IX. THE GENERAL DIRECTION OF RESEARCH

BEFORE trying to discover anything or to solve a determinate problem, there arises the question: what shall we try to discover? What problem shall we try to solve?

Two Conceptions of Invention. Claparède, in his introductory lecture before the above-mentioned meeting at the Centre de Synthèse, observes that there are two kinds of invention: one consists, a goal being given, in finding the means to reach it, so that the mind goes from the goal to the means, from the question to the solution; the other consists, on the contrary, in discovering a fact, then imagining what it could be useful for, so that, this time, mind goes from the means to the goal; the answer appears to us before the question.

Now, paradoxical as it seems, that second kind of invention is the more general one and becomes more and more so as science advances. Practical application is found by not looking for it, and one can say that the whole progress of civilization rests on that principle. When the Greeks, some four centuries B.C., considered the ellipse—i.e., the curve generated by the points M in a plane such that the sum $MF + MF'$ of their distances from two given points F, F′ be a constant—and found many remarkable properties of it, they did not think and could not think of any possible use for such discoveries. However, without these studies, Kepler could not have discovered, two thousand years later, the laws of motion of planets, and Newton could not have discovered universal attraction.

Even results which are more strictly practical obey the same rule. Balloons, in earlier days, were filled with hydrogen or lighting gas, which constituted a serious danger of fire. At the present time, we are able to fill balloons with incombustible gas. This progress has been possible for two reasons: in the first place, because one has succeeded in knowing which substances exist in the atmosphere of the sun and which do not; secondly, because research was started, by Lord Rayleigh and Ramsay among others, in order to determine the density of nitrogen exactly to the 1/10,000, instead of the precision of 1/1,000 which was known previously.

Both are subjects which were investigated and elucidated without foreseeing any possible applications.

We must add, however, that, conversely, application is useful and eventually essential to theory by the very fact that it opens new questions for the latter. One could say that application's constant relation to theory is the same as that of the leaf to the tree: one supports the other, but the former feeds the latter. Not to mention several important physical examples, the first mathematical foundation in Greek science, geometry, was suggested by practical necessity, as can be seen by its very name, which means "land-measuring."

But this example is exceptional in the sense that practical questions are most often solved by means of existing theories: practical applications of purely scientific discoveries, important as they may be, are generally remote in time (though, in recent years, this delay may be considerably shortened, as happened in the case of radio telegraphy, which occurred a few years after the discovery of Hertzian waves). It seldom happens that important mathematical researches are *directly* undertaken in view of a given practical use: they are inspired by the desire which is the common motive of every scientific work, the desire to know and to understand. Therefore, between the two kinds of invention we have just distinguished from each other, mathematicians are accustomed only to the second one.

The Choice of Subjects. But setting aside practical applications, which generally, if they exist, lie far away in time, mathematical discoveries can be more or less rich in theoretical consequences. Even these are most often unknown to us, as fully unknown as incombustible balloons were to the men who, for the first time, discovered the chemical composition of the atmosphere of the sun.

Then, how are we to select subjects of research? This delicate choice is one of the most important things in research; according to it we form, generally in a reliable manner, our judgment of the value of a scientist.

Upon it we base even our judgment of research students. Students have often consulted me for subjects of research; when asked for such guidance, I have given it willingly, but I must confess that—provisionally, of course—I have been inclined to classify the man as second rate. In a different field, such was the opinion of our great Indianist Sylvain Levi, who told me that, on being asked such a question, he was tempted to reply: Now, my young friend, you have attended our courses for, say, three or four years and you have never perceived that there is something wanting further investigation?

But how is that important and difficult choice to be directed? The answer is hardly doubtful: it is the same which Poincaré gave us concerning the means of discovery, the same for the "drive" as for the "mechanism." The guide we must confide in is that sense of scientific beauty, that special esthetic sensibility, the importance of which he has pointed out.

As Renan also curiously notices,[1] there is a scientific taste just as there is a literary or artistic one; and that taste, according to individuals, may be more or less sure.

Concerning the fruitfulness of the future result, about which, strictly speaking, we most often do not know anything in advance, that sense of beauty can inform us and I cannot see anything else allowing us to foresee. At least, contesting that would seem to me to be a mere question of words. Without knowing anything further, we *feel* that such a direction of investigation is worth following; we feel that the question *in itself* deserves interest, that its solution will be of some value for science, whether it permits further applications or not. Everybody is free to call or not to call that a feeling of beauty. This is undoubtedly the way the Greek geometers thought when they investigated the ellipse, because there is no other conceivable way.

As to applications, though completely unforeseen, they do most often arise later on, if our original feeling has been a right one. I shall report one or two personal instances, apologizing for that repeated intervention of my own example on which, of course, I am especially informed.

When I presented my doctor's thesis for examination, Hermite observed that it would be most useful to find applications. At that time, I had none available. Now, between the time my manuscript was handed in and the day when the thesis was sustained, I became aware of an important question (the one we have spoken of at p. 118 in con-

[1] *L'Avenir de la Science*, p. 115.

nection with Riemann) which had been proposed by the French Academy of Sciences as a prize subject; and precisely, the results in my thesis gave the solution of that question. I had been uniquely led by my feeling of the interest of the problem and it led me in the right way.

A few years later, having, in a further study of the same kind of questions, obtained a very simple result[2] which seemed to me an elegant one, I communicated it to my friend, the physicist Duhem. He asked to what it applied. When I answered that so far I had not thought of that, Duhem, who was a remarkable artist as well as a prominent physicist, compared me to a painter who would begin by painting a landscape without leaving his studio and only then start on a walk to find in nature some landscape suiting his picture. This argument seemed to be correct, but, as a matter of fact, I was right in not worrying about applications: they did come afterwards.

Some years before (1893), I had been attracted by a question in algebra (on determinants). When solving it, I had no suspicion of any definite use it might have, only *feeling* that it deserved interest; then in 1900 appeared Fredholm's theory,[3] for which the result obtained in 1893 happens to be essential.

Most surprising—I should say bewildering—facts of that kind are connected with the extraordinary march of contemporary physics. In 1913, Elie Cartan, one of the first among French mathematicians, thought of a remarkable class of analytic and geometric transformations in

[2] For technicians, the "composition theorem."

[3] This is the theory which, as said in Section IV, I failed to discover. It has been a consolation for my self-esteem to have brought a necessary link to Fredholm's arguments.

relation to the theory of groups. No reason was seen, at that time, for special consideration of those transformations except just their esthetic character. Then, some fifteen years later, experiments revealed to physicists some extraordinary phenomena concerning electrons, which they could only understand by the help of Cartan's ideas of 1913.

But hardly any more typical instance in that line can be set forth than modern functional calculus. When Jean Bernoulli, in the eighteenth century, asked for the curve along which a small heavy body would go down from a point A to a point B in the shortest possible time, he was necessarily tempted by the beauty of that problem, so different from what had been attacked hitherto though evidently offering an analogy with those already treated by infinitesimal calculus. That beauty alone could tempt him. The consequences which "calculus of variations"—i.e., the theory of problems of that kind—would carry for the improvement of mechanics, at the end of the eighteenth century and the beginning of the nineteenth, could not be suspected in his time.

Much more surprising is the fate of the extension given to that initial conception in the last part of the nineteenth century, chiefly under the powerful impulse of Volterra. Why was the great Italian geometer led to operate on functions as infinitesimal calculus had operated on numbers, that is to consider a function as a continuously variable element? Only because he realized that this was a harmonious way of completing the architecture of the mathematical building, just as the architect sees that the building will be better poised by the addition of a new wing. One could already imagine that, as explained in Section III, such a harmonious creation could be of help for solving problems concerning functions considered in the previous fashion; but that "functionals," as we called the new conception, could be in direct relation with reality could not be thought of otherwise than as mere absurdity. Functionals seemed to be an essentially and completely abstract creation of mathematicians.

Now, precisely the absurd has happened. Hardly intelligible and conceivable as it seems, in the ideas of contemporary physicists (in the recent theory of "wave mechanics"), the new notion, the treatment of which is accessible only to students already familiar with very advanced calculus, is absolutely necessary for the mathematical representation of any physical phenomenon. Any observable element, such as a pressure, a speed, etc., which one used to define by a number, can no longer be considered as such, but is mathematically represented by a functional!

These examples are a sufficient answer to Wallas's doubt on the value of the sense of beauty as a "drive" for discovery. On the contrary, in our mathematical field, it seems to be almost the only useful one.

We again see how direction in thought implies affective elements, such being especially the case as concerns that continuity of attention, that faithfulness of the mind to its object, the importance of which we have already pointed out in Section IV.[4]

In the present stage, as in inspiration, choice is directed by the sense of beauty; but, this time we refer to it con-

[4] In a question of inversive geometry (see Section IV), I had underestimated the beauty of the question and failed to devote to it a sufficient continuity of attention.

sciously, while it works in the unconscious to give us inspiration.

Direction of Inventive Work and Desire of Originality. May other reasons influence the direction of research?

As Dr. de Saussure rightly observes, the intervention of emotional causes is often possible (he gives me typical examples in the life of Freud, the creator of psychoanalysis). However, this chances to be less the case as concerns mathematics, on account of the abstract character of that science where, according to Bertrand Russell's celebrated word: "We never know what we are talking about, nor whether what we are saying is true."

Dr. de Saussure has also raised the question whether creative workers could not be moved by a less laudable kind of passion, deriving from human vanity: the desire of doing something unlike others.

It seems to me that something of this kind is possible in art or literature. More exactly, any question of vanity being set aside, not being similar to others is a requisite which the artist (or similarly, the literary man) must consider in itself. Of course, this observation does not apply to the really great ones: for instance, we have seen by Mozart's letter (p. 17) that he did not have to think of being original. But did not such a necessity have its part in the founding of some schools of painters; or in the works where some literary men try to interpret in a paradoxical way the actions or psychology of known personalities? One may ask that question.

We might see some connection between this and a certain number of known cases where poets or other artists have produced works in abnormal states (for instance, Coleridge in a state of laudanum-sleep). Wallas,[5] who reports such examples, considers that a slight degree of "dissociation of mind" may be useful for the artist "who wishes to break with his own habits of thought and vision and those of his school." Also, it is not unusual to hear of poetical works composed in dreams, while we have seen that this is very rare, if not doubtful, in mathematical production.

The case of the scientist who, as has been said in the beginning, is a servant and not a master, is indeed a different one. Any result, the solution of any problem he knows of, makes new problems arise before him. As a matter of fact, I can hardly think of more than two or three memoirs which I would describe as bizarre rather than as truly original.

Nevertheless, the scientist may be and often is discouraged from studying such and such a problem not by the knowledge that it has been solved, but by the fear that it has been solved without his knowing it, a fact which would render his work useless; or—and this is more disinterested on his part—it is natural for him to be attracted by a question not devoid of importance in itself on account of its having been overlooked until then. Such has often been my case; I even add that, after having started a certain set of questions and seeing that several other authors had begun to follow that same line, I happened to drop it and investigate something else. I have been told by physicists that some of the prominent men in contemporary physics often act in the same way.

[5] *The Art of Thought*, pp. 206-210. That very great masters do not need to strive for originality is interpreted by Wallas in saying that, for them, "at the moment of production, a harmony is attained between an intense activity of the whole nervous system, higher and lower alike, and the conscious will."