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, by M. Quetelet stripped 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 familiarised 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 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 is, therefore, a compound event of which they are the simple 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 1, 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.

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, *à 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. To illustrate this, we may recur to the case of a marksman aiming at a target. Suppose, that on counting the marks left by his practice, it has been found, on the result of a great number of (say 1000) trials, that half his shots had struck within 10 inches of the centre. About this point let circles be described, the first at 2 inches distance, and others at distances progressively greater by 2 inches at a time. Then it will be found, on counting the marks within the areas of these several circles, that their numbers, up to the tenth circle or to 20 inches distance, will run as follows: — viz. 107, 213, 314, 411, 500, 582, 655, 719, 775, 823. Within the 15th circle, or 30 inches, already 957 shots will be found to have struck; and within 40 inches, 993. Only one out of the whole thousand will be found beyond the 25th circle, or have erred so far as 50 inches from the point aimed at; and not one in 20,000 (were the practice prolonged so far) would stray beyond the 30th or err 60 inches. Computations of this sort are rendered exceedingly 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. Were our marksman, for instance, only half as skilful, or to have 20 instead of 10 inches as the expression of his probable error, we have only to double the diameters of all the circles, and his shots will be found distributed among them according to the same succession of numbers. 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 two principles of demonstration having less in common than that given above with that 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 developement, 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