AN ESSAY ON THE THEORY OF THE COMBINATION OF OBSERVATIONS

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BY

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ON THE

THEORY OF THE COMBINATION OF OBSERVATIONS.

PRESENTED TO THE ASHMOLEAN SOCIETY, FEB. 26, 1844.

IT is well known that the method of Least Squares does not, strictly speaking, give the most probable result to be drawn from a system of observations, unless either the number of observations be indefinitely great, or the function expressing the relative facility of errors have a particular form; whilst it is certain that, in practice, neither of these conditions ever subsists otherwise than approximately.

Yet it seems to be admitted that the method in question does strictly, in some sense or other, give the *best* result which can be obtained. No one, however, attempted a demonstration of this principle before Professor Gauss. In his treatise entitled "Theoria Combinationis Observationum erroribus minimis obnoxiae," (Göttingen, 1823), he has considered the following question: Admitting that the number of observations is finite, and that we do not know the form of the function by which their character is defined, what conclusion ought we to adopt under these circumstances? or, to employ his own term, what is the most *plausible* result? And he has demonstrated that the method of Least Squares must be used, provided we assume that result to be most plausible which would give the least average value to the *square of the error* in an infinite number of trials. (*Theor. Comb.* art. 6. and 21.) But this assumption, however strongly it may commend itself to the instinct of the mathematician, confessedly involves something arbitrary, and so there is still something wanting to the logical consistence of the theory which rests upon it.

In the following essay the subject is considered under a different aspect, but the results are identical with those obtained in the treatise just referred to, at least so far as they are comparable with them. The reasoning employed professes to be demonstrative in the same sense in which the ordinary *a priori* proofs of the fundamental theorems of Statics profess to be so; and as it is important that it should be clearly understood in what sense this is, I have added some remarks at the end upon this point. But whatever may be thought of the logical character of the processes, I believe it will be allowed that they contain nothing *arbitrary;* that no principle is assumed which is not obviously and unquestionably the most plausible, or rather the only plausible one which could be assumed at all. And if so, the complete coincidence of the conclusions with those deduced from a perfectly distinct and independent set of principles may be looked upon as an interesting fact, notwithstanding the failure of any particular attempt to explain it.

SECTION I.

1. If the value of an unknown quantity x has been determined by one observation not absolutely accurate to be $=x_1$, and by another observation to be $=x_2$, and if there are no other data, it is plain that we must assume for it some value x, intermediate between x_1 and x_2. Suppose x (algebraically) less than x_2. Then the first observation, considered by itself, supplies a reason for *diminishing* x; the second, a reason for *increasing* x. Therefore x must be so assumed that these two reasons may exactly counterbalance one another. Let *force* be defined to be *reason for alteration*, and let a quantity be said to be in equilibrio when the force tending to diminish it equals the force tending to increase it. We have now to shew how such forces can be numerically compared, and to investigate the laws of their equilibrium.

2. Let two observations be said to be of the same kind, when two numbers m and n can be found, such that if m repetitions of the first observation concurred in giving the same value for the unknown quantity, our knowledge of it would be exactly the same as if n repetitions of the second had similarly concurred. Let the reciprocals of m and n be taken to measure the *weights* or *absolute forces*[a] of the respective observations. Thus

[a] It is important to observe that this is a mere definition, and affirms no proposition whatever.

if we take a particular observation as a standard, and call its weight 1, the weight of any other observation will be represented by g, if $g=\frac{m}{n}$, and n repetitions of it be equivalent to m repetitions of the standard observation. In what follows, all the observations are supposed to be of the same kind, i. e. comparable in respect of their weights.

3. The determination of a single unknown quantity x by direct observation, may be considered as the determination of the position of a point in a straight line by means of its distance from a fixed point in the same line.

Suppose two observations of the same weight g give two different positions A and B for the unknown point P. It is manifest that we must place it at a point C half way between A and B, since there is no reason for placing it nearer to one of these points than to the other.

4. The *weight* of this determination is $2g$. That is, our knowledge of the position of P is exactly the same as if one observation of weight $2g$ had placed it at C.

For let a be the distance between A and B. Then if the weight in question be not equal to $2g$, let it $=\gamma$, and γ must depend in some way upon g and a. Suppose

$$\gamma=g\cdot\phi(a)$$

(the equation must be homogeneous with respect to γ and g, otherwise its meaning would be altered by altering the unit of weight). We know that $\phi(o)=2$, since if both observations had concurred, the weight of the result would have been $2g$ by definition. Also $\phi(a)$ cannot be a function varying continuously with a; for if it were, the meaning of the equation would be altered by altering the unit of linear measure. Therefore either $\phi(a)$ is constant and $=2$, or it is discontinuous, and $=2$ when $a=0$, but has some other constant value when a is different from nothing.

I shall assume that the supposition of discontinuity is inadmissible[b], and consequently that $\gamma=2g$.

5. Hence we may substitute for the results of two observations of equal weight g, their arithmetical mean; and consider it to be the result of a single observation of weight $2g$. And conversely, for a single observation of weight g, giving $x=x_0$, we may substitute two observations of weight $\frac{1}{2}g$, giving $x=x_0+c$, $x=x_0-c$ respectively, where c is entirely arbitrary.

Consequently, by a process identical with the well-known Archimedean demonstration of the equilibrium of the lever, it follows that if a number of observations whose weights are $g_1, g_2, g_3 \ldots\ldots$ give respectively $x_1, x_2, x_3, \ldots\ldots$ for the value of an unknown quantity x, we may substitute for them a single imaginary observation giving

$$x=\frac{g_1 x_1+g_2 x_2+g_3 x_3+\cdots}{g_1+g_2+g_3+\cdots} \qquad (1)$$

with weight $=g_1+g_2+g_3+\ldots\ldots$

[b] The same assumption is necessary in the theory of parallel forces. The expression for the resultant of two equal and parallel forces cannot involve a continuous function of the distance between them; but it might conceivably be equal to their sum when the distance was nothing, and have some constant ratio (not unity) to their sum when the distance was not nothing.