is continually proving in diagnosis, it does not therefore follow that it is a safe guide to rules of practice; 2, that the sensible alterations of tissue made by diseased action are but a small part of pathology; 3, that the whole effect of the variation of external circumstances on the course of diseases, including the *juvantia* and *lædentia* of the old schools, should decidedly control the practical conclusions drawn from such alterations of tissue; 4, that medicine still is, and must continue for ages to be, an empirico-rationalism; 5, that the day of a purely rational practice is far distant, being rather a day of hope than of certainty; 6, and that the attempt to cultivate medicine as a system of pure rationalism has invariably proved the highway to error.

The limits of this paper preclude me from entering into any lengthened illustration of what is here laid down. The single example I propose to offer is chosen for its general nature, rather than on account of having any very particular adaptation to the point under review.

One of the most striking effects of the microscope on the study of diseases arises from the discovery of sensible alterations of tissue in affections hitherto regarded as functional. But the distinction of diseases into functional and organic is one of the principal features of the old pathology. This distinction the microscope threatens to subvert and render useless, because the diseases which it shows, and is hereafter likely to show, as organic, essentially differ from those hitherto designated by that term. Now, before we consent to the virtual abolition of a distinction so well founded as that of diseases into functional and organic, it is proper to consider whether the new light promised by the microscope do really necessitate the sacrifice. The microscopist, doubtless, will exclaim, How can you retain amongst functional disorders maladies which are proved to consist in an organic lesion? To this it might be answered, that it would be better to do so than to sacrifice pathological truth to mere propriety of words. But it is obvious, that an arrangement may continue to rest on characters the most perfectly natural, though the progress of knowledge may have rendered the terms originally bestowed on it inexact

or literally inapplicable. An arrangement is justly described as natural when the members of the several divisions agree with each other in many more particulars than that to which the name of each group points. Do the members then, of the two groups in question, possess any other striking feature in common besides the dependence of the one on a sensible lesion of structure, and the absence in the other of any such lesion? The ready answer is, that organic diseases, in the original sense of the term, are incurable, while functional diseases are curable. And the real difference between functional and organic diseases turns out to be, that the one order of maladies is dependent on internal changes susceptible of being removed by living action, while the other consists in the presence of permanent alterations. Thus the old distinction holds its ground on a practical basis. And the proper object of the microscope should be, not to introduce confusion into the older system, but to point out, among the alterations which it discovers in the tissues, what are transitory and what permanent, these being disposed of under appropriate names in groups corresponding to the ancient arrangement.

This, however, is not the view taken by most microscopists. They are apt to regard the discovery of a structural alteration in a disease before termed functional, as a triumph of the microscope over the old pathology—as the detection of an error—as the application of a new principle. But their triumph is premature; they have merely discovered a new fact in the history of the disease, they have realised what before lay only in speculation. For the old pathology never denied that the so-called functional diseases depended on physical alterations. It was never taught under any system of medicine, that deviations from health could manifest themselves without a corresponding physical change on the tissues of the organ or part concerned. The old pathologists believed in the existence of a change, though they could not detect it. And even when the change was manifest, they referred it, if transitory, to functional diseases. Inflammation, for example, even in its earliest stage, implies important physical alterations in the affected parts; yet at least this stage of inflammation, has been always considered of a functional character. Hepatization of the lungs, as a physical change, is as great as most of the changes likely to be detected by the microscope in the so-called functional diseases; yet being removable, no one has thought of placing it among the organic diseases of the chest. Even the humoralists could not have denied the dependence of diseased action on structural changes, provided it had been first explained to them, that the microscopists pique themselves on the small verbal novelty of

representing vitiations of such fluids as the milk, the blood, &c. as being alterations of structure.

The microscopists, no doubt, may urge, that the changes of tissue corresponding to the old order of organic diseases, are permanent only on account of the extent to which the morbid action has proceeded, and that, in its first stages, it is capable of being removed by the excitation of living power. But this is not a kind of reasoning which avails in practical medicine. The peculiarity of an organic disease, in the old sense of that term, is less in the action by which it originated, than in the effect of the ultimate change of texture on the function of the part itself or adjacent organs. Such a disease, in its matured state, that is, when termed organic according to the old rule, is a very different disease from what it was at the time when its beginnings could have been controlled by a knowledge of the morbid action in which it took its rise.

Thus, under whatever aspect this subject is viewed, it is still apparent, that the expressions transitory morbid alterations and permanent morbid alterations hold the same relation, and preserve the same correspondence to each other, as the older terms functional diseases and organic diseases, and that these last terms, though inexact, refer to a pathological distinction of much utility on which the microscope should not be suffered to encroach.

The next modern source of medical facts to which I refer is the stethoscope. As respects this instrument, I merely wish to notice the tendency which it, as well as the microscope, has to introduce confusion, by more or less altering the sense in which many terms used in the old pathology of the chest were received. One example will suffice to show the nature of this confusion. Thus, the peripneumonia of former times differs essentially in many respects from what may be termed stethoscopic peripneumonia,—that is, from peripneumonia which is not discoverable without the aid of auscultation and percussion. The effect of the use of the stethoscope has been to increase very greatly the number of cases of disease referred to peripneumonia. While the general symptoms were relied on, many cases of peripneumonia must have escaped detection, being considered as catarrhal fever, common cold, or ordinary febrile disturbance. And, as in most of these cases the symptoms had disappeared under confinement and slight treatment, no suspicion arose from any untoward result of an oversight having been committed. Thus the term peripneumonia was restricted till of late to cases of severity and danger—to those cases in which the lung was affected to no small extent, and the constitutional

reaction proportionately great, as indicated by oppression of the breathing, often amounting to orthopnœa, cough, turgescence and lividness of the countenance, and much febrile uneasiness. The disease was always accounted both difficult of detection, and of the most dangerous character; and the danger was justly held to be extreme if it ran its course undiscovered. But the stethoscopic peripneumonia, taken in its most restricted sense, is far from coming up to this character. It is a disease in which the patient may suffer but little, which often passes away spontaneously, or is cured by slight treatment. Or, if this should appear to be a statement which underrates its severity and danger, it is at least true that cases are at present published almost daily of peripneumonia detected by the stethoscope, which disappeared under the most moderate treatment. And there is no good reason to believe that any considerable proportion of the cases of peripneumonia, which in former times escaped detection, owing to the defect of the then recognized signs, had a fatal event. The treatment appropriate to the trivial affections they were taken for sufficed in general for their cure. What is here said, however, is not to be understood as having any reference to the dangerous form of the disease, which is strictly termed latent peripneumonia. Here the peculiar merits of the stethoscope are conspicuous. The general symptoms do not fail to arise, owing to the small degree and extent of the inflammation, but from a defect of the power of reaction in the system, the result of some previous or simultaneous disease. With this exception, the old physicians seldom failed, if not to detect with certainty, at least to suspect the presence of the more dangerous forms of peripneumonia. It is one of the diseases which they looked for with an anxious eye; they knew its obscurity; they knew its danger; and they allowed no suspicious cases to run on without resorting to what would be considered at present as an excess of the antiphlogistic treatment.

Pure peripneumonia, in the old pathology, was regarded as rather a rare disease; it was best known in its combination with pleurisy, as pleuro-pneumonia. Stethoscopic peripneumonia is more frequently pure than the old peripneumonia. This is what might have been anticipated. But neither is it to be forgotten that pleuro-pneumonia, the most frequent form of the old peripneumonia, is more dangerous than simple peripneumonia, and that the implication of the pleura very uniformly denotes a greater extent and severity of the pulmonic inflammation.

From these considerations, it obviously follows that the stethoscope has attached essentially new ideas to the word pneu-

monia or peripneumonia. And, therefore, in estimating the opinion of our predecessors on any point connected with this disease, if we use that word in its new sense, when it should be used in the old, we fall inadvertently into the well-known sophism which substitutes one idea for another by changing the import of an essential term in any proposition. And hence, whoever should say that our predecessors, being ignorant that peripneumonia often subsides under mere regimen and nominal treatment, used remedies of too active a character, would erroneously assert that to be true of the old peripneumonia which is true only of the new. And if it be the case, as many considerations seem to prove, that highly active inflammations of the internal organs have been much less frequent of late years than at some former periods, then this reasoner would be guilty of at once extending and contracting the signification of the original term peripneumonia, by using it at first to include stethoscopic peripneumonia, which had never been in former times included under it, and then to exclude thoracic inflammation with very dangerous constitutional reaction, which it had been originally employed to denote.

And the next step is the erroneous and dangerous inference, because stethoscopic peripneumonia, which has been known but for a few years, yields to regimen and simple treatment; that, therefore, peripneumonia, in general, may be at all times safely allowed to run on without active treatment.

Like the microscope, chemical analysis has much enriched, and, in the highest degree, promises still further to enrich, the science of medicine. The additions supplied by chemistry come unquestionably under the denomination of facts. They are facts not directly observed, but determined by inference from a succession of phenomena. Such truths, therefore, are liable, particularly on their first announcement, to an admixture of error proportioned to the complexity of the processes by which they have been deduced. This is not a reason for rejecting their aid in medicine, but merely one for accepting it with due precaution.

But what we have most to fear from the over-ardent application of chemistry to medicine, are the errors resulting from the spirit of system. The humoral pathology owed its prolonged existence and repute among the physicians of its later days to the hope of rendering chemistry available for the purposes of pathology. Chemistry was then, however, utterly inadequate to the task. Even now, organic chemistry is far from being sufficiently advanced to have any just pretensions to supply a system of chemical pathology and treatment. It is easy

to predict that it must continue for many long years in a humble state of subordination to the prevailing empirico-rational medicine. A sober thinker can hardly venture to look forward to such an advanced state of chemical rationalism as would be sufficient for pronouncing *a priori* that sulphur could cure scabies, iodine goitre, citric acid scurvy, or carbonate of iron neuralgia. Chemistry might perhaps have boasted at the present moment of having discovered iron to be a remedy in chlorosis, had not empirical medicine anticipated that discovery by a long lapse of years. This science has afforded antidotes to poisons, and has shown alkalies to be the proper remedies for the uric acid diathesis, and certain acids for the phosphatic diathesis. But such additions to medicine are not the fruit of organic chemistry, but merely obvious applications of facts in inorganic chemistry. To chemistry—perhaps I should say to organic chemistry—we are indebted for the characteristic symptoms of granular disease of the kidney, the coagulability of the urine, its low specific gravity, the defect of urea; but chemistry does not supply the remedies for this disease, these being drawn exclusively from the principles of the empirico-rational medicine of the day. That colchicum augments the proportion of urea in the urine, is a fact we owe to chemistry; and it will be recollected that Dr Douglas Maclagan read to the Society, the summer before last, an interesting case of urticaria in one of Dr Scott's patients, accompanied by a deficiency of urea in the urine, which being augmented by the administration of colchicum, the disease subsided. This is the kind of field in which chemistry promises to be of immediate service in the practice of medicine,—that is, not as offering us a rational chemical pathology, but as enlarging the sources from which empirical rules are to be drawn. Here, though the fact on which it rests be supplied by chemistry, the treatment is empirical, not rational, the immediate object being simply to increase the proportion of urea in the urine. Nor is there any hope that in our times the means will be afforded of determining *a priori* such truths as that colchicum augments the proportion of urea in the urine.

We may, indeed, already look forward at no very distant time to the establishment of partial chemical rules for the preservation of health, and for the exact chemical treatment of many diseases, particularly of those in which the urinary function participates. But we shall be deceived, if we expect that chemistry will soon teach principles, which can be acted on independently of what mere experience has taught in past ages in reference to the general management of health, and the treatment of prevailing diseases. For example, the proteine theory, so beautifully applicable in appearance to all the circumstances

of alimentation, is still a much less perfect rule for the preservation of health than the common wayward experience of mankind. It may truly teach that the vegetable kingdom manufactures, from the inorganic matter of the earth's surface, a primary organic substance which in its essential composition represents the blood and flesh of animals, which substance in some of its forms must be supplied to animals in quantity proportioned to the daily waste of their bodies, otherwise they perish. But there are manifestly other conditions necessary to healthful alimentation besides a due supply of proteine; and till these conditions be completely brought out, science must be content to bow to vulgar experience. And the case of the proteine theory exactly represents too many of the cases in which the over-sanguine partizans of the new pathology already proclaim the triumph of science over the old empirico-rationalism. The proteine theory does not confine itself to health. It already aims at expounding the theory of diseases. But we are much deceived, if any better rules for the treatment of fever and inflammation will be discovered in our time, from the knowledge that the state of fever augments the quantity of oxidized proteine in the system, that the state of inflammation is one of the consequences of this augmentation, and that blood-letting is serviceable by evacuating this peccant matter. Here there is example enough of the rash application of chemistry to pathology.*

That the progress of organic chemistry is destined to clear up many difficulties in pathology, it would be absurd to dispute. But it is too soon to speak of this science giving a decided turn to the theory and practice of medicine. The chemistry of the living body is a chemistry altogether *sui generis*;—its laws and principles can resemble nothing that is known out of organic nature. These, then, must be sought by long and laborious efforts in the living body itself; and in seeking for these, the established principles of the earlier chemistry will oftener mislead than assist, or there can hardly be too little trust put in the suggestions of inorganic chemistry. Pathology cannot profit much by this higher grade of inorganic chemistry till physiology by the same means has made long strides beyond its present state. And no spirit of prophecy is required to pronounce, that any attempt at present to found practice largely on a chemical rationalism will only issue in error and disappointment.

The next subject for our consideration is the search after new medical facts by the numerical method, or by statistical inquiries. The evidence of statistics seems, at a first inspection,

* See Simon's Chemistry, by Day, Vol. i. p. 12.

so very simple and certain as to defy error. And in many cases, with no other care than some provision against positive blunders, it is unerring. In a given place, within a given time, any one may reckon up the number of births and the number of deaths, and, migration on either side being allowed for, may thus deduce the rate at which the population is increasing or decreasing. Again, it is easy to count up the male births and the female births in a place within a certain time, and thus to test the proportion alleged to exist between these in civilized countries. And in both these inquiries, mere blunders apart, perfect certainty is attainable. And this certainty is here attainable simply because it is impossible for any one to misunderstand, confound, or misapply the terms "birth," "death," "male," "female." But let the inquiry turn on the number of cases of pneumonia, phthisis, croup, asthma, pericarditis, peritonitis, gastro-enteritis, or any like diseases which have occurred in a given time under the care of several different practitioners, then the result will approximate to the truth only in proportion to the agreement between them as to the nomenclature which they have adopted, their general skill in diagnosis, the pains bestowed by each, and the opportunities afforded for overcoming the uncertainties of medical opinion. And if the inquiry be conducted on such a plan as that on which the ancient London bills of mortality were made up, when the old-women searchers in each parish determined the name of the disease which had caused death from the description given of its symptoms by the relatives of the deceased, then it will be problematical whether truth or error be predominant in the result.

It may indeed, at first sight, appear, that all the received aphorisms and precepts in the history and treatment of diseases, usually described as the result of past medical experience, are founded on a species of statistics differing from the statistics of modern times only in being of a looser character. But if we consider the origin of aphorisms and practical rules in medicine, we shall find them to rest on a foundation somewhat different from that of mere statistics. They owe their value to the sanction of authority. It is not every one who practises medicine, that is competent to make observations fit to be generalized into maxims and precepts for the use of succeeding ages. The most valuable of these we owe to the experience of the Magnates of the profession. In ordinary statistics every record is of equal weight, by whomsoever made and howsoever erroneous; and the several records which make up the aggregate being usually taken by a number of persons who cannot be placed so much under the same exact circumstances at each observation as the single authoritative observer in the individual observations from which

he generalizes his precepts, there is much wider scope for errors of misapprehension and misapplication, both as to names and things, in the former case than in the latter.

The great bulk of practical medical knowledge is obviously the fruit of individual minds naturally gifted for excellence in medicine. And the axioms and precepts so originating, when made known to the medical world, having been found to be in accordance with the experience of others, have acquired the additional sanction of time and numbers. There can be no doubt that a great proportion of the medical knowledge which comes under this head is true, and as little doubt that some part of it is erroneous. And the grand obstacle in the way of improvement is the difficulty of separating the chaff from the grain. It was erroneous knowledge of this kind that Cullen complained of when he said that "false facts" are more common than false theories. The same would form a principal part of the material for that revision of medical opinions which Cabanis proposed as the first step towards a reform in medical science. Such a revision, however, could not be made in one age; it cannot be accomplished otherwise than by the slow growth of medical improvement; and any attempt at an immediate revisal of the kind suggested by Cabanis, though it might make extensive changes on the face of medical science, would probably but substitute new forms of error for the old errors which it threw aside.

At all events, the evidence of statistics can in no case be deemed adequate to overturn the long-received maxims and precepts of practical medicine, unless when the basis of observation is unimpeachable, and the ideas and terms concerned are so simple as to be incapable of misapprehension.

And when any attempt is made to render statistics available for the solution of complex questions in medical science, no pains must be spared for the thorough understanding of the numerous sources of fallacy incident to this kind of inquiry; and though the utmost vigilance be exerted to prevent their intrusion, it will not uniformly be successful.

Sometimes the blunders that may be committed in the mere mechanical part of the inquiry are of a kind so obscure as to pass for a time unobserved. Of this description was the blunder formerly made in the estimate of the mortality in prisons, namely, dividing the number of committals by the deaths. The result thus obtained is so favourable to the healthiness of prisons, as to have led a French minister to pronounce prisons the healthiest places in the world, and an English inspector gravely to affirm that "in very few situations of life is an adult less likely to die than in a well-conducted English prison." Only one in 500

prisoners dies; "so, according to this view," says Mr Farr, "if a man desire to live to the age of Methuselah, he should go to Newgate." The inmates of a prison, however, are, on an average, renewed nearly eight times in the year. One in 500, therefore, represents the mortality not for 365 days, but only for something between 40 and 50 days, or the eighth part of a year. But if one in 500 be the mortality for the eighth part of a year, it represents a mortality per annum among the persons confined of 8 on 500, or 16 on the 1000,—which is more than one-half greater than the mortality of the population of England in general at the average age of prisoners.

We have had recently, in the Edinburgh newspapers, an example of a very common kind of fallacy in statistics, referring to the mortality of the present epidemic in the Infirmary. The facts mentioned are perhaps correctly stated, but as professing to illustrate the mortality of the epidemic, they are wholly deceptive. The objectionable part of the paragraph is as follows: "The number of paupers which the city parochial board alone has sent to the Infirmary since the first of June,—when the fever manifested itself as virulent and epidemic,—till Monday last (the 27th September), is 887, of which no less than 485 have died."* Here, by a common fallacy of statistics,—the taking a special part of the cases concerned to represent the whole,—the mortality of the present epidemic is made to appear to have been at that time between 50 and 60 per cent., while the most exact returns prove that the mortality on the whole, up to that time, had not exceeded 10 per cent. The great mortality on the cases sent in by the parochial board must have arisen from the miserable state in which the patients had lain, in lodging-houses and the like, during the first days of the attack. Many of them died a few hours after they were brought in, and some, on being carried to the wards, were found to be actually dead.†

Such circumstances as those just stated show how readily mistakes may be committed in comparing, by statistical returns, the mortality of one epidemic with that of another, or the mor-

* Caledonian Mercury, Thursday, 30th September 1847.

† The above statement has been allowed to remain as it stood at the time the paper was read; but in the discussion which followed, Dr Myrtle, one of the city parochial surgeons, expressed his belief that the newspaper paragraph was wholly erroneous, and that the number, 485, related not to the deaths from fever among the parochial poor in the Infirmary, but to the supply of coffins to all who had died within the parochial bounds of the city in a destitute state, in the period referred to. This turned out to be the truth; and then, at the end of four months, a contradiction was made in the newspapers, but not before this apparently official testimony to the great mortality of fever in Edinburgh had been copied into the continental journals, probably to make the round of Europe, with small chance of being followed by the tardy contradiction. Such are too often the precarious foundations of so-called statistical facts. The actual mortality of fever in the Royal Infirmary for the whole year 1847, was nearly 13 per cent.

tality in one hospital with that in another, either as regards epidemic diseases, or the general mortality medical or surgical. In typhus, the disease in most seasons, as every one must have observed, falls much less severely on females and the young of both sexes, than on male adults, and is especially fatal to the old of either sex; and thus, when, in the general results, numbers alone are regarded, erroneous inferences are apt to be drawn from statistical returns as to very important points,—such as the effect of the treatment, the care bestowed by the attendants, the healthiness of the wards, and the like.

On the inferences apt to be drawn from the statistics of surgical operations, Dr Fenwick of Newcastle has made a very important observation. Speaking of the comparative success of different operations in amputation, he says—"I have seldom found the apparent success of any of our most celebrated surgeons commensurate with his reputation, but, on the contrary, have often observed those who have enjoyed the greatest name, number the largest amount of deaths amongst their patients."* And this he illustrates in the following manner:—Two surgeons have each six patients affected with gangrene; one amputates on three and recovers two of these, while the three left to nature die; the other amputates on all the six and recovers three,—thus he saves one more than the other, though as respects his success in amputation, he loses one-half, while the other saves two out of three. The application of this observation to the statistics of surgical practice is too obvious to require comment.

A volume would not contain all the kinds of fallacy to which the rash application of statistics to complex cases gives rise. But as these can hardly be pointed out otherwise than by examples, I have one or two additional illustrations to offer.

There is a well-known proposition ascribed to Hippocrates, namely, that the age of pulmonary consumption is between 15 and 40 years of age. Out of this arises a question of importance to life insurance societies, namely, whether the risk of insuring the life of a person belonging to a phthisical family be much diminished after forty, as compared with the earlier part of his life. The general belief at present doubtless is, that the risk is little if at all diminished; and this belief is supposed to rest on statistics. It is not unlikely that this belief is well founded, yet the statistics relied on hardly touch the question. It is proved sufficiently by statistics, that, in the great hospitals of Europe, tubercular phthisis occurs at all ages. But the patients in these hospitals do not belong to the class of persons who insure their lives. And surely it is not impossible that

* Monthly Journal of Medical Science, October 1847.

phthisis might occur principally between 15 and 40 in the class of persons who live comfortably, notwithstanding that it has been found to prevail almost equally throughout life among the inmates of the hospitals in the great cities of Europe. For it is to be remembered that the inmates of these hospitals are to a great extent made up of an ill-fed, half-naked, houseless population, as well as of the many artisans victimised in such cities by employments in themselves unhealthy, or by being crowded together in ill-aired, over-heated work-shops. These conditions of life cannot but have a very material influence on the frequency of a disease which owes its origin so often to a cachectic state, induced by a disregard of the precautions necessary for the preservation of health. Before, then, the proposition of Hippocrates can be justly set aside, which applied, unquestionably, to persons living somewhat comfortably, like those who insure their lives, a new statistical inquiry would be requisite, limited to the class of persons concerned in the proposition. Though sufficient data do not at present exist to decide this point, Sir James Clark regards the proposition of Hippocrates as so far warranted even by modern statistics, that by far the largest proportion of phthisical patients are cut off between 15 and 40, though it must be confessed, that, according to the statistics on which he relies, a considerably greater proportion die of phthisis from 40 to extreme old age than between 15 and 40. On the contrary, however, according to the new London tables of mortality, the deaths ascribed to phthisis are between four and five times more numerous between 15 and 40 than above 40.

The examples hitherto produced have related to the use of statistics for the establishment of facts, or supposed facts of a positive character. But we find statistics sometimes resorted to for a use little short of the difficult task of proving a negative. When supposed facts are put forward on insufficient grounds, mere logic has no resource but simply to affirm that these should not be received because the proof has failed. But, unfortunately, in medicine, there always have been many things, which, though doubtful, having been at one time received and acted on as true, require a particular refutation before they can be wholly got rid of; and though such a refutation falls, as I have just said, little short of proving a negative, it has been attempted by statistics. It is manifest that such a proceeding is very likely to run into error. It is, however, on evidence of this kind that many persuasions of past times are now rejected; —witness the belief that marks and monstrosities in the infant at birth depend on affections of the mother's imagination during pregnancy. The refutation here, though far from having been

carried out in a systematic manner, is justly held to be sufficient. But this is not the case in every instance. For example, statistics have been applied with much confidence, though hardly with due consideration, to the all-important question as to the period of gestation essential to the production of a viable child. On the faith of statistics, sometimes limited to the cases presented in the practice of one accoucheur, all the alleged instances of viability before the end of the seventh calendar month have often been authoritatively set aside. To assume *a priori* that, if exceptions occur to the observed rules, they must be met with out of a certain number of deliveries, be that number 1000, 5000, 10,000, or even 1,000,000, is an unwarrantable mode of reasoning. It should at least be shown that the exceptions to the ordinary laws of development cannot have place unless within the limits set forth. Few data exist for the explanation of the extreme rarity of some deviations from the ordinary development of the animal body. But if an exception be as it were grafted on an exception, and neither be of a kind to occur frequently, then the so grafted exception can be understood to be of extreme rarity. It is admitted, I suppose, that an infant not destined to reach maturity before the end of nine calendar months may be born accidentally at the end of seven calendar months and survive to adult age. But if it be a fact, as many think, that infants sometimes reach maturity considerably before the completion of the ninth month, then it is reasonable to suppose that an infant, thus prematurely developed, may be capable of living, though brought into the world accidentally, some time before the completion of seven calendar months. Thus, if it were the case, as maintained by Levret and others, that infants occasionally reach maturity of development as early as the end of the seventh month, then it would be possible that some of those precocious children might come into the world in a viable state at a date antecedent to nine calendar months, bearing a proportion to the difference between nine and seven; or, at so early a period as the 170th day—that is, ten days before the time fixed as the earliest by the Code Napoleon.

If to the difficulty here stated be added the additional difficulty that the term viability, so much used of late in inquiries of this sort, is manifestly far from being strictly defined, we shall be able to judge how much the question as to the minimum period of gestation of a viable infant is from being ripe for the application of statistics.

And it will occur to every one, that many questions stand in the same predicament with this.

In statistical inquiries, then, it should be an axiom, that the

inferences cannot be more exact than the facts founded on. If the individual facts concerned be absolutely alike, as birth to birth and death to death, then the truth of the inference will be equally absolute. But there may be, as we have seen, among the individual facts, only an apparent resemblance, while a latent contrariety exists; they may be called by the same name, though they be far from being categorically alike, and thence the inferences become proportionately erroneous.

As to facts deduced from previously-established general principles or laws, the last topic to which I proposed to advert, a very few words will suffice. In those sciences, the objects of which are less complex than those of medicine, many facts are deducible in this manner with an unerring certainty. But, for obvious reasons, such inferences in medicine can seldom be relied on as facts without the support of direct evidence. The chief use, then, of inferences from general principles in medicine, is as suggestions for new inquiries. And while the most serious errors are the uniform result of regarding, as available truths, inferences of this kind without sufficient examination, nothing is better ascertained than that, under skilful management, there are no more effectual means of advancing the improvement of medical science.

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