

SCIENCE

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ON THE NATURE OF MATHEMATICAL AND SCIENTIFIC DEMONSTRATION¹

IN the development of every science there is a growth of method as well as of results. We are accustomed to give close attention to the latter, and frequently we reorganize them into connected and logical wholes so that every student may conveniently view them in their entirety and in their proper relations to one another. In determining the method by which the matter shall thus be organized we are generally guided by considerations of convenience in exposition.

In much of our teaching, likewise, the selection and arrangement of material is determined primarily by a desire to arrive at results in the most expeditious manner possible.

One effect of this controlling emphasis, both in lecturing and in the writing of books, is that many of us never come to a proper appreciation of the labor which has been expended in perfecting our tools of investigation and never have a vital conception of the character of the important problem of method. Such a person usually will be able to employ only the tools which are presented to him by others. He will not be able to devise a new method to meet the needs of the new problem which arises in his own work.

Now the most important steps forward are made by the introduction of new methods of advancement. It is obvious that the person most likely to discover the

¹ An address delivered on the evening of October 6, 1913, to "The Euclidean Circle," an organization among the graduate and undergraduate students of mathematics in Indiana University.

new method is the one who understands best the fundamental ideas on which the methods of his subject are based and the relation of these ideas and methods to corresponding ones in allied fields of study.

It is, therefore, important to the student of every science to analyze the growth of method in his science and to ascertain the fundamental basis on which it has developed. This analysis requires a wider grasp of the subject than the student can possess in the early years of his labor. But he can appreciate, to a large extent, the results of such an analysis and profit by a knowledge of them, if they are presented by some one of a fuller experience than himself.

It is my purpose this evening to present to you the outcome of such an analysis of the nature of mathematical and of scientific demonstration.

A method which was considered useful and legitimate in one generation has often been discarded in the next. Sometimes it has been replaced by another which was merely more powerful and at least equally convenient. At other times it has been found to be not a legitimate method; and it has been necessary to abandon it because investigators could no longer be sure of results obtained by means of it. This has been true both of mathematics and of experimental science, but less frequently of the former than of the latter.

For a mathematical method a first requisite is that the mind shall assert with the strongest emphasis that the method is legitimate. We shall say nothing about how this conviction may have arisen: we shall first demand of it only that it shall be a profound and universal conviction of the human mind.

I shall illustrate what I mean here by an example. Let us take the principle or method of mathematical induction. It is

convenient to consider a particular case of its use. Suppose that we wish to demonstrate the binomial theorem,

$$(a + b)^n = a^n + na^{n-1}b + \dots + nab^{n-1} + b^n,$$

for every positive integer exponent n . Our method of procedure is as follows: We first observe that the theorem is true for n equal to 1. The next step is to prove that if it is true for n equal to k , where k is any positive integer, it is likewise true for n equal to $k + 1$; and we shall suppose now that this step has been made by the necessary argumentation. Now we know that the theorem is true for n equal to 1; from the result last mentioned we conclude further that the theorem is true for n equal to 2. Since it is true for n equal to 2 we may apply our previous result again and conclude that it is true for n equal to 3. Likewise we proceed to the case when n is equal to 4; and so on.

Now, if one analyzes the principle on which this argument is based, the conclusion comes home to him with a compelling force; and he can not fail to have confidence in it. He has verified the theorem perhaps in only a few cases; but he has no fear that a case will ever be found to contradict it.

The first requirement of a mathematical method, as I have said, is that it shall possess just this property of compelling confidence in the conclusions reached by its means. The ground of this compelling power in the method the mathematician (as such) does not seek to find; that is a problem for the philosophers.

But such credentials as those mentioned, however good they may appear to be, are never accepted by the mathematician as entirely satisfactory. He does not, indeed, dispute their legitimacy. But, through much experience, he has found that methods exist concerning which the uninitiated

mind asserts emphatically that they are valid, whereas he knows cases in which they lead to inconsistent results.

Therefore these credentials are treated by the mathematician as affording him only a means of making a first choice of methods to be examined. They are still to be subjected to tests in the laboratory of the mind.

You may ask: To what sort of test may one conceivably subject a method which the mind approves with as much confidence as it does that of mathematical induction, for instance? There seems to be just one such test available. Does it always lead to consistent results? I do not say true results; for there is no one to determine whether the results are true. If several methods are involved at once, it is to be demanded of them also that the results obtained by means of any of them shall be consistent with those obtained from others.

Effectively, what the mathematician does, then, is to select a number of methods in the intuitional way which I have indicated and then to subject them to the most exacting requirements in the way of consistency of results obtained by their use—results exact in their nature and deduced from exact data and covering a wide range of thought.

The only methods which he retains after these extended tests are those which have never been known to lead to a contradiction at any time in the history of human thought. One other analysis must finally be made before they can be admitted into the privileged circle of mathematical methods. It must be ascertained of a given method whether it is perfectly precise in its nature in the sense that no two persons of intelligence have a different opinion as to what the method is. There is no disagreement, for instance, among

thinkers concerning the definition of mathematical induction.

Once the mathematician has selected some methods which he is willing to employ, he uses them in argument in the coldest and most formal way. In making discoveries intuition plays a most important rôle and is a precious guide which he can not dispense with. But when he states his proofs he does it in terms which are entirely free from intuition. Further, he is careful to make sure that he has used no methods except those which have already successfully passed his most searching scrutiny. Through sore experience he has learned that safety lies in no other direction.

But this is not all. Every new use of his methods gives rise to the possibility at least that a contradiction has crept in through some argument which has never before led into such error; and this possibility must be examined—certainly in all cases where the research opens up a new field of thought, if not also in the more common investigations.

It is due to this extreme carefulness on the part of the mathematician that we have so strong a feeling of certainty in his conclusions. But if we analyze this feeling with care we shall find, unexpectedly perhaps to most of us, that it is due after all to our experience with the methods employed, since under the most severe tests they have never led us into contradiction. (They are the only methods which possess this latter property.)

If you will recall what I said about the way in which the mathematician has selected his tools of investigation, you will see why he can never be absolutely sure that he has employed a proper procedure in argument. At no stage in the development of his method was there an absolute criterion according to which a method was to be

retained. He proceeded entirely by exclusion. First, all conceivable methods which did not come up to a certain standard were put aside. Those that remained were subjected to further tests, one after another, and some of them were found to be unsatisfactory. Those left over were finally retained because they had the negative recommendation of never having been caught in an act of deception.

What shall we say then of the certainty of mathematical doctrine at the present day? To answer this question, let us observe that, in all preceding generations, methods in mathematics have been used with confidence which, in the experience of a later day, were found to be not legitimate; they have been discarded, sometimes after generations of confident use. It is not likely that men have heretofore always made mistakes of this kind and that we have suddenly come upon an age in which mathematical methods are certain in the absolute sense.

We are then forced to the conclusion, however unwelcome it may be, that the certainty of mathematics is after all not absolute, but is relative. To be sure, it is the most profound certainty which the mind has been able to achieve in any of its processes; but it is not absolute. The mathematician starts from exact data; he reasons by methods which have never been known to lead to error; and his conclusions are necessary in the sense, and only in the sense, that no one now living can point to a flaw in the processes by which he has derived them.

When we find ourselves forced to this result, our first feeling is probably one of disappointment. But a deeper analysis of the matter will bring us to a different attitude. It gives us a new sense of the problem which lies before us in the development of mathematical thought. We have not

merely to seek new results; but we have also the larger problem of method to inspire our activity and to lead us perhaps to fundamental achievement.

It is conceivable that methods may be devised by means of which we shall attain to well-nigh perfect certainty. Let us suppose that we have found a method of argument, or a principle *A*, which has this property, namely: In whatever way we start from a principle not in accord with it we shall be led into results which are themselves mutually contradictory. Now suppose that principle *A* is itself not a legitimate one. Then there is a legitimate principle *B* not in accord with it. From this new principle we can get mutually contradictory results. That is, principle *B* is both legitimate and not legitimate. This being a contradiction in itself, we conclude that the hypothesis from which it is deduced is false. Therefore principle *A* is legitimate. I say that it is conceivable that such principles *A* will some day be discovered; but they have not yet been found.

In an earlier day, and of course without the aid of such principles as I have just mentioned, men apparently had come to a feeling of absolute certainty about the accuracy of mathematical conclusions. Those fundamental methods of argumentation, of which I spoke in the outset, they conceived to belong to a class of innate or inherent ideas which had been put in the mind of man by the Creator. The initial hypotheses and basic notions of a mathematical discipline they thought of as belonging to the same category. If these innate ideas did not have all the elements of absolute certainty, there could be only one conclusion: the Creator had deliberately deceived man. Since they considered this to be absolutely impossible, they had complete confidence in the certainty of mathematical results.

This is merely one example of the usual dependence of the ancients on the authority of abstract reason. By this means they sought absolute certainty in scientific as well as in mathematical and philosophical thought. A brief account of their general point of view in regard to this matter will serve to connect the two topics which I have asked you to associate together this evening; for it is in the ancient time that the two methods are most closely related.

It is convenient to speak of the position of Plato. This philosopher refers, with a touch of contempt, to one who gives his life to the investigation of nature, feeling that such a person was concerned with the visible universe alone and was immersed in its phenomena. These, whether past or present or to come, admit of no stability and therefore of no certainty. "These things," he says, "have no absolute first principle and can never be the objects of reason and pure science." Plato believed that the senses are deceptive and could never lead to the discovery of truth. The only way to develop science was to look within and find there the fundamental principles on which it should be based; and then to develop logically the consequences of these principles.

But I shall not take up your time with an analysis of these old opinions, however much they may have influenced or retarded science in times past. Neither shall I pause to indicate how the old Greek science, such as it was, came into a place of authority, dominating the thought of many generations and giving rise to a fearful intellectual stagnation. I prefer to come to the time when the development of scientific method began to recover men from their stupor and to kindle a new intellectual light and fervor.

Let me direct your attention to the Italian philosopher Bernardino Telesio

(1509-1588) as the great figure who marks the period of transition from authority and reason to experiment and individual responsibility. He was the forerunner of all subsequent empiricism, scientific and philosophical, sowing the seeds from which sprang the scientific methods of Campanello and Bruno, of Francis Bacon and Descartes and the scientists of our day. He abandoned completely the purely intellectual sphere of the ancient Greeks and other thinkers prior to his time and proposed an inquiry into the data given by the senses. He held that from these data all true knowledge really comes.

The work of Telesio, therefore, marks the fundamental revolution in scientific thought by which we pass over from the ancient to the modern methods. He was successful in showing that from Aristotle the appeal lay to nature; and he made possible the day when men would no longer treat the *ipse dixit* of the Stagirite philosopher as the final authority in matters of science.

It is true that Telesio had been preceded almost three centuries by Roger Bacon (1214?-1294?), a modern thinker in the middle ages, whose conceptions of science were more just and clear than those at a date four centuries after his birth. But this Bacon was a man born out of time, too far in advance of his age to be appreciated by it; and consequently he had but little influence on the growth of scientific method. The balance has now been restored in his favor, so far as the judgment of historians is concerned; but that leaves untouched the facts of effective scientific progress.

Telesio had several followers, or perhaps we should say fellow pioneers, in the same field. Among these Francis Bacon probably stands out as the most prominent of all. He said of himself that he "rang the bell which called the wits together." But his contributions to the stock of actual scien-

tific knowledge were practically inconsiderable. His great merit lay in his making men see that science was in fundamental need of a new method. The method he suggested was not adopted; but his analysis of the need was the signal for the search which has ended in modern science.

I need not take you further through the long history. It is sufficient to my purpose to point out that primitive man first developed by experience a way of his own for observing and fixing in mind external phenomena, that the Greeks seized upon their own and their predecessors' observations and sublimed experience into theory, that Telesio and Bacon and others taught mankind the insufficiency of Greek methods and the need of new ones, and that modern science came into being and fulness of stature through generations of workers who sought to put, and succeeded in putting, the new ideas into the form of effective tools of advancement.

From this brief historical account it is seen that the method of experimental science has itself grown through experiment. The style of argument employed by Plato, for instance, has been entirely superseded by another and better. Man had to learn by the experience of failure how to ascertain the true relations of phenomena. In other words, there was no "preestablished harmony" between the mind and the phenomena it had to interpret of such character as to lead the former to a ready explanation of the latter.

Our progress in this respect has been over a hard and long and rough road. We go a very short distance, relatively, into our past to find the time when methods were uniformly employed in science which are now known to be quite untrustworthy. What is the bearing of this fact on our confidence in the conclusions of science? In order to answer this question properly we

shall have to analyze briefly the general nature of scientific investigation as at present practised.

In the first place, scientific demonstration starts from data which involve the ever-present inexactness which is due to experimental error. In the nature of things it is impossible that the argumentation should ever have an exact basis to rest upon; and consequently all conclusions must again be tested by a direct appeal to phenomena. In another important respect also the method is essentially different from that employed in mathematics. Here intuition is a fundamental guide in argument as well as in discovery; and a "proof" whose leading elements are grounded in intuition is accepted with a confidence at least equal to that which is accorded to one characterized by mathematical precision and rigor.

One result of this inexact basis and especially of this loose method of argumentation is that the conclusions reached often are primarily of the nature of inference from examples. They have little or none of the compelling property which attaches to mathematical conclusions.

In other words, scientific (as opposed to mathematical) truth is not necessary truth. It is in the nature of things that the experimental scientist can not give us absolute truth. This is no criticism of his work; it is not his province to give us absolute truth—even if such a thing were supposed to exist.

What then is the purpose of the experimental scientist? His province is to enable us to get around among the phenomena of the external world, to predict what will happen under a given set of circumstances. He will accomplish this end by studying the relations among phenomena. He does not need to know their ultimate explanation; it is sufficient if he can find the essen-

tial threads of interconnection among them. Therefore he does not seek absolute certainty in his theories, at least when he realizes the fundamental limitations of his methods; but he understands his theories rather as the most convenient means by which he may summarize for himself and others the actually observed interrelations in nature.

Now, let us suppose that an experimental scientist attempts to attain absolute certainty in his conclusions, and enquire as to the kind of difficulty which he will encounter.

An analysis of the matter shows, first of all, that he must make one fundamental assumption—that involved in the hypothesis of the uniformity of nature. If phenomena have no laws it is futile to ascribe laws to them; and therefore a first requisite for the existence of experimental science is the supposition that laws exist. It must be assumed that the universe will not suddenly depart to-morrow from its previous way of behaving; it must not be a thing of caprice.

But what ground have I for believing that to-morrow will not put forth a set of phenomena totally different from those which I have observed before? None at all, except what comes through my belief in the uniformity of nature. It is clear that this is not the way by which the principle is to be established. In fact, we can go further and say with confidence that there is no absolute certainty, but only a high degree of probability, that nature is uniform.

There is also another fundamental assumption at the basis of experimental science—one that is curiously related to the mind that has made the assumption.

A fundamental property of mind is memory; without it mind can not exist in its usual state. What one does to-day is colored, modified, perhaps determined by one's memory of past acts. No experiment

on a thinking subject can be performed for the second time; for the presence of memory in the second event is a factor of determining importance and can not be left out of account.

And yet mind, of which this is a characteristic and fundamental property, has chosen to assume that matter is without memory. If I desire to experiment with a falling stone, I need not enquire whether the stone has gone through the same experience before. In other words, I assume that the stone has no memory of its previous existence; and consequently its previous history will not affect my present experiment.

If it is true that experimental science is so shot through with basic assumptions, what is to be said of our confidence in its results? What measure of certainty attaches to them and how do we come to that certainty? Clearly, the evidence must be indirect; but it need not on that account be less trustworthy.

We may arrive at one phase of this evidence by noticing what change has taken place in man's relation to natural phenomena since the dawn of the modern era in scientific investigation. It is patent to every one that there has been an immense gain in control; man has harnessed the forces of the world and is using them for his purpose. A thousand and one new instruments of power and pleasure attest to his more profound understanding of the relations among phenomena. For hundreds of miles he can transfer the immense power of Niagara along a slender wire, and then use it to run his machinery and light his cities and warm his houses. In every conceivable direction he is making progress decade by decade; and the momentum of his progress increases as the years pass.

But even this is not the chief reason for believing that he is essentially right in his

interpretation of the relations of phenomena. His strongest ground of confidence is in the multiplicity and the accuracy of his predictions—predictions which he verifies by further tests in the laboratory.

Probably the severest test of a physical theory is the requirement that it predict accurately a phenomenon which has not yet been observed; and this is a test to which theory is constantly subjected—and it comes out successful. This is the ground of our confidence in physical theories. It is this which lends the strongest possible credence to such a general hypothesis, for instance, as that of the uniformity of nature.

This ultimate test of prediction finds its most extensive exemplification in the results obtained by the apparatus of abstract mathematical ideas. From a few fundamental laws, as for instance those of static electricity, an immense body of doctrine is built up by the processes of mathematical analysis. The results so obtained are exact and are stated with careful precision. Notwithstanding their great variety and the absolute precision with which they are stated, they are found to be always in accord with new experiment however the conditions may be varied. It is this which furnishes our strongest ground of confidence in physical theory; it is not the argumentation or inference by which the theory was first discovered or created.

The success of this prediction through mathematical or other argumentation is so great that we can not escape the conclusion that science is on the right track; improvements will come, to be sure, but we have certainly made some fundamental progress. In fact, the ground for this conclusion is so strong that the burden of proof must rest on whoever disputes its validity. If our theories are essentially erroneous, it requires careful explanation

to understand why our attempt to put them in mathematical language has issued in such a remarkable success in the way of relating and predicting phenomena.

Even though we are still left face to face with the conclusion that there is no absolute certainty in our scientific theories, we see nevertheless that our ground of confidence in them is such as to justify our laying out our life and its activity as if they were so. We shall accept them as our guide in getting around among external phenomena. And we can do this even with more confidence than we can plan those things which depend on our own acts. Indeed there is much greater certainty attaching to the prediction of physical phenomena than to the prediction of our own acts; and what more could one reasonably demand of science?

Now of the two methods which we have considered, the mathematical and the experimental-scientific, which is the better? You will probably expect me to say that the mathematical method is the better; but I do not say it. Neither is the better; the question is meaningless. Each method is of profound importance and each is suited to its proper purposes; each will be improved as time passes and will be carried over more and more into all fields of thought and conduct; and each will continue to add new conquests to human achievement. But we shall not say that one is better than the other.

Most of you to whom I have spoken this evening are at the threshold of life. The future lies before you. You will doubtless choose some definite work to do in it. Would you like to have a part in promoting those fundamental ends of human development which may be secured through the use of one or the other of these great methods of advancement?

But what is it to have a part in using

and perfecting these tools, the two chief means by which mankind is making progress in our day? What sort of work is it? It is hard; it is no child's play; it is the work of maturity and strong purpose. The material rewards are few; probably not many of your generation will appreciate your labors, and most of you perhaps will not be heard of after your day. But you will leave mankind a heritage of profit forever, you will hasten the day when all men will know that their chief benefactors are those who delve into the secrets of nature and reveal them to their fellows. Does that work appeal to you?

R. D. CARMICHAEL

RECOLLECTIONS OF DR. ALFRED RUSSEL WALLACE

It is impossible for any man to discuss adequately the life work of Alfred Russel Wallace. His activities covered such a long period, and were so varied, that no one living is in a position to critically appreciate more than a part of them. We are very much interested, of course, and have our opinions; but we need not pretend to any final or complete judgment. All must agree that a great and significant career has just been closed, but its full measure will probably never be known to any single man.

On the other hand, it may be possible to gain a clear idea of the character and aims of Dr. Wallace; and for our purposes this is perhaps the more important thing, since his guiding principles may also become ours, while the work he did is his alone. I once asked him about the origin of his interest in biology, and in the course of his reply¹ he said: "As to my interest in biology, . . . I doubt if I had or have any *special* aptitude for it, but I have a natural love for *classification* and an inherent desire to *explain things*; also a great love of beauty of form and color." Again, in writing to the biology students of the University of Colorado, he said:²

¹ *Popular Science Monthly*, April, 1903, p. 517.

² *SCIENCE*, March 29, 1912, p. 487.

The wonders of nature have been the delight and solace of my life. . . . From the day when I first saw a bee-orchis in ignorant astonishment . . . nature has afforded me an ever-increasing rapture, and the attempt to solve some of her myriad problems an ever-growing sense of mystery and awe.

This is the spirit of the amateur, using that word in its best and true sense. When Wallace had been long in the Malay Archipelago, a relative wrote urging him to return, and in his reply he gave the reasons why he could not do so, and said:

So far from being angry at being called an enthusiast (as you seem to suppose), it is my pride and glory to be worthy to be so called. Who ever did anything good or great who was not an enthusiast?

This was his attitude to the end of his life, and only those who have some measure of the same feeling can understand it. The worldly wisdom of a professional threading his way through the maze of opportunity to one of the prizes of life was wholly foreign to his nature; he was, instead, the "irresponsible enthusiast," keenly anxious to see and know, *loving* nature and man, always wishing to communicate to others some of the pleasure and knowledge he had gained. To some his frequent advocacy of unpopular causes suggested perfect indifference to public opinion, and a total disregard of ordinary prudence. Whether, in this or that matter, we believe him to have been right or wrong, we must admire a man who always had the courage of his convictions; and so far from being indifferent to the feelings and opinions of others, his sympathetic nature and longing for fellowship *caused* him to so zealously expound what he believed would be helpful to other men.

I had of course revelled in "The Malay Archipelago" when a boy, but my first personal relations with Dr. Wallace arose from a letter I wrote him after reading his "Darwinism," then (early in 1890) recently published. The book delighted me, but I found a number of little matters to criticize and discuss, and with the impetuosity of youth, proceeded to write to the author, and also send a letter on some of the points to *Nature*. I have