On the Theory of Probabilities

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highest term of X, and that from this and the other divided differences we may find the coefficients in the other terms of X, as is actually shown in the example in PROP. I.

Newton then adds simply, *From these Propositions the following may easily be obtained.* ("Ex his Propositionibus quæ sequuntur facile colligi possunt"), and this is perfectly true.

In the two next Propositions are given the formulæ for Interpolation by *central* divided differences, in PROP. III. for equidistant arguments with the constant difference 1, in PROP. IV. for arguments not equidistant. In both Propositions, the two cases of an odd and of an even number of given values, are distinguished.

Then it is shown that the preceding may be applied to *Approximate Interpolation* (PROP. V.) and *Integration* (PROP. VI.) of any function of which a number of values are known.

After these six Propositions comes a SCHOLIUM, in which Newton—after pointing out how useful the preceding theory is in the calculation of tables and in solving problems depending on integrations—concludes his important little tract with this theorem: "Through any number of points may be drawn not only a parabolic "curve, but also an infinite number of other curves," or in modern analytical language: "The condition, that to any given number of "arguments (a, b, c . . . .) correspond as many given values of an "unknown function (A, B, C . . . .), may be satisfied not only by an "integral and rational algebraical function, but also by an infinite "number of other functions." This is actually proved in a very ingenious manner, without making use of periodic functions.

On the Theory of Probabilities. By SIR JOHN F. W. HERSCHEL, BART., K.H., M.A., D.C.L., *&c. &c. Being extracts from a review of "Quetelet on Probabilities," which appeared in the "Edinburgh Review" for July,* 1850.*

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THE theory of Probabilities has been characterized by Laplace, one of those who have contributed most largely to its advance,—as "good sense reduced to a system of calculation;" and such, no

* This essay contains so much that is of permanent interest to all students of the Theory of Probabilities that we believe we are doing our readers a real service in reproducing the greater part of it. We would gladly have reprinted the whole, but for the limited amount of space at our disposal; but those who may wish to read the essay in its entirety, will find it in the collected volume of Sir John Herschel's essays published by Messrs. Longmans in 1857.—ED. *J. I. A.*

doubt, it is. But it must be especially noticed that there is hardly any subject to which thought can be applied, which calls for so continuous an application of that excellent quality, or in which it is easier to make mistakes from simple want of circumspection. And, moreover, that its reduction to calculation is attended with difficulties of a very peculiar nature, such as occur in no other application of mathematical analysis to practical subjects, arising out of the great magnitudes of the numbers concerned, which defeat the ordinary processes of arithmetical and logarithmic calculation, by exhausting the patience of the computer, and require special methods of *approximate* evaluation to bring them within the compass of human industry. These methods form a conspicuous feature of the general subject, and have furnished scope for very extraordinary displays of mathematical talent and invention. That very large numbers will inevitably be concerned in questions where numerous and independent contingencies may take place, and in any order or mode of combination, will be apparent to any one who considers the astonishing *fecundity* of such combinations numerically estimated, when the combining elements are many. For example, the number of possible "hands" at whist (regard being had to the trump) is 1,270,027,119,200.

The calculus of Probabilities, under the less creditable name of the doctrine of Chances, originated at the gaming table; and was for a long time confined to estimating the chances of success and failure in throws of dice, combinations of cards, and drawings of lotteries. It has since effectually obliterated the stain of its cradle, as there is no monitor more severe, no lecture which can be delivered on the certain ruin which attends habitual gambling more emphatic than may be found in its demonstrations. Questions of this kind, it is true, are still retained in treatises on the subject; nor indeed can they be conveniently dispensed with, since they furnish the simplest and readiest illustrations of the combination of independent events, and the superposition of contingencies arising out of them, which belong essentially to its principles. They, however, form a very insignificant part of its applications, in comparison with the problems which its scope at present takes in, and which its modern developments have enabled it to handle.

Its first advances towards the dignity of a distinct branch of Mathematics are attributable to the celebrated Blaise Pascal, and his no less celebrated contemporary and correspondent Fermat,—

both reasoners of extraordinary acuteness, and who seem to have been specially attracted (like many of their followers) by the close reasoning and careful analysis its problems demand for their successful issue. Subsequent to these, but still among its earlier contributors, we find the distinguished names of Huyghens (to whom we owe the first treatise on the subject), those of the Grand Pensionary De Witt, Hudde, and Halley (with whom originated its application to the probabilities of life and the construction of tables of mortality), and that of James Bernouilli, who may be considered the first philosophical writer on the subject. To him we owe the demonstration of two great fundamental theorems or laws of Probability, as applied to the results of very numerous trials of any proposed species of contingency: viz., 1st, that in any vast number of trials there is a demonstrably greater probability that the events will happen in numbers proportional to *their respective chances in a single trial*, than in any other *specified* proportion; and, 2dly, that a number of trials may always be assigned so great, as to make the probability of the events happening in numbers falling within any assigned limits of deviation from that proportion, however narrow, approach to certainty as nearly as we please. The first of these propositions has the air of a truism, when the meaning of its terms is not nicely weighed. But the second is obviously of paramount importance; since it goes to take the totality of results obtained in any sufficiently extensive series of trials, almost out of the domain of chance, and to place in evidence the influence of any "cause" or circumstantial condition common to the whole series, which may give even a trifling preponderance of facility to any one of the classes of events contemplated over the rest.

Common sense, it may perhaps be said, would tell us as much as this. No doubt it might suggest some such propositions as likely enough to be true; and the usual course of inductive reasoning up to causes tacitly assumes their truth. But when we come to demand what number of trials may reasonably be expected to bring out into prominence a very small given preponderance of facility? or to declare within what limits of accuracy such preponderance may reasonably be expected to be represented on the upshot or final average of a given number of trials?—or, lastly, what is the probability that on a given number of trials such an average will represent the preponderant facility in question within given limits of exactness? all of them, and especially the last, evidently practical questions of much interest; we find ourselves

forced to appeal from the unaided judgment of simple good sense, to strict numerical calculation,—taking for its basis not a mere *aperçu* but a rigorous demonstration of the truth of the propositions above stated. This is very much the case with all the more important conclusions of this theory; when generally enunciated, they are almost universally seen to be pretty plainly conformable to ordinary clear-judging apprehension of their relations. Even the apparently paradoxical conclusions by which we are occasionally startled, lose that aspect when their exact wording is duly attended to, and all the conditions implied in it clearly apprehended. It is their applicability to exact computation, and the handle they afford thereby for precise determinations useful in practice, which give them all their value.

Problems of the class above mentioned were first successfully treated by De Moivre, to whom also we owe the happy idea of applying Stirling's theorem to approximate to the ratio of the high numbers which enter into such calculations, without which they would be impracticable. From these it would appear but a small step to pass to what may be deemed the *inverse calculus* of Probabilities, which applies the knowledge gained by the observation of past events to the prediction of future, by concluding from the succession of facts observed the respective degrees of probability of the existence of each out of several equipossible determining conditions, and thence starting as it were anew, and ascertaining from the knowledge thus acquired the probability of an event or events similarly determined *in futuro*. It was reserved, however, for another member of the gifted family of Bernouilli to make this step, which has in some respects changed the whole aspect of the subject, and given to it that degree of importance it possesses as an auxiliary of the inductive philosophy.

It may perhaps be doubted whether subsequent writers have added very materially to the intrinsic philosophy of the subject, though there can be no hesitation as to the value of the improvements they have made in its methods of procedure, whether in point of elegance or power; the extension given to its formulæ; or the numerous and important applications made of its principles, especially in those cases (which comprise almost all the really interesting ones) where the transition has to be made from the finite to the infinite, from the limited though often large number of possible combinations which its simple and more elementary problems offer, to the *literally infinite* multitude which the gradation of natural causes and influences obliges us to consider,

and which calls for the perpetual employment of the most refined theories, and the most delicate and abstruse applications of the integral calculus. In all these respects the great work of Laplace ("Théorie Analytique des Probabilités") stands deservedly pre-eminent; occupying in this department of science the same rank and position which the "Mécanique Analytique" of his illustrious rival Lagrange holds in that of force and motion, and marking (we had almost said) the *ne plus ultra* of mathematical skill and power. So completely has this sublime work been held to embody the subject in its utmost extent, and to satisfy every want of the theorist, that an interval of a quarter of a century elapsed from the date of its appearance (1812) before any further original contribution of moment was made to the theory. The valuable memoir of Poisson, published in 1837, on the probability of judicial decisions* (which contains a *résumé* of the whole theory of Probabilities), though admirable for its clear exposition of principles and elegant analysis, can hardly be said to have carried the general subject much beyond the point where Laplace left it.

It may easily be imagined that a work like this of Laplace, followed at a short interval by an admirable *exposé* of its contents by himself ("Essai Philosophique sur les Prob."), could not fail to make a lively impression and to excite general attention. Laplace possessed in an eminent degree the talent of stating the most profound results of his own geometry in a style at once philosophical, luminous, and pleasing. Few works have been more extensively read or more generally appretiated than this Essay and that on the "Système du Monde" by the same author. There is in both a breadth and simple dignity corresponding to the greatness of the subjects treated of, a loftiness of style, the direct result of generality of conception, and which is felt as adding to rather than detracting from clearness of statement, and a masterly treatment which fascinates the attention of every reader. Nowhere can be found so great a body of important discoveries, so consecutively linked together, and so distinctly and impressively announced. It is not, perhaps, too much to say, that were all the literature of Europe, these two Essays excepted, to perish, they would suffice to convey to the latest posterity an impression of the intellectual greatness of the age which could produce them, surpassing that afforded by all the monuments antiquity has left us.

* Recherches sur la Probabilité des Jugemens en Matière Criminelle et en Matière Civile; précédées des Règles Générales du Calcul des Probabilités. Paris, 1837.

Previous to the publication of the "Essai Philosophique," few except professed mathematicians, or persons conversant with insurances and similar commercial risks, possessed any knowledge of the principles of this calculus, or troubled themselves about its conclusions,—regarding them as merely curious, and perhaps not altogether harmless speculations. Thenceforward, however, apathy was speedily exchanged for a lively and increasing desire to know something of a system of reasoning which for the first time seemed to afford a handle for some kind of exact inquiry into matters no one had ever expected to see reduced to calculation and bearing on the most important concerns of life. Men began to hear with surprise, not unmingled with some vague hope of ultimate benefit, that not only births, deaths, and marriages, but the decisions of tribunals, the results of popular elections, the influence of punishments in checking crime—the comparative value of medical remedies, and different modes of treatment of diseases—the probable limits of error in numerical results in every department of physical inquiry —the detection of causes physical, social, and moral,—nay, even the weight of evidence, and the validity of logical argument—might come to be surveyed with that lynx-eyed scrutiny of a dispassionate analysis, which, if not at once leading to the discovery of positive truth, would at least secure the detection and proscription of many mischievous and besetting fallacies. Hence a demand for elementary treatises and popular exposition of principles, which has been liberally answered.

Among the valuable works of this kind in the French and English languages which have appeared since the epoch in question, we may notice more especially Lacroix's "Traité Élémentaire du Calcul des Probabilités; Paris, 1822," and the several encyclopædic essays and articles on the subject by Sir John Lubbock and Mr. Drinkwater (Bethune), in the Library of Useful Knowledge, by Mr. Galloway in the Encyclopædia Britannica (since published separately in a small and compendious form—a work of great merit and utility), and by Mr. De Morgan in the Encyclopædia Metropolitana. To the last-mentioned treatise, as well as to two admirable chapters on the subject in the recent elaborate work by the same author on the Formal Logic, we may refer as containing, *par excellence*, the clearest views of the *métaphysique* of the subject, and the most satisfactory analysis of the state of the mind as to belief or disbelief, and the degree of assurance afforded by the conclusions of the calculus in cases where the data themselves are vague and uncertain, which can any

where be found. All or any of these works will afford the English student a perfect insight into the mathematical treatment and reasonings of the subject, and consequently serve as an abundant preparation for the study and mastery of Laplace's great work; but we would caution all who desire to enter upon the more general and intricate parts of the theory, never for an instant to lose sight of special examples and numerical particulars, since nothing can exceed the bewilderment of ideas experienced by the tyro in this department of mathematics, who trusts himself *with both feet off the ground* to the whirl of symbols and notations in which those who are accustomed to ride these storms know how to guide their course, and even seem to feel a wild and fierce delight in the turmoil.

There is, however, a very large portion of those who desire to know something of the results at which thinking men have arrived in this as in all other departments of knowledge, to whom a book full of mere algebraic formulæ and calculations must remain for ever sealed. These are not necessarily or generally persons of despicable acquirements or intellect; nor is this their curiosity to be slighted as devoid of a reasonable object or motive. They desire to understand with a view to apply. Mathematicians, in common with men of high science in all departments, have long since begun to perceive that they have to address a mixed audience of a highly important and respectable character—an audience by no means disposed to treat them with derision or distrust, but, on the contrary, to regard them as their fitting instructors in matters within the scope of their legitimate pretensions, if only they will condescend to make themselves intelligible. Learned jargon such an audience will not endure. Charlatanerie of every description it can detect and chastise. Common-sense statement driven home by pointed illustration, and an earnest endeavour to inform, are what it eagerly desires, and in such a spirit is assuredly entitled to receive at the hands of those able to afford it.

The work now before us is conceived on these principles, and on this view of the duty devolving on those who have advanced beyond the ordinary limits of knowledge, to pause occasionally in their onward career, and inform the world, in plain terms and without exaggeration, whither they have got, and what they see beyond, which may make it worth while either for themselves to continue in the track, or for others to follow in it; as well as to render easy and intelligible to all whom it may concern the practical application of the information acquired. Its author is a teacher

well worth listening to, and may claim attention on the excellent ground that he has himself approached his subject in a practical manner, through a long and severe apprenticeship to the actual collection of data in a great variety of departments, and to the deduction from them of definite results of unmistakeable value and import, by the rules and principles he professes to teach.

* * * * *

A comparatively small portion of the work, the first and least extensive only of four divisions into which it is broken, and an appendix in the form of notes containing tables and formulæ, are devoted to the theory of Probabilities in the abstract, and to the illustration of its fundamental axioms and propositions; all which have been so repeatedly and so well laid down and elucidated in the various treatises we already possess, that it is hardly possible to place them in any very new and more than usually striking light. The distinction between mathematical and moral expectation belongs to this part of the subject, and can hardly be put more pointedly than it was originally done by Buffon, who first called attention to it.

"If two men were to determine to play for their whole property [supposed equal, and with equal risks], what would be the effect of the agreement? The one would only double his fortune, and the other would reduce his to nought. What proportion is there between the loss and the gain? The same that there is between all and nothing. The gain of the one is but a moderate sum; the loss of the other is numerically infinite, and morally so great that the labour of his whole life may not suffice to restore his property."

It was on such considerations that Daniel Bernouilli was led to propose, as a rule for estimating the value of a very small pecuniary or other material advantage, its *relative* value as compared with the total fortune of the party benefited, and for the moral as distinguished from the mathematical expectation of such advantage, that relative value multiplied by the probability of its accruing. On this or some equivalent mode of estimation is founded the principle of the subdivision of risks, which, rightly understood, so as to preserve their absolute independence while multiplying their number, is the best guarantee of commercial security. It is by such subdivision carried to an extreme point,* that insurance and annuity offices thrive, and that benefit societies might do so, were it not for the single great risk which the dishonesty of entrusted agents throws in their way as a fearful stumbling-block.

* This phrase appears to us rather too strong. While the same Office will grant insurances for £100 and for £10,000 at its own risk, it can scarcely be said that the subdivision of risks is carried to an extreme point.—Ed. *J. I. A.*

In the case of savings' banks, this is, in fact, the only risk; and, as experience has too recently* and abundantly shown, a most imminent and fatal one. To annihilate this risk by a perpetual and searching superintendence, carried even to the utmost stretch of suspicious vigilance, obnoxious as it may appear, is the paramount duty of all who connect themselves with them as managers or trustees. Of the general benefit of such institutions, which, by guaranteeing the security of the produce of successful exertions, tend to cherish habits of industry, prudence, and frugality, no one can entertain a doubt. It is in this point of view that a certain considerable amount of national indebtedness, so far from meriting denunciation as an evil, ought to be regarded as an indispensable element and engine of civilization. In its practical working it resolves itself into the establishment of a savings' bank on a vast scale, administered with what may be considered a perfect exemption from the consequences of dishonesty in its officials, and subject only to the inconvenience (no doubt a considerable one), of its deposits being withdrawable only at a market value,—but that market the fairest, readiest, and openest which can anywhere exist. Yet it is too commonly forgotten by those who deprecate taxation, while insisting on the objects for which taxation is instituted, and which alone it can secure, that the interest on savings' bank deposits is derivable only from that source, and that every depositor is as truly (and in some respects even more emphatically) a tax-holder—as the proprietor of consols.

To render the consequences of our actions certain and calculable as far as the conditions of humanity will allow, and narrow the domain of chance, as well in practice as in knowledge, is so thoroughly involved in the very conception of law and order as to make it a primary object in every attempt at the improvement of social arrangements. Extensive and unexpected fluctuation of every description, as it is opposed to the principle of divided and independent risks, so it also, by consequence, stands opposed to the most immediate objects of social institutions, and forms the element in which the violent and rapacious find their opportunities. Nothing, therefore, can be more contrary to sound legislative principle than to throw direct obstacles in the way of provident proceedings on the part of individuals (as, for instance, by the exorbitant taxation of insurances), or to encourage a spirit of general and reckless speculation, by riding unreservedly over established laws of property, for the avowed purpose of affording a

* 1850.

clear area for the development of such a spirit on a scale of vast and simultaneous action. The sobering influence of an upper legislative assembly, refusing its sanction to the measures demanded, or spreading it over time, can alone repress or moderate these epidemic outbreaks of human cupidity: and its mission is abandoned, and its functions *pro tanto* abdicated, if it retreat from the performance of this duty.

The first and most important application of the calculus of Probabilities (since it applies to all departments of science, and affords a measure of the degree of precision attained in all numerical determinations) is that which relates to means and limits, and forms the second division of M. Quetelet's work. A general idea of the sort of questions contemplated in this department of the theory, and the kind of relations they involve, may be conveyed by the following simple case. Suppose a man to throw stones at random, and without any aim. From the marks left by any given number of them, however great, on a wall, we could obtain no impression, or a fallacious one, of his intention. All that we could conclude from their evidence would be, that, if he aimed at anything, it was not a point in the surface of the wall, and that only stray missiles had struck it. But, suppose he had been practising with a rifle at a wafer on the wall; which being subsequently removed, we were required to indicate at once the situation it had occupied, and his skill as a marksman. It is obvious enough that, from the evidence of a great number of shot-marks, both might be determined, at least with a certain degree of approximation, and with a probability of error less in proportion to their number. The theory of Probabilities affords a ready and precise rule, applicable not only to this, but to far more intricate cases: it is this: that the most probable determination of one or more invariable elements from observation is that in which the sum of the squares of the individual errors or aberrations from exactness which the observations imply, shall be the least possible. In the case before us the "errors" are the distances of the shot-marks from the point where the centre of the wafer was fixed; to ascertain which we have, therefore, to resolve the geometrical problem (a very elementary one)—"to find a point such that the sum of the squares of its distances from a certain number of given points shall be a minimum,"—a problem which is, in effect, identical with that of finding their centre of gravity. As to the skill of the marksman, it may be estimated in two different ways:—1st, by ascertaining what is the probability that he will place a single shot within a given distance: this may be done by

counting the number of marks within that distance of the point ascertained as above, and dividing it by the total number: or, 2ndly, by ascertaining within what distance of the mark he would probably (*i.e.* more probably than the contrary, or with a probability exceeding one half) place it: this may be done by describing circles about the wafer's place (found as above) for a centre, and measuring the radius of that which just includes half the total number of marks. For it is obvious that, so far as the evidence before us goes, and judging only from the numbers of instances favourable or unfavourable, there is just as great a presumption that he will shoot within as without that circle; and, if it be ever so little enlarged, the scale will turn in his favour.

Suppose the rifle replaced by a telescope duly mounted; the wafer by a star on the concave surface of the heavens, always observed for a succession of days at the same sidereal time; the marks on the wall by the degrees, minutes, and seconds, read off on divided circles; and the marksman by an observer; and we have the case of all direct astronomical observation where the place of a heavenly body is the thing to be determined. Or we may substitute for the wall the floor of a lofty building or deep mine, and for the marksman an experimenter dropping, with all possible care, smooth and perfectly spherical leaden balls from a fixed point at the summit of the building or the mouth of the mine, with intent to determine, by the means of a great number of trials, the true point of incidence of a falling body,—a physical experiment of great interest. We might, if we pleased, instance more complicated cases, in which the elements to be determined are numerous and not *directly* given by observation, but with such we shall not trouble our readers: suffice it to say that the rule above stated, or, as it is technically called the "Principle of Least Squares," furnishes, in all cases, a system of geometrical relations characteristic of the *most probable* values of the magnitudes sought, and which, duly handled, suffice for their numerical determination.

This important principle was first promulgated, rather as a convenient and impartial mode of procedure than as a demonstrable theorem, by Legendre. Its demonstration was first attempted by Gauss,—but his proof is in fact no proof at all, since it takes for granted that in the case of a single element, variously determined by *any finite number of observations however small* the arithmetical mean is the most probable value,—a thing to be demonstrated, not assumed, not to mention other objections. Laplace has given a rigorous demonstration, resting on the comparison of equipossible

combinations, infinite in number. His analysis is, however, exceedingly complicated, and, although presented more neatly by Poisson, and in this work stripped by M. Quetelet of all superfluous difficulties and reduced to the most simple and elementary form we have yet seen, yet must of necessity be incomprehensible to all whose knowledge of the higher analysis has not perfectly familiarized them with those delicate considerations involved in the transition from finite differences to ordinary differentials. Perhaps, therefore, our non-mathematical readers will pardon us if we devote a single page to what appears to us a simple, general, and perfectly elementary proof of the principle in question, requiring no further acquaintance with the transcendental analysis than suffices for understanding the nature of logarithms.

We set out from three postulates. 1st, that the probability of a compound event, or of the concurrence of two or more independent simple events, is the product of the probabilities of its constituents considered singly; 2dly, that there exists a relation or numerical law of connexion (at present unknown) between the amount of error committed in any numerical determination and the probability of committing it, such that the greater the error the less its probability, according to some regular LAW of progression, *which must necessarily be general and apply alike to all cases, since the causes of error are supposed alike unknown in all;* and it is on this ignorance, and not upon any peculiarity in cases, that the idea of probability in the abstract is founded; 3dly, that the errors are equally probable if equal in numerical amount, whether in excess, or in defect of, or in any way beside the truth. This latter postulate necessitates our assuming the function of probability to be what is called in mathematical language *an even function,* or a function of the square of the error, so as to be alike for positive and negative values; and the postulate itself is nothing more than the expression of our state of *complete* ignorance of the causes of error, and their mode of action. To determine the form of this function, we will consider a case in which the relations of space are concerned.

Suppose a ball dropped from a given height, with the intention that it shall fall on a given mark. Fall as it may, its deviation from the mark is *error,* and the probability of that error is the unknown function of its square, *i.e.* of the sum of the squares of its deviations in any two rectangular directions. Now, the probability of any deviation depending solely on its magnitude, and not on its direction, it follows that the probability of each of these

rectangular deviations must be the same function of *its* square. And since the observed oblique deviation is equivalent to the two rectangular ones, supposed concurrent, and which are essentially independent of one another,* and is, therefore, a compound event of which they are the simple independent constituents, therefore its probability will be the product of their separate probabilities. Thus the form of our unknown function comes to be determined from this condition, viz., that the product of such functions of two independent elements is equal to the same function of their sum. But it is shown in every work on algebra that this property is the peculiar characteristic of, and belongs only to, the exponential or antilogarithmic function. This, then, is the function of the square of the error, which expresses the probability of committing that error. That probability decreases, therefore, in geometrical progression, as the square of the error increases in arithmetical. And hence it further follows, that the probability of successively committing any given system of errors on repetition of the trial, being, by postulate I., the product of their separate probabilities, must be expressed by the same exponential function of the sum of their squares however numerous, and is, therefore, a maximum when that sum is a minimum.

Probabilities become certainties when the number of trials is infinite, and approach to practical certainty when very numerous. Hence this remarkable conclusion, viz., that if an exceedingly large number of measures, weights, or other numerical determinations of any constant magnitude, be taken,—supposing no bias, or any cause of error acting preferably in one direction, to exist—not only will the number of small errors vastly exceed that of large ones,† but the results will be found to group themselves about the mean of the whole, always according to one invariable law of numbers (that just announced), and *that* the more precisely the greater the total number of determinations.

* That is, *the increase or diminution in one of which may take place without increasing or diminishing the other.* On this, the whole force of the proof turns. (H. 1857.)

† Sir Joshua Reynolds, in his celebrated Lectures to the Royal Academy, has laid it down as the fundamental principle of the pictorial art, that beauty of form and feature consists in their close approximation to the mean or average conformation of the human model. Were this the case, ugliness ought to be extremely rare, and the highest degrees of beauty those of the most ordinary occurrence, a conclusion contrary to all experience. (H. 1857.) Another consequence follows, viz., that in designing the original prototype of the human form and face, the designer *had not in view especially* the production of what men call beauty, but some one or more objects of greater importance to the well-being of the total organism. The *animus* of making the *beautiful* thing, in that sense, was absent. The *capability* of beauty having been secured in the plan of the organization, it seems as if it were intended that the perfection of personal beauty like the highest genius or the most exalted goodness should occur but rarely in our species.

Such being the case, and the law of distribution of errors over the whole range of possible error being known, it becomes practicable to assign the relative numbers of cases in which the errors will fall respectively within and beyond any proposed limit on the average of an infinite number of trials, and thence to assign, *á priori*, the probability of committing in any single future trial—not a given specific amount of error, but an error *not exceeding that limit*, provided only the probable error of a single trial be known; which, as we have seen, can always be ascertained on the evidence of foregone experience, if very extensive. Computations of this sort are rendered exceeding easy by a table, originally calculated by Kramp, with a widely different object, which is given in the notes to M. Quetelet's book, and more *in extenso*, with differences, at the end of Mr. Galloway's work above noticed.

What is yet more remarkable is, that the skill with which the trials are performed is absolutely of no importance so far as the *law* of distribution of the errors over their total range is concerned. An important consequence follows from this: viz., that rude and unskilful measurements of any kind, if accumulated in very great numbers, are competent to afford precise mean results. The only conditions are the continual *animus mensurandi*, the absence of bias, the correctness of the scale with which the measures are compared, and the assurance that we have the *entire range of error* at least in one direction within the record.

In a matter so abstract, and on which, at first sight, human reason would appear to have so little hold, it is assuredly satisfactory to find the same conclusion, and *that* one so positive and definite, reached by different roads and from different starting points. It is not easy to imagine a principle of demonstration having less in common than that given above with those of Laplace, Poisson, and Quetelet. Yet the conclusions are identical, and the verifications afforded by experience in all cases where the trials have been sufficiently numerous, and care taken to guard against bias, have been of the most unequivocal character.

Some of these verifications, adduced by M. Quetelet as instances of the practical application of his rules of calculation in the theory of means and limits, have an interest independent of their value as such. They form part of a series of researches in which he has engaged extensively on the normal condition, physical and moral, of the human species, and, *inter alia*, as regards its physical development, in respect of stature, weight, strength, &c. By the assemblage of data collected from the experience of others, as well

as his own, he has arrived at a variety of interesting conclusions as to the law of progressive increase and decay in all these respects, of the *typical* individual, of either sex, during the period of life, which are given at large in his work "Essai de Physique Sociale."* We shall offer no apology for placing one or two of these before our readers.

From the 13th volume of the "Edinburgh Medical Journal," M. Quetelet extracts a record of the measurement of the circumference of the chests of 5738 Scotch soldiers of different regiments. The measures are given in inches, and are grouped in order of magnitude, proceeding by differences of 1 inch, each group containing of course (we presume) all that differ by less than half an inch in excess or defect from its nominal value. The extreme groups are those of 33 and 48 inches, and the respective numbers in the several groups stand arranged as in the table below.† Supposing each measure exactly performed, these, therefore, may be taken as the results of nature's own measurements of her own model; and the question whether she recognizes such a model? is at once decided by inspection of the groups, in which the *animus mensurandi* is broadly apparent. It is equally so that such model would fall within the group of 40 inches. An exact calculation of the mean, allowing to each group a weight in proportion to the number it contains, assigns 39·830 inches as the circumference of the chest of this model.

Now this result, be it observed, is a *mean* as distinguished from an *average*. The distinction is one of much importance, and is very properly insisted on by M. Quetelet, who proposes to use the word mean only for the former, and to speak of the latter as the "arithmetical" mean. We prefer the term average, not only because both are truly arithmetical means, but because the term *average* carries already with it that vitiated and vulgar association which renders it less fit for exact and philosophical use. An average may exist of the most different objects, as of the heights of

* Sur l'Homme et sur la Développement de ses Facultés; ou Essai de Physique Sociale. Paris, Bachelier, 1835.

† Inches	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	Totals.
Groups as per Observations	3	18	81	185	420	749	1073	1079	934	658	370	92	50	21	4	1	5738
M. Quetelet	4	17	63	185	419	765	1054	1140	961	629	321	125	40	9	2	1	5738
Our calculation..	6	21	72	200	433	746	1024	1103	943	639	341	145	50	12	2	1	5738

houses in a town, or the sizes of books in a library. It may be convenient, to convey a general notion of the things averaged; but involves no conception of a natural and recognizable central magnitude, all differences from which ought to be regarded as deviations from a standard. The notion of a mean, on the other hand, does imply such a conception, standing distinguished from an average by this very feature, viz., the regular march of the groups, increasing to a maximum, and thence again diminishing.* An average gives us no assurance that the future will be like the past. A mean may be reckoned on with the most implicit confidence. All the philosophical value of statistical results depends on a due apprehension of this distinction, and acceptance of its consequences.

The recognition of a *mean*, as thus distinguished from a mere average, among a series of results so grouped in order, depends on the observance of a conformity between the law of progression in the magnitude of the groups, and the abstract law of probability above stated, from which every consideration has been excluded, but the reality of *some* central truth, and an intention of arriving at it, liable to be baffled by none but purely casual causes of error. And the test to be applied, in this and all similar cases, is this. Is it possible to assign such a mean value, and such a probable error as shall alone, by the simple application of the table of probabilities, reproduce the numbers under the several groups in order with no greater deviations than shall be fairly attributable to a want of observations numerous enough to bring out the truth? In the instance before us, the answer to this inquiry is contained in the results of calculation as compared with fact in the table above referred to. The mean we have used is 39·830 inches, and our probable error 1·381 inches. Those of M. Quetelet differ somewhat from these values, which accounts for the trifling discrepancy of the results.

The coincidence admits of being placed in even a more striking light. In the complete expression, by theory, of all the groups in a statement of this kind, three elements are involved—the mean value— the maximum group *having that mean for its centre*—and the probable error. And to determine these, it ought to suffice to have before us three terms of the series. Suppose then we take for our data the numbers corresponding to 35, 39, and 43 inches, viz., 81, 1073, and 370, given by observation. Then, by a com-

* Adopting this distinction, which appears to us not only scientifically correct, but practically convenient, it follows that we must speak of the "*average* duration of life at a given age," instead of the "*mean* duration"—the phrase introduced by Dr. Farr.—Ed. *J. I. A.*

putation of no great difficulty, there will result for the mean value, 39·834 inches, and for the probable error 1·413 inches, both agreeing almost precisely with those already stated. For the greatest possible group of an inch in amplitude the same calculation gives 1161, which is in obvious accord with observation. No doubt, then, can remain, as to the reality of a typical form, from which all deviations are to be regarded as irregularities. On this M. Quetelet observes,—

"I now ask if it would be exaggerating to make an even wager that a person little practised in measuring the human body would make a mistake of an inch in measuring a chest of more than 40 inches in circumference. Well! admitting this probable error, 5738 measurements made on the same individual, would certainly not group themselves with more regularity as to the order of magnitude than these 5738 measurements made on the Scotch soldiers; and if the two series were given us without their being particularly designated, we should be much embarrassed to state which series was taken from 5738 different soldiers, and which was obtained from one individual with less skill and ruder means of appretiation. (*Transl.* p. 92.)

This is assuredly an over-statement. So far from less skill being supposed in the measurements of the individual, the probable error of nature is nearly half as much more than that assumed here for the term of comparison (1 inch); and it is clearly beyond the bounds of any supposable negligence or rudeness of practice, to commit such errors as the extreme registered deviations (7 inches one way, and 9 the other), in a series of such measurements however multiplied, or even half those amounts.

We are thus led to the important and somewhat delicate question,—What we are to consider as reasonable limits, in such determinations—beyond which, if deviations from the central type be recorded, they are either to be referred to exaggeration, or regarded as monstrosities.

The answer to this question must evidently depend, first, on the "probable" deviation from the mean or typical value; secondly, on the number of cases experience has offered, or within which we agree to limit our range of speculation. It results from the tables above cited that 20,000 might be betted against 1, that an observed deviation, one way or other from the type, will not exceed sixfold its "probable" value; and therefore we shall have double that amount of chances against such a deviation in either direction separately. Among 40,000 individuals, therefore, we are entitled to expect to find one so far deviating from the mean type in excess, and one in defect. Beyond this the probabilities decrease with extreme rapidity. Thus, for a 7-fold deviation, we must seek

our specimen among 263,000; and, for an 8, 9, 10-fold, among 4,760,000, 250,000,000, and 25,000,000,000 respectively.

* * * * *

Practically speaking, nothing can be simpler or more easily stated than the rules for handling any given series of determinations of a single *quæsitum* supposed to be arranged to our hands in regular progressive groups, with a view to derive from it numerically the only things which it is really important to know, viz., the *most probable value*, the *probable error* of a single determination, and the *weight* of the result as compared with that similarly derived from a different and independent series. But when the data are otherwise grouped, which is a case by no means of unfrequent occurrence, or when a portion only is regularly arranged in groups, and all above or below certain limits massed together in the gross without regard to grouping, much delicacy subsists in deciding, according to just principles, on the exact amount of all these elements; and it would have added much to the practical utility and value of M. Quetelet's work had he given some examples of this nature, with plain and brief rules or formulæ for their working. This is the more to be regretted, because we are actually left at a loss to decide by what numerical process his mean results, where stated, have been arrived at in some of the examples set down. For instance, in that of the Scotch soldiers, where all the groups are regular and all stated, we find it merely mentioned incidentally that the mean is "a little more than 40 inches, whereas the really most probable mean is 39·830, while that which the course of the figures in the tabulated working of the example would appear to indicate as resulting from an equipartition of the numbers of cases in excess and defect is 39·525. Again, in the example of the conscripts, where the extreme groups are massed undistinguishably, the rule of equipartition, according to its simplest and most obvious application to the tabulated figures, would place the mean at 63·939 inches, whereas we find it indicated rather than stated, as follows: "*If it be observed* that the mean height is about 63·947 inches." The difference, it is true, is trifling in itself, but becomes of consequence when the object is from the figures set down to discover by what process they have been obtained.

We come now, however, to that highly interesting part of the work before us which treats of the study of causes, in general; and in the peculiarly complex form it assumes, in those moral and social inquiries, the data for which are gathered by statistical enumeration. A few remarks on the part which the theory of

probabilities plays in these inquiries will not be out of place here.

This theory is connected with the general philosophy of causation and with inductive inquiry in two distinct ways—the one theoretical, and the other practical. When we see an event happen several times in succession in some particular manner, there arises, in the first place, a *primâ facie* probability that it will happen once more in that manner; which, if the number of repetitions be large, forms of itself a very cogent ground of expectation. But the probability that such repetition has not been merely fortuitous, but has resulted from a determining, or at least a biassing cause, increases with each repetition in a far higher ratio, than the simple probability of the once more happening of the event itself. The distinction is that between a geometrical and an arithmetical progression. Thus, for example, the expectation that the sun will rise to-morrow, grounded on the sole observation of the fact of its having risen a million times in unbroken succession, has a million to one in its favour. But to estimate the probability, drawn from that observation, of the existence of an influential cause for the phenomenon of a daily sunrise, we have to raise the number 2 to the millionth power—thus producing a number inexpressible in words and inconceivable in thought, and the ratio of this enormous number to unity, is that of the probability of the phenomenon having happened *by cause*, to that of its having happened *by chance*. The theorem on which depends this curious application of the doctrine of *probabilities* to the expulsion from philosophy of the idea of *chance*, is known to geometers by the name of its first promulgator, Bayes. It must be observed, that as to the nature of the cause thus insisted on, the calculus says nothing. There may be opposing causes, and a daily struggle between them for the mastery. In this case we are simply forced to admit that the arrangements of Nature are highly favourable to the successful exertion of the one, and highly unfavourable to the other.

It is however as a practical auxiliary of the inductive philosophy that we have chiefly to contemplate this theory. Its use as such depends on that mutual destruction of accidental deviations from the regular results of permanent causes which always takes place when very numerous instances are brought into comparison. Examples of this sort have been already adduced, and might be multiplied indefinitely in every department of practical inquiry. Indeed, every phenomenon which Nature offers on the great scale

may be regarded as such. Nothing can be more irregular and uncertain than the action of the wind on the waters,—yet, in the most violent storms, the *general* surface of the ocean preserves its level. What more fortuitous than the fall of a drop of rain in a shower, or the growth of a blade of grass? Yet the soil is uniformly irrigated, and the unbroken sheet of verdure testifies to the resultant equilibrium of that and a thousand other causes of inequality. These things, it will perhaps be said, are the results of Providential arrangement. No doubt they are so; but it is an arrangement working through a complication of secondary causes and contingencies,—on which man, if he will philosophize at all, is obliged to do it by reference to the laws of probability. Still there is no one who is not astonished, in cases where what we are obliged to call contingency enters largely, to find not only that the mean results of several series of trials agree in a wonderfully exact manner with each other, but that the very errors of individual trials—precisely those portions of the special results which are purely attributable to that which is contingent in the process—group themselves around the mean with a regularity which would appear to be the effect of deliberate intention.

"This singular result" (says M. Quetelet) "always astonishes persons unfamiliar with this kind of research. How, in fact, can it be believed that errors and inaccuracies are committed with the same regularity as a series of events whose order is calculated in advance? There is something mysterious, which however ceases to surprise when we examine things more closely."

The rationale of this mystery is this. Where the number of accidental causes of deviation is great, and the maximum effect of each separately minute in comparison of the result we seek to determine,—great total deviations can only arise from the conspiring of many of these small causes in one direction,—the more that so conspire the greater the deviation. Now all combinations being equally possible *individually*, and those combinations which can alone give rise to the extremes of error being necessarily very much fewer in number than those which result in moderate amounts of deviation, we easily perceive that the opportunities for the occurrence of great errors are much rarer than for small ones. And this is in fact the reasoning, which, carried out by exact analysis (assimilating the causes of *plus* and *minus* error to black and white balls in an urn), takes the form of that demonstration of the law of probability, which we have above spoken of as devised by Laplace and simplified to the utmost by M. Quetelet.

There still remains behind, however, this inquiry,—which we have known to occur as a difficulty to intellects of the first order,—*Why* do events, on the long run, conform to the laws of probability? What is the *cause* of this phenomenon as a matter of fact? We reply (and the reply is no mere verbal subtlety), that events do not so conform themselves,—the fact to the imagination,—the real to the ideal,—but that the laws of probability, as acknowledged by us, are framed in hypothetical accordance with events. To take the simplest case, that of a single contingency,—the drawing of one of two balls, a black and a white. We suppose the chances equal, in theory; but, in practice, what is to assure us that they are so? The perfect similarity of the balls? But they need not be similar in any one quality but such as may influence their coming to hand. And, on the other hand, the most perfect similarity in all visible, tangible, or other physical qualities cognizable to our tests is not such a similarity as we contemplate in theory, if there remain inherent in them, but undiscernible by us, any such difference as shall tend to bring one more readily to hand than the other. The ultimate test then, of their similarity in that sense is not their general resemblance, but their verification of the rule of coming equally often to hand in an immense number of trials: and the observed fact, that events *do* happen according to their calculated chances, only shows that *apparent* similarities are very often *real* ones.

The application of this calculus to the detection of causes turns essentially upon this view of the conformity in question, and of the nature and delicacy of this *test by indefinite multiplication of trials* which we are enabled, in many cases, to apply to mixed phenomena. All experience tells us, that where efficient *causes* are known, but from the complication of circumstances cannot be followed out into their specific results, we may yet often discern plainly enough their *tendencies*, and that these tendencies *do* result, in the long run, in producing a preponderance of events in their favour. Were it asked, Why do the strong men, in a general scramble, carry off the spoil, and the weak get nothing? the reply would be, that such is not the fact in every instance; that, although we cannot go fully into the dynamics of the matter, we can clearly see the mode of action in some individual struggles, and that in the whole affair there is a visible enough *tendency* to the defeat of the weaker party. Again, when we reverse this process of reasoning, and declare our conviction that success in the long run is a proof of ability, we give this name to some personal

quality or assemblage of qualities which, acting as an efficient cause through a complication of events we do not pretend to penetrate, has a tendency in that direction which issues in success. Here the tendency becomes known by observation, and the nature of the cause is concluded from the nature of the tendency, by appeal to experience, which, in some instances, has shown us the cause in action, and informed us of its direct effect. But it may happen that observation may plainly enough indicate the direction of a tendency which yet experience has not enabled us to connect with any known cause. And it may further happen that this tendency, which we are driven to substitute in our language for its efficient cause, may be so feeble—whether owing to the feebleness of the unknown cause, its counteraction by others, or the few and disadvantageous opportunities afforded for its efficacious action (general words, framed to convey the indistinctness of our view of the matter)—as not to become known to us but by long and careful observation, and by noting a preponderance of results in one direction rather than another.

And thus we are led to perceive the true, and, we may add, the only office of this theory in the research of causes. Properly speaking, it discloses, not causes, but tendencies, working through opportunities,—which it is the business of an ulterior philosophy to connect with efficient or formal causes; and having disclosed them, it enables us to pronounce with decision, on the evidence of the numbers adduced, respecting the reliance to be placed on such indications,—the degree of assurance they afford us that we have come upon the traces of some deeply-seated cause,—and the precision with which the intensity of the tendency itself may be appretiated.

Such tendencies are often apparent enough, without any refined considerations, or reference to any calculus. Thus, on the consideration of thirteen instances of coincidence between the direction of circular polarization in rock crystal, with that of certain oblique faces in its crystalline form, — it was asserted that the phænomena were connected in that invariable manner which is one of the characters of efficient causation. The chances against such a coincidence happening thirteen times in succession by mere accident are more than 8000 to 1; and this, therefore, was the probability that some law of nature, some cause, was concerned. Subsequent observation has brought forward no exception; but, on the contrary, other cases of a similar character have arisen, which go to place the observed tendency in *uncounteracted* con-

nexion with the efficient cause—which, however, still remains concealed.*

It is, however, the extreme delicacy of the test above spoken of—that property it possesses of bringing out into salience and placing in indisputable evidence, by sufficient multiplication of observations, any preponderance, however small, among the efficient causes in action—that it becomes applicable to those complicated cases in which we find it resorted to. As an instance of this nature, we shall take a phænomenon which has engaged the attention of all who have written on probabilities, from Laplace downwards; one which has been much insisted on by M. Quetelet, and on whose acknowledged obscurity his inquiries have at length thrown a ray of light; viz., the excess of the number of births of male over that of female infants. As a matter of observation, the phænomenon is indisputable; but it requires the assemblage of a great number of instances to bring it out into evidence. In individual experience, or in the birth registers of a parish or small town, the tendency to excess on the male side is quite overlaid and concealed by accidental irregularities. It is otherwise when those of great cities or whole nations are consulted. The irregularities then disappear by mutual destruction, and the result exhibits the tendency in question in its full prominence. If we extract from the population returns of England and Wales the total numbers of registered births in the seven years, from 1839 to 1845 inclusive, we find 1,863,892 males and 1,772,491 females, the excess being 91,401 on the male side, or 105·157 males to 100 females. Suppose it were urged that this may, after all, be a purely accidental excess. It might be said, not without apparent plausibility, that as it would be the height of improbability to expect in so vast a number an exact equality, so, on the other hand, an excess of 91,401, which, though a large number in itself, is yet but $2\frac{1}{2}$ per cent. on the total number of cases, *does not seem so very improbable.* To this theory replies that, where such high numbers are concerned, it *is* so:—that the case assumed in the objection is identical with that of drawing 3,636,383 balls out of an urn containing black and white balls in equal proportion and infinite in number, and that the expectation

* So again, an examination of the elements of all known cometary orbits has disclosed a tendency to direct or eastward motion, increasing in the degree of its prominence with the approach to coincidence of the orbit with the plane of the ecliptic,—and especially marked in the cases where calculation has assigned elliptic elements to the orbit. Here we have a tendency pointing to a cause, still unknown, but with whose effects we are so far familiar that we can trace its action throughout the planetary system, with only two known exceptions among its most remote and insignificant constituents, and those of a very undecided character.

of drawing such an excess of one colour in such a number, so far from a mere moderate unlikelihood, is, in fact, equivalent, supposing the chances equal, to the expectation of throwing an ace 643 times successively, with a single fair die.* Even on a total of 20,000 births we might bet many thousand millions to one that the same relative preponderance would not be found, were the chances even.

It is abundantly evident, therefore, that we have here arrived at a proof of a tendency which must be taken as a law of human nature under the circumstances in which it exists, at least in this country; and the constancy with which the proportion is maintained in successive years, and even in different nations, is not less striking than the fact itself, and shows it to be a result of deep-seated causes, acting with almost absolute uniformity on great masses of mankind. Thus in the seven years from which the above ratio has been concluded, taking them *seriatim*, we find 104·8, 104·7, 105·3, 105·2, 105·4, 105·4, 105·2, on totals averaging about half a million each; while in France a similar comparison gives 105·9, 105·7, 106·1, 106·2, 105·8, 105·9, 105·9, on nearly double the total numbers. As to the causes of this most striking phænomenon, much speculation has, of course, prevailed; but the inquiries of M. Quetelet into the statistics of marriage have rendered it extremely probable† that the relative ages of the parents very materially influence the sex of the offspring, and that the effect is therefore a resultant one, due to this physiological cause, acting through the medium of all those prudential and moral considerations which in civilized states determine the relative ages of parties contracting marriage. This view of the subject is strongly corroborated by a separate examination of the registers of illegitimate birth, which indicate an excess of only 3 instead of 5 per cent.

The causes, or tendencies indicative of causes, which may be disclosed by the assemblage and comparison of numerous recorded instances, are classed by M. Quetelet under three heads: constant, variable, and accidental. The latter class may be considered as entirely eliminated by their mutual destruction when vast numbers are concerned, and the whole series of collected cases is so treated as to afford a single result. The same process also will in great measure destroy the effect of variable causes, if their variation be periodical in its law, and the observations be made indifferently in all the phases of their period. It is the peculiar property, however, of causes of this latter description, through whatever

* The chances against throwing an ace only nine times in succession, are ten millions to one.

† Essai de Phys. Sociale, i. 57. Citing Hofacker and Sadler in corroboration.

train of circumstances their action is propagated, ultimately to emerge to view in manifestations equally periodical with the causes themselves. In cases of dynamical action this peculiarity is susceptible of demonstration, and has been so demonstrated under the name of the "principle of forced vibrations:"* and experience abundantly proves its general applicability to every case of indirect action, whether physical or moral. To those, therefore, who assiduously watch the development of phænomena, and register effects as they arise with sufficient exactness, such causes will be detected, and their periods at the same time disclosed by the periodical fluctuations they occasion; or they may be searched for, if suspected to exist overlaid by accidental errors, by dividing the series of observed results into groups, differing in *phase* (*i. e.*, dividing the extent of the period suspected into several equal portions, and grouping the results observed in each together). The influence of the periodical cause suspected will then become apparent in the form of differences in the mean results of the several groups. Of this process every part of science teems with examples. In astronomy we owe to it the grand discoveries of the aberration of light, the nutation of the earth's axis, the separation of the effects of the sun and moon on the tides, and an infinity of others; in meteorology, that of the diurnal and annual fluctuations of the barometer; in magnetism, the daily and annual changes in the direction and intensity of the magnetic forces; and in statistics, the annual oscillations observable in all the great elements of population, which the researches of M. Quetelet have placed in a distinct light.

But among accumulated masses of results, without any attempt at subdivision into *periodic* groups, the influence of periodical causes may start into evidence on a general inspection of the differences from a mean result, after a totally different manner. We have seen that these differences present *inter se* a definite and perfectly cognizable law of arrangement, so long as their causes are purely casual. Any deviation *from this law* among the differences of the observed values from the mean, then, becomes at once an indication of a determining tendency, and will very often, by the character of the deviation, lead to a well-grounded surmise of the nature of its cause. For instance, if a sudden falling off in the number of observed differences, beyond certain limits either way from the mean, *accompanied with some degree of improbable accu-*

* Encyclop. Metropol. Article Sound, § 323, *et seq.*

mulation at or about those limits, should be noticed, it may be taken as a certain indication of a periodical disturbing influence, having those limits for the maximum and minimum of its effect.

Again, if at any particular point in the scale of results arranged in order of magnitude we should notice a sudden and marked irregularity confined to a small extent, we may be sure that it arises from the action of some single, powerful, and exceptional influence. Thus, from the undue accumulation of conscript measurements below the standard height of 5 feet 2 inches, accompanied with a deficiency to the extent of 2275 cases in the two inches just above that standard, M. Quetelet is led to conclude that an influence foreign to the subject—in fact, a fraudulent practice, favouring the escape of the shorter men, has prevailed to that extent in the formation of the official returns he has employed as the basis of his calculations. (*Transl.* p. 98.)

Astronomy affords us a very remarkable example of this nature, which we adduce, by reason of a singular misconception of the true incidence of the argument from probability which has prevailed in a quarter where we should least have expected to meet it. The scattering of the stars over the heavens, does it offer any indication of law? In particular, in the apparent proximity of the stars called "double," do we recognize the influence of any *tendency to proximity,* pointing to a cause exceptional to the abstract law of probability resulting from equality of chances *as respects the area occupied by each star?* To place this question in a clear light, let us suppose that, neglecting stars below the seventh magnitude, we have measured the distance of each from its nearest neighbour, and calculated the squares of the sines of half these distances, which therefore stand to each other in the relative proportion of the areas occupied exclusively by each star. Suppose we fix upon a circular space of 4″ in radius as the unit of superficial area, and that we arrange all the results so obtained in groups, progressively increasing from 0 by the constant difference of one such unit. Now the fact, to which M. Struve originally called attention*, and on which we believe all astronomers are agreed, is, that the first of these groups *is out of all proportion richer than any of the others;* and that the numbers degrade in the groups adjacent with excessive rapidity; so that, for example, calculating on the numbers given by Struve†, we find the first group to contain 182 cases; the next three 68, or on an average 22 each; the next twelve 70, or 6

* Catalogus Novus Stellarum duplicium, &c. Dorpati, 1827.

† *Ibid.*, p. xxxii., Introduction. Each of M. Struve's classes is doubled, since each constituent of a double star counts as a separate case.

each on an average; and the next forty-eight only 94 in all, averaging 2 to each; while a general average* would assign only one star to 540,000 such units of area. The case, then, is parallel to that of a target of vast size, marked out into 6700 millions of equi-distant rings, riddled with shot marks in the bull's eye, and with a tolerable sprinkling in the first fifty or sixty rings, beyond which the whole area offers nothing for remark indicative of any particular local tendency, though *dotted all over with marks*, in the sparing manner above described. Any one who should view such a target, bearing in mind what is said above, must feel convinced that a totally different system of aiming had been followed in planting the interior and exterior balls.

Such we conceive to be the nature of the argument for a physical connexion between the individuals of a double star prior to the direct observation of their orbital motion round each other. To us it appears conclusive; and if objected to on the ground that every attempt to assign a numerical value to the antecedent probability of any given arrangement or grouping of fortuitously scattered bodies must be doubtful,† we reply, that if this be admitted as argument, there remains no possibility of applying the theory of probabilities to any registered facts whatever. We set out with a certain hypothesis as to the chances: granting which, we calculate the probability, not of one certain definite arrangement, which is of no importance whatever, but of certain *ratios* being found to subsist between the cases in certain predicaments, on an average of great numbers. Interrogating nature, we find these ratios contradicted by appeal to her facts; and we pronounce accordingly on the hypothesis. It may, perhaps, be urged that the scattering of the stars is *un fait accompli*, and that their actual distribution being just as possible as any other, can have no *à priori* improbability. In reply to this, we point to our target, and ask whether the same reasoning do not apply equally to that case? When we reason on the result of a trial which, in the nature of things, cannot be repeated, we must agree to place ourselves, in idea, at an epoch antecedent to it. On the inspection of a given state of numbers, we are called on to hold up our hands on the affirmative or negative side of the question, Bias or No bias? In this case who can hesitate?

* Taking 12,400 as the number of stars of the magnitudes and within the region of the heavens contemplated, viz., from the North Pole to 15° south declination, which number, for the reason in the foregoing note, has to be doubled.

† London, Ed. and Dub. Philosoph. Magazine, &c. Aug., 1849.

Accidentally variable causes overlay altogether the evidence of regular action, so that the elimination of their influence is in all cases synonymous with the extension of knowledge. It is not, however, to this or to any other calculus that we can look for special rules of conduct in this part of inductive inquiry beyond the simple precept of collecting facts in great numbers, and employing mean results in lieu and to the exclusion of single observations wherever numerical magnitude is concerned. This precept is, however, of infinite use in all cases where we test the efficacy of a presumed cause by the numerical correspondence between its known energy and the amount of the observed effect.

* * * * *

Among those branches of knowledge which are most effectually advanced by the consideration of mean or average results concluded from great masses of registered facts, to the exclusion of individual instances, statistics hold beyond all question the most important rank as regards the social well-being of man. To this subject M. Quetelet devotes the fourth and last division of his work; not, indeed, to the delivery of statistical tables or results, nor to the actual discussion of any particular class of documents, but to the points which it so much imports to have generally well understood of the methods and principles which ought to prevail in the collection and subsequent employment of such documents.

Whether statistics be an art or a science (a question to which he devotes a preliminary letter) or a scientific art, we concern ourselves little. Define it as we may, it is the basis of social and political dynamics, and affords the only secure ground on which the truth or falsehood of the theories and hypotheses of that complicated science can be brought to the test. It is not unadvisedly that we use the term Dynamics as applied to the mechanism and movements of the social body; nor is it by any loose metaphor or strained analogy that much of the language of mechanical philosophy finds a parallel meaning in the discussion of such subjects. Both involve the consideration of momentary changes proportional to acting powers,—of corresponding momentary displacements of the incidence of power,—of impulse given and propagated onward, —of resistance overcome,—and of mutual reaction. Both involve the consideration of time as an essential element or independent variable; not simply delaying the final attainment of a state of equilibrium and repose,—the final adjustment of interests and relations,—but, from instant to instant, pending the process of mutual accommodation, altering those relations, and, in effect,

rendering any such final state unattainable. One great source of error and mistake in political economy consists in persisting to regard its problems as statical rather than dynamical in their character; confounding the propagation of impulse with a step towards equilibrium,—a state unattainable where the interests of masses of mankind are concerned. So long, indeed, as society is little developed, its movements fettered, its commercial activity sluggish, and all things go on leisurely, the distinction is one of small importance; a state of *acquiescence*, nearly approaching to that of equilibrium and final adjustment, being taken up from instant to instant, and following at a little distance, yet *pari passu*, the slow changes of the acting causes. It is otherwise under the increased facilities, excessive mobility, and excited energy which prevail under the high temperature and pressure of modern civilization. Friction (which has an equally real existence in both mechanisms) is diminished, the intensity of the active powers increased, the scale on which movements are carried on enlarged,—a state of things which finds its expression in the "over-speculation," "gluts," "panics," "reactions," *et hoc genus omne* of modern commerce and social change. The same must be the case whenever efficient causes, of whatever nature, act through a train of varying circumstances, and result in effects of which it can only be securely asserted that their momentary and infinitesimal changes stand under given circumstances in given relations. It may be true, for example, that capital tends to a common level of profit in the choice among its possible employments; but endless fallacies would be involved in any reasoning which should proceed on the assumption that it finds that level. Demand may tend to increase supply by stimulating exertion, but a supply proportionate to the demand, and steadily following its variations, is what no sound political economist will ever expect to see. The Rule of Three has ceased to be the sheet anchor of the political arithmetician, nor is a problem resolved by making arbitrary and purely gratuitous assumptions to facilitate its reduction under the domain of that time-honoured canon.

Number, weight, and measure are the foundations of all exact science; neither can any branch of human knowledge be held advanced beyond its infancy which does not, in some way or other, frame its theories or correct its practice by reference to these elements. What astronomical records or meteorological registers are to a rational explanation of the movements of the planets or of the atmosphere, statistical returns are to social and political

philosophy. They assign, at determinate intervals, the numerical values of the variables which form the subject-matter of its reasonings, or at least of such "functions" of them as are accessible to direct observation; which it is the business of sound theory so to analyse or to combine as to educe from them those deeper-seated elements which enter into the expression of general laws. We are far enough at present from the actual attainment of any such knowledge, but there are several encouraging circumstances which forbid us to despair of attaining it.

The first of these is the exceeding regularity which is found to prevail in the annual march of statistical returns and the constancy of the ratios they indicate where great masses of population are concerned, where leading features of human nature are the obviously influential elements on which the observed results depend, and where temporary or periodical causes of disturbance (evidently such) do not visibly interfere. As instances might be cited the relative proportion in the births of the sexes already spoken of; the ratio of illegitimate to legitimate births in the same country and the same section of the population; nay, even the number of the still-born (with a distinct percentage for town and country), which M. Quetelet has ascertained to be so uniform in Belgium that, on a total number of nearly 6000 annual cases, the yearly deviation from the mean falls short of 140; the ratio of marriages to the whole population, of second marriages to the whole number of annual marriages, and, still more minutely, of widowers with widows, widows with bachelors, and widowers with spinsters; the relative ages of parties intermarrying; and innumerable other particulars; all which, free as air in individual cases, seem to be regulated with a precision, where masses are concerned, clearly proving the existence of relations among the acting causes so determinate, that there is evidently nothing but the intricacy of their mode of action to prevent their being subjected to exact calculation, and tested by appeal to fact. *Taken in the mass,* and in reference both to the physical and moral laws of his existence, the boasted freedom of man seems to disappear; and hardly an action of his life can be named which usages, conventions, and the stern necessities of his being, do not appear to enjoin on him as inevitable, rather than to leave to the free determination of his choice: while yet, throughout, he feels himself to be a free agent.

Another encouraging feature in the aspect of statistical docu-

ments, which shows them, when properly collected, to be trustworthy for the purposes to which we desire to apply them, and holds out a rational hope of their available application,—is their evident *sensitiveness* to the influence of real and unmistakeable causes, which we are sure, *à priori*, ought to influence them. Thus we see the uniform march in the number of annual marriages, corresponding to an increasing population, visibly accelerated in years of prosperity and abundance, and visibly retarded in those of scarcity and public distress. Thus, too, we see in Bavaria laws restraining marriage result in an increased number of illegitimate births.* Wherever monthly returns, of whatever kind, are compared, the influence of season is marked by a more or less conspicuous annual maximum and minimum. Instances of this, of the most striking character, are adduced by our author in his "Essai de Physique Sociale." In these and similar cases, where we clearly perceive the existence of definite tendencies, or of a generally modifying cause pervading the whole field of their action, it is satisfactory and reassuring to find the result in correspondence with our views. For it must never be forgotten that tendencies only, not causes, emerge as the first product of statistical inquiry,—and this consideration, moreover, ought to make us extremely reserved in applying to any of the crude results of such inquiries the axioms or the language of direct unimpeded causation. The proportionality of cause to effect, for instance, is a principle rather emphatically repudiated in the history of the correspondence of increase of imposts with increase of revenue, and of profits as compared with prices.

"Population," says M. Quetelet, "is the statistical element, *par excellence:* it necessarily rules all others, since it relates, above all, to the people and the appretiation of their welfare and their wants. It would be vain to attempt to form statistics of value without taking as a basis the results of a census executed with all the care and precision which so delicate an operation requires. The other data have no real value, except in so far as they relate to the number of the population. A census carefully made sums, in a measure, the most important problems which can be proposed to a statist. The classification according to age allows of the establishment of tables of population, of forming correct ideas on mortality, on the forces at the disposal of the state in case of necessity, and of fixing the ratio between the useful fraction which contributes to the general well-being, and the fraction which yet requires assistance and support to become in its turn useful. The classification by professions, indicates the means by which the population provides for its subsistence and tends to augment

* The vast multitude of illegitimate births in France would seem to be traceable in great measure to the difficulties thrown in the way of marriage by requiring the expressed consent of a great number of relatives of both parties to its celebration.

its prosperity. . . . Those by civil condition, by origin, by education, furnish the administration with no less precious information to assure internal good order, and to facilitate the execution of the laws."—(*Transl.* p. 183.)

A well-organized system of civil registration ("*état civil,*") is therefore one of the first wants of an enlightened people. No man in such a people is above or beneath the obligation of authenticating his existence, his claims on the protection of his country, and his fulfilment of the duties of a citizen,—or of contributing his individual quota of information, in what personally concerns himself or his family, in reply to any system of queries which the Government in its wisdom may see fit to institute respecting them. Such information may be regarded as a poll-tax, which, in this form, a Government is fairly entitled to make, and which indeed is at once the justest and least onerous of taxes; or rather, it may be looked on as a mode of self-representation, by which each individual takes a part in directing the views of the legislature in objects of universal concern. Nothing, therefore, can be more unreasonable than to exclaim against it, or to endeavour to thwart the views of Government in establishing such a system,—nor anything more just than to guarantee its fidelity by penalties imposed on false returns or wilful omissions.

The analysis of the population returns of a great nation, or rather the drawing from that analysis, duly executed according to rational classifications, just and philosophical conclusions, is a task calling for the exercise of much acuteness and discrimination in appretiating the influence which the relative proportions between the classes, as to age, condition, calling, must necessarily have on national character and habits, and in weighing—with reference to future prospects—the probable influence on that character and those habits which is involved in even a very moderate observed change from time to time, in those proportions.

"The numerical tables of a population, when made with care and with all the development which science requires . . . form, in the annals of a people, the most eloquent page that a statesman can read, if he understand them well. In fact it only belongs to the practical observer completely to understand the language of figures, and not to go beyond what they can teach him. Censuses, well made, and which succeed one another on a uniform plan and at intervals sufficiently near, should present most precise notions of the physical and moral condition of a people,—of the degree of its power,—of its prosperity,—and of the tendencies which may compromise its future: they would teach much better than voluminous inquiries, which are often fettered by prejudices and private interests, what

we ought to think of the retrograde state or the immoderate development of certain branches of industry."

Among the first results of such an analysis, are those general ones which our Continental neighbours technically understand by the "movement" of the population—its increase, that is to say, by the excess of births over deaths and emigrations, and the internal change in the proportions of those living at different ages corresponding to changes, if any, in the law of mortality as indicated by the ages of death. On this point M. Quetelet, in an earlier part of this work, has the following pertinent remark:—

> "The movement of a stationary population is often compared with that of a population increasing by an excess of births over deaths. However, this is a comparison of heterogeneous elements: all other things being equal, the latter population should have a greater mortality; for there are more children in it."

So far as this remark goes it is just, but it does not include the whole case, or exhibit fully the influence of the consideration in question. To judge of the extent of this influence it is only necessary to consider that, in a given population now existing, the individuals living at any assigned age are not the survivors of that age among a number equal to that born in the current year, but among a number born antecedently, when the population was less than at present, in a proportion easily calculated, the age being given, and the annual rate of increase known. Thus, supposing the population of a country to double in fifty years, a man fifty years old is the survivor of only half the number of cotemporary births, and of one hundred of only one-fourth those which would appear, on a comparison of the number actually born in a given year with those actually living at the age specified, in that year. Not only, therefore, are there more children in comparison with adults in an advancing population, but at the same time fewer old men. Now the ratios of the helpless, the active, and the meditative elements of a population to the entire mass and to each other,—of giddy youth and adult enterprise to mature experience, timid caution, and declining powers, must necessarily give rise to corresponding features of national character. A disproportion in this respect, influencing all the great lines of development of national activity and impressing the whole career of a people, cannot but make itself felt in every feature of their existence. It is only necessary to contrast the energy displayed by a nation whose population doubles in twenty-five years, as in the United States, with the sobriety of movement, not to say torpor, of another,

where, as in Holland, it is nearly stationary, to perceive the connexion in question to be that of effect with cause.

"An exposition of the political condition belongs essentially to the statistics of a country. We do not, however, know how to express it in figures. The same may be said of information relative to the moral and intellectual condition. The simple recital of what has passed in a locality at a particular time sometimes better teaches the moral condition of a people than all the numerical tables possible."

* * * * *

The chief difficulty to be encountered in aiming at correct results in the collection of agricultural, industrial, and commercial statistics is, that it—

"Requires the intervention of persons who are almost always interested, or think they have an interest, in disguising the truth. When the government collects them, it is generally opposed by the manufacturer, who supposes it done with fiscal views. The desire to obtain freedom for his industry, and to obtain what are called protecting laws . . . almost always tends to exaggeration in one direction or another. Governments also publish documents on importations and exportations. These tables, which are useful to consult, nevertheless often contain very vague returns: they are generally confined either to the fixing of prices from faulty valuations or of quantities without considering either price or quality. In the official valuations, moreover, we only know a part of the truth: it is especially here that information not susceptible of reduction to numbers becomes necessary, in order to determine the probable quantity which escapes the legally stated values."

Owing to these causes of jealousy and partial presentation, many important statistical elements, relating to matters of pecuniary concern, can hardly be collected by official intervention. It is here that a Statistical Society may render most valuable service by setting on foot systematically, yet amicably and unobtrusively, local and private inquiries, with the guarantee of personal veracity for their answers, and the purely scientific and truth-loving spirit of such a body of enlightened inquirers for their fair presentment.

"The statistics of the moral and intellectual condition of a people," he goes on to observe, "present still greater difficulties; for the appretiation can only be founded on facts much more contestable than those given by industry and commerce. When we say that a province produces so many quarters of corn or so many gallons of oil, we know that the figures may be more or less in error; but we understand the nature of the unit. It is not the same when we say that a province produces annually so much crime. . . . Infinite precaution and sagacity are necessary to read with success the statistics of tribunals, for the documents they contain are very complex in their nature, and almost always incomplete." . . . .

"What a mass of errors have we not accumulated in treating of pauperism! To probe this leprosy of society we have had recourse to lists

of the poor, and very often without inquiring if these lists were complete and comparable in different countries or even within the limits of the same country. Real poverty is nearly always very different from the poverty officially returned. . . . *In Belgium a man will enter his name on the list of paupers to escape serving in the civic guard or to obtain other advantages, without receiving a farthing of public benevolence*" [*! !*].

With such difficulties in the way of exhibiting fairly, and interpreting truly, statistical facts, arises a necessity for laying down precautionary rules for the guidance of those to whom is confided the important task of their collection and registry—for checking their correctness when collected—and for their legitimate employment in aid of legislative or administrative purposes. On each of these heads M. Quetelet gives us a letter—short, indeed, and somewhat desultory; but abounding in useful and sensible remarks. Each of them would, in fact, require a treatise for its complete illustration.

A fool can ask questions, but only a wise man pertinent ones; and it often takes a wiser man to ask than to answer. After recommending to the statist a due and ample course of preparatory study of the subject in hand, our author goes on to observe, on the collection of statistical information:—

"The principal considerations which should guide an administration as to the questions to be asked are the following:—

"1. Only ask such information as is absolutely necessary, and as you are sure to obtain.

"2. Avoid demands which may excite distrust, and wound local interests or personal susceptibility as well as those whose utility will not be sufficiently felt.

"3. Be precise and clear, in order that the inquiries may be everywhere understood in the same manner, and that the answers may be comparable. Adopt for this, uniform schedules, which may be filled up uniformly.

"4. Collect the documents in such a way that verification may be possible.

. . . . . .

"Simplicity and clearness of demand, together with uniformity in the forms to be filled up, are essential conditions to obtain comparable results. Without them, no statistics are possible. When the question relates to ages, professions, or diseases, it is of the greatest importance to employ classifications perfectly identical, in order that the general information may be compared even to the slightest detail. The most perfect unity should reign throughout the whole. It is to establish a unity like this that in certain states, such as Belgium and Piedmont, central commissions have been formed to collect and arrange the different elements which should be included in the national statistics. The necessity of such institutions is particularly shown when we see in very enlightened countries the principal departments sometimes publish very different numbers to express the same

things, or make classifications which render comparison impossible."—(*Transl.* pp. 196, 197.)

Not to secure facility for the verification of the documents we collect is to miss one of the principal aims of the science. Statistics are only of value according to their exactness, without which they can serve but to establish error. Every statistical document requires a twofold examination—a moral and a material one, the former being, in all cases, by far the most important, as it involves the inquiry into the influence under which it has been collected—a point on which the whole colouring of the document essentially depends:—

"During the war of independence, the United States carefully misrepresented the true number of their population: they exaggerated considerably the numbers of inhabitants in maritime cities, in order to put the enemy on the wrong scent. Assuredly no good estimate of the American population could be founded on the documents of this period."—(*Transl.* p. 202.)

Every statistical document ought to carry on the face of it, the exceptions, exemptions, and limitations, under which its entries are made. In respect of the use which may be made of it, negligence in this respect may amount in effect, if not in culpability, to a falsification.

"Thus, by means of official numbers, M. Sarauw pretended to prove that in the island of St. Croix, in the Danish Antilles, the mortality of the black slaves was less than that of white men even in Europe; and this assertion might appear so much the more imposing, as M. Sarauw resided in the island in question."

This result (which was arrived at in good faith) rested solely on the omission of negro children, dying before attaining their first year, from the register of births, such children being exempt from poll-tax, and therefore their omission being deemed of no importance.

The material examination of statistical documents rests chiefly on the internal evidence they may offer of self-consistency. It is singularly aided by diagrams. A simple line, properly laid down from a consecutive series of numbers, by what is called graphical projection, enables us to apprehend at a glance the continuity and regular progression of their succession; and, what is of still more importance, to apprehend correspondences between two series so projected, which often afford immediate conviction of a relation between them, such as the most subtle mind would find it difficult to perceive without such aid. They give to the study of pheno-

mena the same advantage which algebra has introduced into calculation—they generalize and allow of abstraction; and they enable us at once to detect and often to rectify errors which, if undetected, would affect mean results, and throw everything into confusion. We are glad to find M. Quetelet strong in his advocacy of this mode of dealing with a series of observations which the generality of French *savans* affect, very unwisely, to despise as inconsistent with their notions of mathematical rigour.

There is nothing more indicative of a man's fitness or unfitness for the duties of a legislator and a statesman than his manner of dealing with statistical documents. When appealed to, as they too commonly are, for the purpose of establishing extreme positions, or of lending support to party views, or to particular interests, we are continually reminded of the doctrine of one long accustomed to listen to such arguments. "Nothing can be more fallacious than theories—except facts!" Those who use them in this manner will be found invariably to sin against truth and common sense in one or other of the following ways, viz.:—

"1. By 'having preconceived ideas of the final result.'

"2. By 'neglecting the numbers which contradict the result they wish to obtain.'

"3. By 'incompletely enumerating causes, and only attributing to one cause what belongs to a concourse of many.'

"4. By 'comparing elements which are not comparable.'"

To which we may add a 5th, the most common of all and the most inexcusable, viz.: singling out the extreme partial results which tell on the side to be defended, and ignoring all the rest.

With such eclecticism we may find in statistics the means of defending almost every position. In politics, especially, they

"Become a formidable arsenal, from which the belligerent parties may alike take their arms. . . . Some figures, thrown with assurance into an argument, have sometimes served as a rampart against the most solid reasoning; but when closely examined, their weakness and nullity have been discovered. Those who allow themselves to be frightened by such phantoms, instead of looking to themselves, prefer rather to accuse the science than to confess their blind credulity, or their inability to combat the perfidious arms opposed to them.

"We see persons profoundly convinced of a truth, seek to establish it directly by the authority of figures, and give, as they think, a mathematical demonstration. However, by means of the statistical documents which they unskilfully employ, they most frequently produce an opposite effect to that which they desired. Thus we cannot reasonably doubt that enlightenment contributes to man's happiness, by illuminating his intellect and fortifying his morals. In the attempt to demonstrate this what has been done? It has been thought necessary to establish that the number of

crimes is inversely as the number of children sent to school—as if the number of crimes, even were it known, had as its only cause the greater or less development of the intellect; and as if the development of intellect were measured by the number of children sent to school. What has been the result of this? It has been found, after well examining statistical documents, that the number of crimes is more generally in a *direct* proportion to the number of children sent to school, than in the *inverse* proportion. The conclusion is exactly the opposite of what was at first desired—a new error, which some have, with the same levity admitted."—(*Transl.* p. 214.)

The necessary incompleteness of all statistical documents is sometimes urged as a general argument against trusting implicitly to conclusions drawn from them. The argument is valid, in so far as we have reason to believe that the unenumerated cases differ systematically, *i.e.*, in some essential point of classification, from the enumerated; so as to render the proportions in which the several classes are represented in the returns different from what they would be were the enumeration complete. But granting their incompleteness—and granting even that the incompleteness is such as to affect injuriously the proportionate numbers in classified results—this does not preclude the drawing of many sound and valuable conclusions from such documents, if only we are assured that in comparing similar ones for several successive years, or under circumstances otherwise different, the same causes of incompleteness prevailed and continued to affect the several classes in an invariable ratio.

This position M. Quetelet illustrates by a reference to the Criminal Statistics of Belgium.—Prior to 1830 the official returns gave only the number of crimes *known* and *prosecuted*, but for the seven years from 1833 to 1839 they included also the number of crimes known, but which were not prosecuted because the authors were unknown. Now it was found that this latter number proceeded from year to year with even more regularity than that of crimes prosecuted. No doubt, therefore, the number of crimes altogether unknown to justice, could it have been made a matter of registry, would have presented a similar constancy. Of known crimes against person, two thirds were regularly prosecuted, and one third escaped, the authors being undiscovered. In the case of crimes against property the proportions were reversed, and were nearly those of one fourth and three fourths; the graver crimes being those most sure of detection. On the whole it would appear from these records that out of 1154 crimes annually known to justice in Belgium, only 416, or little more than one third, formed

subjects of prosecution. Assuming, then, that the number of unknown crimes is equal to that of known (this would hardly be admissible for crimes against person), the amount of prosecuted crimes in Belgium would not exceed one sixth of those actually committed.

"I am absolutely ignorant and shall never know whether the crimes on which the tribunals have to pass judgment form the sixth or seventh or any other part you will of the total number of crimes. What is important for me to know is that this ratio does not vary from year to year. On this hypothesis I can judge *relatively* whether one year has produced more or less crimes than another."

Admitting that this ratio remains invariable from year to year, and that justice pursues criminals with the same activity, two countries or two provinces of the same country might be compared in respect of morality. But as the latter condition almost certainly does not hold good under different administrations, it becomes impossible, from the official returns of prosecutions, fairly to institute such a comparison between nations. Even should the same legislation, the same repression, and the same activity to bring criminals to justice, subsist, if the result be made to depend on a comparison of the number of *condemnations*, instead of those of *prosecutions*, a difference in the mode of trial would alone suffice to destroy the comparability of the cases.

"We know, in fact, that the establishment of the jury in Belgium has doubled the number of acquittals."—(*Transl.* p. 227.)

The letters on the use of statistics to the administration and on the ulterior prospects of this branch of science, though they can hardly be said to contain anything very new or striking, yet come opportunely at a period like the present, when vast changes, both legislative and economical, are in progress, and when opportunities are lapsing of seizing *in transitu* results which will one day be most valuable for future comparison. Steam, railroads, and free-trade principles are making such inroads into all that used to be considered fixed or slowly alterable, that it will be of the utmost interest to have secured points of departure in the new career which opens on society.

"Statists should be eager to register, from this time forward, all the facts which may assist in the study of this vast transformation in the social body, which is in process of accomplishment.

"A government in modifying its laws, especially its financial laws, should collect with care documents necessary to prove, at a future stage, whether the results obtained have answered their expectation. *Laws are*

made and repealed with such precipitation that it is most frequently impossible to study their influence."

These words deserve to be written in letters of gold. They point to an evil whose tendency is to degrade social policy from the list of sciences of observation and experiment to the rank of an empirical art. *Avant nous le Chaos! Après nous le Déluge!* should be the motto of that statecraft which, under a momentary sense of pressure from those whom even the uneasiness of change makes restless and impatient, urges on the social movement faster than a sound philosophy can count the revolutions of its mechanism or register the work accomplished; or of that which, by the simultaneous alteration of every condition, makes the separate estimation of any single effect hopelessly impracticable.

HOME AND FOREIGN INTELLIGENCE.

WESTMINSTER AND GENERAL LIFE ASSURANCE ASSOCIATION.

Established 1836.

EXTRACT FROM THE REPORT OF THE DIRECTORS.

The Directors have much pleasure in meeting the Members on this the Sixth Quinquennial General Meeting of the Association, as it is their agreeable duty to report that the Accounts show the result of the business for the last five years to have been the most favourable which has occurred for any similar period since the establishment of the Association.

The Association during that period has progressed steadily, though not with any marked rapidity. The number of New Policies issued since 1862 having been 900, assuring £363,947., the New Premiums on which have been £12,306. 10s. 10d.

The total Income of the Association has increased from £34,769., in 1861, to £42,830., in 1866; £68,794. has been added to the assets, the funds of the Association having increased from £188,786. to £257,580., as shown by the accounts appended.

143 Policies have become Claims, assuring £73,263. 18s. 4d. on 120 lives. On these Policies there has been a further sum of £3722. 2s. 2d. paid for Bonus additions, making the total amount paid £76,986. 0s. 6d. On this occasion, a son the last periodical division, the Directors have to observe that the average sum assured by the Policies which have become Claims exceeds the average sum assured by the Policies of the Association generally.

£13,097. 8s. 6d. has been received for the purchase of 33 Immediate Annuities, amounting to £1241. 0s. 2d.; on this branch of business, the Directors have to report, that hitherto the result has been very favourable to the Association.