MATHEMATIKA

A JOURNAL OF PURE AND APPLIED MATHEMATICS

VOL. 1. PART 1. JUNE, 1954. No. 1.

HISTORY OF SCIENCE

and

PSYCHOLOGY OF INVENTION

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That these two subjects—the History of Science and the Psychology of Invention—are intimately connected with one another, is immediately evident and needs no explanation. Perhaps, however, it has not always been sufficiently appreciated. The recent Congress for the History of Science (Jerusalem, 1953) has given me an opportunity of trying to apply to the latter the data of the former.

Indeed, a striking, but by no means infrequent, fact observed in scientific research is that a result may be overlooked by a scientist although it is an obvious and immediate consequence of those which he has himself obtained. Barrow, in his *Lectiones Geometricae*, mentions it, most appropriately calling it an ἀβλεψία—we should say, today, a psychical blindness.

That such an oversight leads to many opportunities for error in the History of Science, is clear. I happen to be able to illustrate this most directly by a personal example. A couple of years after the publication of my Thesis, Edmund Landau wrote to me to say that the German mathematician Pringsheim thought he had discovered the theorem that a power series with real and positive coefficients (and a finite positive radius of convergence) has necessarily a singularity at the point of the circle of convergence on the positive part of the real axis. " But ", he added, " of course, this result is yours ".

Now, I had to undeceive Landau. Of course, the result *ought* to have been found by me, as being implicitly and most evidently contained in my own work; but, as a matter of fact, it was not. I should certainly have seen it if I had looked for something of that kind—but, as it happened, I did not.

So much for my exchange of letters with Landau, as it fell out; but what would have happened if I had not been alive to receive his letter? He would not have been undeceived and the discovery would have been thought to be mine—until other readers would have restored it, not to Pringsheim but to E. Borel.

Another somewhat analogous circumstance is that an important discovery may have been made by somebody who, however, *does not perceive* that he has made a discovery—I mean a significant one. Or, at least, he misinterprets its meaning. He has made an essential discovery, but he does not know it.

Both phenomena occur fairly often in the History of Science. For instance, one cannot but notice them in the origins of the Infinitesimal Calculus. Ptolemy in Antiquity, then Nicole Oresme in the Middle Ages, then again Kepler in the XVIIth century, had remarked that the variation of a quantity becomes imperceptible in the neighbourhood of a maximum or a minimum. None of these three understood that this was an important principle, which deserved and required further developments. Fermat (in about 1637) was the first to be conscious of this, and he introduced, in correspondence with any function f of a variable t, the quantity

$$\left(\frac{f(t+h)-f(t)}{h}\right)_{h=0},$$

in our present language, its derivative. He applied this new concept to the discovery of maxima and minima, and also to the finding of tangents, noticing that the two problems are immediately reducible to one another. Moreover, he observed that the tangent can be practically identified with the curve itself in the neighbourhood of the point of contact, as regards both the ordinate itself and the length of an element of arc.

Now, you would say that this is creating the Differential Calculus, the fundamental notions of which are all included in the above. But we shall see presently that this conclusion is not in accordance with the later course of Science.

The next essential step in the foundation of the Infinitesimal Calculus was the discovery of the fundamental theorem of the Integral Calculus, the " theorem of inversion ", *i.e.* the fact that quadrature is precisely the operation inverse to differentiation. This was the work of Torricelli (following Galileo) and, some 20 years later, of Barrow. Or rather it was —and it was not. What Torricelli and Barrow enunciated is indeed, when we read it, exactly the fact in question, but nobody in their time recognized it nor could recognize it; and they themselves did not recognize its true meaning, as we now do. For us, the " inversion theorem " permits the evaluation of areas, volumes, and so on, by reducing such operations to their inverses, *i.e.* differentiations; for the contemporaries of Torricelli or Barrow, differentiation was *the difficult* operation and a victory was won in the (quite exceptional and somewhat artificial) case when the given function happened to be the result of a quadrature.

This is what we can hardly understand in our present state of mind. The rules of differentiation, one of which corresponds to every operation used to define a function, are quite simple and are apparent to us at once. How is it that Torricelli as well as Barrow had no idea of them?

Unless, during the 30 years which preceded the *Lectiones Opticae et Geometricae*, scientists were not acquainted with Fermat's work—which is very unlikely to be true for Barrow, while we know it to be false for Torricelli—must we admit that Fermat's Memoirs did not contain these principles concerning differentiation?

Now, such is the case. Running over these celebrated Memoirs, we see that Fermat obtains the values of derivatives in numerous instances, but never gives any general rule for obtaining them.

The most curious instance of this kind concerns the derivative of a quotient. The historian of Mathematics, Zeuthen, notices that the rule for such a differentiation is formulated by Barrow . . . with the help of Integral Calculus! Now, Fermat had already applied differentiations to a quotient, for he had investigated the extrema of the quadratic function $\frac{ax^2+bx+c}{a'x^2+b'x+c'}$. Summing up, we see that:

Fermat carried out differentiation in every special case, but did not realize that there were general rules for this, by means of which the operation was always simple;

Nor did the contemporaries and the immediate successors of Fermat realize that these rules existed;

Therefore they thought that the theorem of inversion might be used for derivation and did not understand its use for quadrature.

How many curious cases of ἀβλεψία!

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(*Received 29th November, 1953.*)