AS LUCK WOULD HAVE IT—A FEW MATHEMATICAL REFLECTIONS[1]

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The kind invitation of the Editors has encouraged me to set down one or two personal reminiscences concerning some of those scientific discoveries with which I have been more or less directly connected. It may be worthwhile, I think, to examine a few circumstances that are the result of contemporary scientific thought.

One phenomenon that has always struck me very forcibly is the importance of the role played by chance, not only in the actual discovery of certain scientific facts, for that is well known, but in the awarding of merit to those who originally make these discoveries—the attribution, for example, to such and such a scholar, of a result later proven to be essential.

For example, it is most certainly not widely known that the seeds of a great number of the main findings of modern mathematics are to be found in the complete works of Cauchy. This illustrious mathematician has been the victim of his own vast output, a mass of work contained in an unbelievable number of volumes, an entire library in themselves. The consequence of this enormous output is that few mathematicians have been able to acquire a detailed knowledge of the whole of Cauchy's thought. To give one instance, it is generally not known that the essential fact of Fredholm's theory of equations (1902) was familiar to Cauchy as far back as 1848. But all that is, however, of very little importance—the vital thing is that a useful discovery should be made, no matter by whom, and that the world should be informed of it and able to make use of it to push ahead even further. The question of priority remains ever secondary in the face of reality.

It so happens that I have known just such a case for having closely followed it, a case that clearly demonstrates how much the true story of mathematical discovery can differ from the story commonly accepted. Allow me to relate this adventure. It is understood that I do so with no intent of claiming precedence of discovery for anyone, and, moreover, the mathematician concerned will be nameless.

At the turn of this century a young and unknown mathematician, toiling alone in his small corner, hit upon a result that seemed to him of interest. Following the custom, still in honor today, he submitted his findings to a member of our Academy of Sciences in the hope that they might be men-

[1] English translation by Mme. Marian Villat.

tioned in the minutes of this illustrious body. The mathematician whom he had approached replied that the problem was undoubtedly correct, but of little interest, in any case not sufficiently outstanding to be presented to the Academy. The young scholar accepted this opinion, relegated his paper to the back of a drawer, and got on with something else.

Five years later, the findings in question became famous under the name of "Joukowski's Theorem." Today everyone knows that this is one of the essential theorems of contemporary mechanics of fluids.

In this article we are not seeking to incriminate anyone: it is the run of things that a discovery may not always be judged at its true value at the time it is made, when no one can foresee how it best can be used or to what problems it can be applied. And it is quite in order that a discovery should be identified with the name of the scholar who first published it. The point of this story is simply to illustrate how chance may intervene in the development of a scholar's career, to the extent perhaps of sending it off in the wrong direction.

This same mathematician, however, later saw his career guaranteed by another stroke of fortune, no less strange than the last but with happier results. Having put to one side as useless his work on "Joukowski's Theorem" he set himself to another problem. Convinced this time of having drawn from this new problem something of real importance he took his findings to Emile Picard. The latter, receiving him with his usual kindness, nevertheless immediately said: "It seems to me rather unlikely that you have really solved your problem, for this question has already been the subject of much research on the part of Vito Volterra and Tullio Levi-Civita. These two have both come to the conclusion that the problem cannot be solved by any of the methods known today. One of them has even written to me to say that it was an insoluble problem. Nevertheless, I shall examine your notes. Come back in a few days and I'll tell you what I think about them." The young mathematician was rather cast down, having been completely unaware of the research already done on his chosen subject. If he had read about the work of these two eminent Italians, he would certainly have abandoned the question, but his very ignorance had served him well, leaving his mind free and completely independent. A short while after, he learned that his calculations were correct. Imagine his joy! By the method that he had used, the problem was really very simple, and it was, to his way of thinking, unjust to be accredited with the exaggeratedly brilliant reputation that became his. This reputation nevertheless opened up for our young scholar a career about which he had no complaints to make.

One would be tempted to say that this was a compensation offered by fate and justly merited. This is not my opinion. Fate is surely blind and is not concerned with justice or equity. Everyone knows this and the Greeks, who knew or divined many truths, had already written:

Ζεὺς δ' αὐτὸς νέμει ὄλβον Ὀλύμπιος ἀνθρώποισιν,
ἐσθλοῖς ἠδὲ κακοῖσιν, ὅπως ἐθέλῃσιν, ἑκάστῳ.

"Zeus himself distributes happiness to men, to the good as to the bad, as he desires." One would have to be very naive or vain to imagine that there exists a supreme will charged with the task of correcting the capriciousness of fate.

What is to be retained from this little tale is chiefly the fact that it does not always pay to have an unquestioning confidence in what one usually calls the "history of science." Many hazards, unheard of and unsung, similar to those I have related, have certainly occurred countless times and in countless different circumstances, without, for all that, impeding the amazing expansion of scientific thought. This is the basic essential.

In the course of my life I have witnessed some very strange adventures. Here is one, the outcome of which was sad and which concerned a well-known mathematical theorem.

Every mathematician knows of Fermat's Last Theorem, that theorem formally innovated by Fermat but which has never yet been demonstrated. Certainly several attempts have been made, some by very eminent brains. It happened that a mathematician of Montpellier, having spent some time laboring on the question, published a demonstration of the theorem. This publication was received with great interest and provoked much resounding discussion, and for several months the geometrician was showered with congratulations and honors. A chair at the Institute seemed likely to be offered him in the near future. And then the bomb exploded! A Swiss scholar published an article in which, after having acknowledged the very great value of the work in question, he went on to say "At a certain point of his deduction the author declares: There are now two possibilities, either this . . . or that. . . . He then takes each alternative, examines it in detail and draws from them both the desired result. But in point of fact at this stage of his deduction, there are not two possibilities, but three, and the author has omitted the third. Consequently the demonstration has not been made."

Rarely has misfortune struck so hard a blow as on this occasion. Naturally the hopes raised by the publication of this false success evaporated in a flash and the author suffered profoundly. He asked to be nominated elsewhere and soon disappeared from the public eye. It is not beyond the bounds of conjecture that this unhappy experience hastened his end. And when one dwells upon it, it is a most peculiar thing that Fermat's Theorem should have become an important element in mathematics, that because it has been proposed by a personality of world renown, it has become a subject of much learned research, all of it sterile, on the part of many emiment mathematicians. And indeed the theorem itself, whether true or false, has no particular importance. It has only the value that one desires to give it. This fact echoes an observation made to me once by one of my colleagues: "I have chosen as my field of research the theory of numbers because it is of no practical use." Heaven preserve us from adopting, without reserve, this nihilistic attitude!

Nothing is more extraordinary than the quips and quirks of fate whose pawns we are. One of the most curious illustrations of this has been given us by the way in which honors are eventually distributed to scholars in our

time. Everyone knows that entrance to the Academy of Sciences in Paris is by election. This Academy is made up of different sections, each having six members so that there are six mathematicians, six geologists, six botanists, six physicists, and so forth. When the question arises of replacing a member of a certain section, all the members of the Academy have a right to vote. Is it really logical to ask a mathematician, for example, to make a choice between several botanists who are candidates? However much he seeks to be informed about a certain candidate can he be absolutely sure that his choice is the best possible?

An esteemed physicist has revealed to me how, in the past, he got over this difficulty. This scholar, by nature very affable and cordial, used to welcome and listen to each candidate in a most kindly fashion and indeed, promised his voice to all, without exception! On the day of the election he would put into the election urn a voting paper on which he had written the names of all the candidates. In this way he kept strictly to his word. The president of the Academy, counting the votes and drawing from the urn this exceptional ballot paper, would rapidly consult with his assessors and would then declare: "One paper null and void." The adoption of such a method seems rather difficult and very infrequently is it attempted. Moreover, one always entertains the hope that by the consultation of competent authories, the problem of election will be correctly solved, to the satisfaction of the section concerned and to the other members. But not always, nevertheless.

I can remember very precisely a case that I, for one, deplored bitterly. About twenty years ago, an illustrious geologist whom we shall call A, a member of the Academy, was unanimously considered as the dominant personality in the entire field of his own specialty. From his works came a universally accepted theory concerning the origins of the Pyrenees. Another geologist, let us call him B, was a candidate for a chair in the section of geology. As a result of research upon these same mountains, B thought to have proved the presence of oil in the region of the Pyrenees. But A esteemed that he had already proved the contrary. The obvious conclusion was that B's findings were considered unreliable, and as a result his candidature for the Academy was shelved for many a long year. Then unexpectedly a company was formed with the intention of financing oil prospecting in Aquitaine. These initiatives resulted in the foundation of L'Association des Pétroles d'Aquitaine, thanks to which the town of Bordeaux and other towns in the area are now abundantly supplied with oil. This amazing coup de théâtre at once removed every obstacle that, up to then, had dogged the advancement of B's career. At the next Academy election he obtained a strong majority. Nevertheless, B had been seriously affected by his painful experience and years of disappointment and now only enjoyed for a few months before his death the honor that he had so much longed for.

The oddities of fate are so numerous that they defy any attempt at counting them. It is particularly worthwhile not to overlook the mishaps that have claimed as victims certain scholars who have not realized at the right

moment the full potential of what they have discovered. For example we know what happened to the great physicist Leduc, famous for his laboratory skill. One day he performed an experiment to do with atmospheric air. The results obtained did not agree with what was already known about the composition of air. Leduc persuaded himself that he had made a mistake somewhere, whereas in reality he had done nothing more or less than discover argon—as an English colleague demonstrated almost at once, having had more confidence in the experimental skill of Leduc than the great master himself.

A similar misadventure befell the mathematician Jacques Hadamard at the beginning of this century. The Traité de Géométrie written by this genius is well known. At a certain stage in this renowned work the author examines the system of circles in space, and, without being aware of it, reveals the properties of what has since been named the cyclic system. Only a few words were wanting to make it all clear but these few words were only written a little while later by André Bloch, whose work, published in the *Mathematical Journal* about 1920, caused a real sensation.

The element of the unexpected must never be forgotten, for the unexpected is the rule in scientific research. We should not be too unduly surprised at this; we must remember that in mathematics, as an eminent scholar has written: "There is nothing more simple than what was discovered yesterday but there is nothing more complicated than what will be discovered tomorrow."