Tradition in Science

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TRADITION IN SCIENCE

One of the originators of twentieth century physics, Werner Heisenberg, traces the influence of historical tradition on the selection of problems, the scientific method and the use of concepts

When we celebrated last year the 500th birthday of Copernicus, we did it because we believe that our present science is related to his work and that the direction which he had chosen for his research in astronomy still determines to some extent the scientific work of our time. We are convinced that our present problems, our methods, our scientific concepts are, at least partly, the results of a scientific tradition which accompanies or leads the way of science through the centuries. It is therefore natural to ask to what extent our present work is determined or influenced by tradition. Are the *problems* in which we are engaged freely chosen according to our interest or inclination, or are they given to us by a historical process? To what extent can we select our scientific *methods* according to the purpose; to what extent do we again follow a given tradition? And finally how free are we in choosing the *concepts* for formulating our questions?

Any scientific work can only be defined by formulating the questions which we want to answer. But to formulate the questions we need concepts by which we hope to grasp the phenomena. These concepts usually are taken from the past history of science and already suggest a possible picture of the phenomena. But if we are going to enter into a new realm of phenomena, these concepts may act as a collection of prejudices which hamper progress rather than foster it. Even then we have to use concepts and we cannot help falling back on those given to us by tradition. I will try to discuss the influence of tradition first in the selection of problems, then in the scientific methods and finally in the use of concepts as tools for our work.

Historical development

To what extent are we bound by tradition in the selection of our problems? When we look back into the history of science, we see that periods with intense activity change with long periods of inactivity. In ancient Greece the philosophers started asking questions of principle with respect to phenomena in nature. There had been a considerable practical knowledge long before; great skill had been developed in building houses, cutting and moving big stones, constructing ships and so on; but it was only in the period after Pythagoras that this skill was supplemented by scientific inquiry.

The relevance of mathematical relationships in natural phenomena was discovered by Pythagoras and his pupils and a great development in mathematics, astronomy and natural philosophy followed. The decline of Greek science after the Hellenistic period, after Ptolemeus had succeeded great astronomers like Aristarchus and Hipparchus, marked the beginning of a long period of inactivity which lasted until the renaissance in Italy. During this period of stagnation, an admirable development of practical knowledge led to a high civilization in the Arabian countries; but it was not accompanied by a corresponding development in science or by a deeper understanding of nature.

More than 1000 years later, when humanism and renaissance had shown the way towards a more liberal trend of thought, when the explorers had demonstrated the possibility of expansion on our earth, a new activity in science was inaugurated by the discoveries of Copernicus, Galileo and Kepler. This activity has lasted until the present time and we do not know whether it will continue for much longer or whether it will give way to a new period in which the interest will go in a very different direction.

Looking back upon history in this way we see that we appear to have little freedom in the selection of our problems. We are bound up with the historical process. Our lives are part of this process and our choice seems to be restricted to the decision whether or not we want to participate in a development that takes place in our time with or without us. Without such a favourable development, our activity would probably be lost. If Einstein had lived in the 12th century, he would have had very little chance of becoming a good scientist.

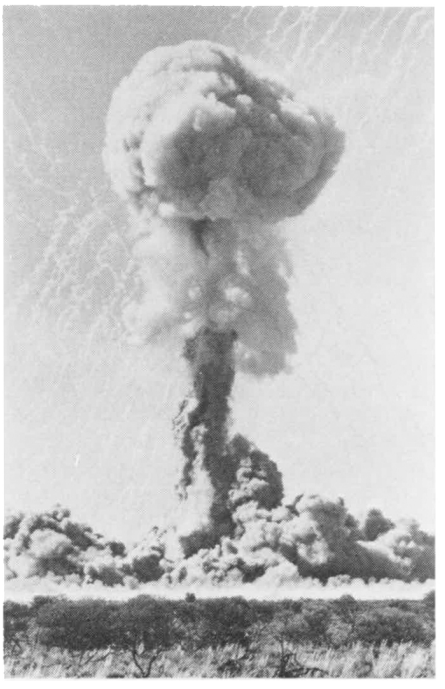
Even within such a fruitful period as ours the scientist has not much choice in selecting his problems. On the contrary one may say that a fruitful period is characterized by the fact that the problems are given; we do not need to invent them. This seems to be true in science as well as in art. When in the 15th century the painters in the Netherlands discovered the possibility of portraying men as active members of their society, many gifted people were attracted by this possibility and competed in solving this given problem. In the 18th century Haydn tried to express in his string quartets those emotions that had become visible in the literature of his time in the work of Rousseau and in Goethe's *Werther*. Then the musicians of the younger generation – Mozart, Beethoven, Schubert – gathered in Vienna competing in the solution of this problem.

In our century the development of physics led Niels Bohr to the idea that Rutherford's experiments on α rays, Planck's theory of radiation and the facts of chemistry could be combined in a theory of the atom. In the following years many young physicists went to Copenhagen to participate in the solution of this problem. One cannot doubt that in the selection of problems the tradition – the historical development – plays an essential role.

This may also sometimes be true in a negative sense. Sometimes traditional themes become exhausted and gifted people turn away from a field in which they see no more outlets for their activity. After Thomas Aquinas, philosophers became tired of the theological and philosophical problems of scholastics and turned to humanism. In our time the traditional themes of art seem to be exhausted. Last year one of the most popular yearly exhibitions of modern art in Germany, called 'Documenta' in the city of Kassel, was a centre of political propaganda rather than art. On the outside of the building of the exhibition the young artists had fixed a huge poster with the text: 'art is superfluous'. In a similar way we cannot exclude the possibility that the themes of science and technology will eventually become exhausted and a younger generation will tire of our rationalistic and pragmatic attitude and will turn their interest to an entirely different activity. At present many problems still remain in pure and in applied science. No effort is needed to invent them and they will be passed on from the teachers to their pupils.

Personal relationships

In this connection it is important to emphasize the role of personal relationships in the development of science or art. It need not only be the relationship between



Two illustrations of the application of science. **a** The mathematical bridge at Queen's College, Cambridge, which is supposed to have been built by Newton. Legend has it that it was originally constructed without nuts or bolts (*photograph courtesy Cambridge Evening News*); **b** One of Britain's atomic explosions (*photograph courtesy UKAEA*)

teacher and pupil; it may simply be personal friendship or respect between people working for the same goal. This is probably the most efficient instrument of tradition. Among the many examples which could be mentioned for this kind of tradition I will recall only some of the personal relationships which have shaped the history of physics in the first half of this century.

Einstein was well acquainted with Planck; he corresponded with Sommerfeld about the theory of relativity and about quantum theory; he was a near friend of Max Born though he could never agree with him on the statistical interpretation of quantum theory; and he discussed with Niels Bohr the philosophical implications of the uncertainty relations. A large part of the scientific analysis of those extremely difficult problems arising from relativity and quantum theory was carried out in conversations between those who took an active part in the research.

Sommerfeld's school in Munich was a centre of research in the early 1920s; Pauli, Wentzel, Laporte, Lenz and many others belonged to this group, and we discussed almost daily the difficulties and paradoxes in the interpretation of recent experiments. When Sommerfeld received a letter from Einstein or Bohr, he read the important parts of the letter in the seminar and immediately started a discussion on the critical problems. Niels Bohr had close connections with Lord Rutherford, Otto Hahn and Lise Meitner, and he considered the continuous exchange of information between experiment and theory as a central task in the progress of physics. The enormous influence of Niels Bohr on the development of physics in his time was not primarily due to his papers, but to his way of discussing again and again with his partners the fundamental difficulties of quantum theory – which he knew did not allow for any easy solution.

When wave mechanics was introduced by Schrödinger, Bohr saw at once that this was a very important new aspect of quantum theory, but that a simple replacement of the electronic orbits in the atom by three dimensional matter waves could not solve the real difficulties. Again the only way of analysing the problem seemed to be by personal discussion with the author. Schrödinger was invited to Copenhagen, and in two weeks of the most intense discussions the way was prepared for the later development in the interpretation of quantum theory, for Bohr's concept of complementarity and for the uncertainty relations. I need not enlarge upon these examples. It is obvious that the personal relationships play a decisive role in the progress of science and in the selection of problems.

Applicability

Other motives for the selection of problems have also played their role in the history of science. The best known of these motives is the practical applicability of science. In ancient times the interest in astronomy and mathematics was stimulated by the fact that knowledge in these fields was helpful for navigation and for the surveying of land. Navigation played a very important role in the 15th century when the explorers left Europe and the Mediterranean and went westwards. It is certainly no mere accident that the discovery of Copernicus occurred shortly after the beginning of this period.

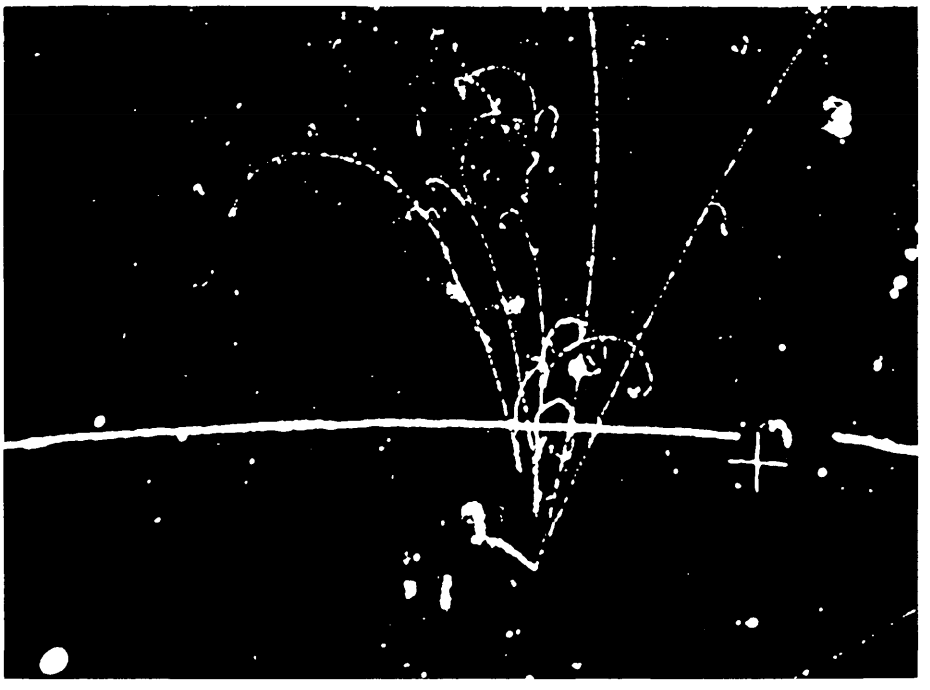
When Galileo defended the ideas of Copernicus he made use of a newly invented instrument, the telescope, thereby demonstrating that the practical tool may be helpful in the progress of science and science may be helpful in leading to the invention of practical tools. Galileo and his followers were strongly interested in the practical side of science. They studied mechanical devices (eg the mechanical clock); they invented optical instruments; Newton constructed a bridge crossing the river Cam in Cambridge and so on. Part of the tradition in science guiding the activity of many generations has always been that science should be applied to practical purposes and that the practical application should be a check on the validity of the results and a justification for the efforts in science. Therefore the atomic physicists of the first half of this century just followed this old tradition of science when they looked for practical applications of atomic physics. It was extremely disappointing for them that the first practical application was for warfare. Still the fact that one now could transmute chemical elements into others in large quantities was justly considered as a real triumph of science.

Individual motives

This interest in the practical application of science is frequently misunderstood as the trivial attempt of the scientist to acquire economic wealth. It is true that this trivial motive does play a role sometimes, depending on the individual. But it should not be overestimated. There is another much stronger motive which fascinates the good scientist, namely to see that it works and that nature has been understood correctly. I remember a conversation with Enrico Fermi after the war, a short time before the first hydrogen bomb was to be tested in the Pacific. We discussed this plan and I suggested that perhaps one should abstain from such a test considering the biological and political consequences. Fermi replied: 'But it is such a beautiful experiment'. This is probably the strongest motive behind the applications of science: the scientist needs

the confirmation from an impartial judge, from nature itself, that he has understood its structure. And he wants to see the effect of his effort.

From this attitude one can easily understand the motives which determine the line of research for the individual scientist. It is usually based on some theoretical ideas or on conjectures concerning the interpretation of known phenomena or the hope of finding new ones. But which ideas are accepted? Experience shows that it is not usually the consistency or the clarity of an idea which makes it acceptable, but the hope that one can participate in the elaboration and verification. It is the wish for our own activity, the hope for results from our own efforts which leads us on our way through science. This wish is stronger than our rational judgment about the merits of various theoretical ideas. In the early 1920s we knew that Bohr's theory of the atom could not be quite correct. But we guessed that it pointed in the right direction and we hoped that we would be able some day to avoid the inconsistencies and to replace Bohr's theory by a more satisfactory picture.



When we look at the tracks in a bubble chamber how do previous concepts influence what we see? A bubble chamber photograph 'proving' the existence of neutral currents (*photograph courtesy CERN*)

Scientific method

But the role of tradition in science is not restricted to the selection of problems, and this brings me to the second part of my article. Tradition exerts its full influence in deeper layers of the scientific process where it is not so easily visible; and here we should first of all mention the scientific method. In the scientific work of this century we still follow essentially the method that had been discovered and developed by Copernicus, Galileo and their successors in the 16th and 17th century. This method is sometimes misunderstood and called empirical science and contrasted with the speculative science of former centuries.

In fact Galileo turned away from the traditional science of his time which was based on Aristotle and took up the philosophical ideas of Plato. He replaced the descriptive science of Aristotle by the strutural science of Plato. When he argued for experience he meant experience illuminated by mathematics. Galileo, as well as Copernicus, had understood that by moving away from immediate experience, or by idealizing experience, we may discover mathematical structures in the phenomena and thereby gain a new simplicity as a basis for a new understanding. Aristotle, for example, had stated correctly that light bodies fall more slowly than heavy bodies. Galileo claimed that all bodies fall with the same speed in empty space and that their fall can be described by simple mathematical laws. Fall in empty space could not be observed accurately in his time but Galileo's claim suggested new experiments. The new method did not aim at the description of what was visible, but rather at the design of experiments and the production of phenomena that are not normally seen and at their calculation on the basis of mathematical theory.

Two features are essential for the new method. The attempt to design new and very accurate experiments which idealize and isolate experience and thereby create new phenomena; and the comparison of these phenomena with mathematical constructs, called natural laws. Before we discuss the validity of this method even in science today, we should perhaps briefly look at the basis of confidence which led Copernicus, Galileo and Kepler on this new way. Following a paper of Weizsäcker I think we have to say that this basis was mainly theological. Galileo argued that nature, God's second book (the first one being the *Bible*) is written in mathematical letters and that we must learn this alphabet if we want to read it. Kepler is even more explicit in his work about world harmony; he says: 'God created the world in accordance with his ideas of creation. These ideas are the pure archetypal forms which Plato terms ideas and they can be understood by man as mathematical constructs. They can be understood by man, because man was created as the spiritual image of God. Physics is reflection on the divine ideas of creation, therefore physics is divine service.'

We are now very far from this theological foundation or justification of physics but we still follow this method because it has been so successful. The essential basis for its success was the possibility of repeating the experiments. We can finally agree about their results because we have learnt that experiments carried out under precisely the same conditions do lead to the same results. This is not at all obvious. It can only be true if the events follow exactly a sequence of cause and effect. Because of its success this kind of causality has been accepted over the years as one of the fundamental principles of science. The philosopher Kant has stressed the point that causality in this sense is not an empirical law but part of our method of science. It is the condition for the kind of science which was inaugurated in the 16th century and has been elaborated on ever since.

A consequence of this attitude in science is the assumption that we study nature as it 'really is'. We imagine a world which exists in space and time and which follows its natural laws independent of any observing subject. Therefore in observing phenomena we take great care to eliminate any influence due to the observer. When we produce new phenomena by means of our experimental equipment we are convinced that we do not really produce new phenomena but that these phenomena occur frequently in nature without our interference. Our equipment is just made to isolate and to study them. In all these points we still follow confidently in the tradition of Copernicus and Galileo.

Yet, are we really entitled to do so considering the well known epistemological difficulties of quantum theory? For example in the big accelerators we study the collisions between elementary particles and imagine that, even if we had not built the accelerators, such phenomena would occur in the atmosphere on account of the cosmic radiation. But would there be waves or particles coming from the outside; would they produce interference patterns or tracks? What does actually happen when we do not observe and do we know what the word 'actually' means in this context? These are hard questions and we see that tradition can lead us into difficulties.

Empirical science

It is generally believed that our science is empirical and that we draw our concepts and our mathematics from the empirical data. If this was the whole truth we should, when entering into a new field, introduce only such quantities that can be observed

directly, and formulate natural laws only by means of these quantities. When I was a young man I believed that this was just the philosophy which Einstein had followed in his theory of relativity. Therefore I tried to do a corresponding step in quantum theory by introducing matrices. But when I later asked Einstein about it, he answered: 'This may have been my philosophy but it is nonsense all the same. It is never possible to introduce only observable quantities in a theory. It is the theory which decides what can be observed.' What he meant by this remark was that when we go from the immediate observation – a black line on a photographic plate or a discharge in a counter – to the phenomena we are interested in, we must make use of theory and theoretical concepts. We cannot separate the empirical process of observation from the mathematics and its concepts. The most conspicuous demonstration of this thesis by Einstein were the uncertainty relations.

But this new situation in quantum theory does not necessarily question the traditional method in science; it only questions the assumption that concepts and mathematical constructs can be taken simply from experience. It is true that in quantum theory we cannot rely on strict causality. But by repeating the experiments many times we can finally derive from the observations statistical distributions and by repeating such a series of experiments we can arrive at objective statements concerning these distributions. This is a standard method in particle physics which may be considered a natural extension of the traditional method.

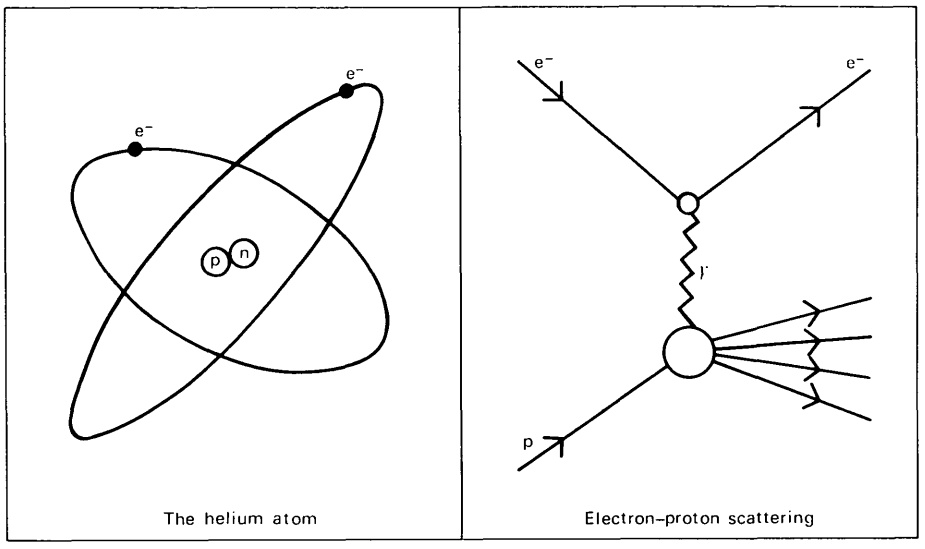
So finally it seems that with regard to the scientific method we follow strictly the tradition inaugurated in the time of Galileo. In spite of the many different fields which have been developed – physics, chemistry, biology, atomic and nuclear science, *etc* – the fundamental method has always been the same. One has the impression that in this period most scientists believed that this was the only acceptable method which could lead to objective, and hence correct, statements concerning the behaviour of nature.

Descriptive science

There has been one attempt to work on an entirely different line which I should mention. The German poet Goethe tried to return to a descriptive science; a science which is interested only in visible natural phenomena not in experiments which produce artificial new effects. He objected to the separation of phenomena into their objective and subjective sides and he was filled with fear of the destruction of nature by an overflowing of technical science. In our time, when we know of the contamination of air and water, the poisoning of the soil by chemical fertilizers, the atomic weapons, we understand Goethe's fear better than his contemporaries could. But Goethe's attempt did not really influence the course of science. The success of the traditional method was too overwhelming.

Concepts

Besides the effect of tradition in the selection of problems and in the scientific method the influence of tradition is perhaps strongest in shaping or passing on the concepts by which we try to grasp a phenomenon. The history of science is not only a history of discoveries and observations; it is also a history of concepts and therefore I will, in this third part of my article, try to discuss briefly the history of concepts during the period following Copernicus and Galileo and the role of tradition in this history.



Particles: are the concepts that were so useful in understanding molecules and atoms still relevant in high energy physics

The new science started with astronomy and therefore the positions and the velocities of bodies were natural first concepts for describing the phenomena. Newton, in his *Mathematical Principles of Natural Philosophy*, added the concepts of mass and force; he introduced the 'quantity of motion', which is essentially what we call momentum, and later such concepts as kinetic and potential energy completed the conceptual basis of mechanics. It remained for more than a century the basis of exact science as a whole and its success was so convincing that whenever phenomena suggested new concepts the scientists tried to follow the tradition and to reduce them to the old ones.

The motion of fluids was pictured as motion of the infinitely many smallest parts of the fluid and their dynamical behaviour was successfully treated according to Newton's laws. When in the second half of the 18th century interest was concentrated on electricity and magnetism the concept of force was used for describing phenomena; and force was meant in the sense of mechanics, a force acting instantaneously and depending only on the positions and the velocities of the bodies concerned. For understanding the different states and the chemical behaviour of matter, Gassendi revived the idea of its atomic constitution and his followers used newtonian mechanics to describe the motion of the atoms and the resulting properties of matter. A beam of light could be considered as consisting of either small quickly moving particles or waves. But even the waves would be waves in some kind of material and one could hope that finally the smallest parts of this material could be treated according to Newton's laws.

Failure of Newton's laws

As in the case of the scientific methods nobody doubted that this reduction to the mechanical concepts could finally be effected. But here history decided otherwise. In the 19th century it gradually became clear that electromagnetic phenomena were of a different nature. Faraday introduced the concept of the electromagnetic field and after the completion of the theory by Maxwell this concept gained more and more reality. Physicists began to understand that a field of force in space and time could be just as real as a position or a velocity of a mass and that there was no point in considering it as a property of some unseen material called 'ether'. Here tradition was more of a hindrance than a help. Even so it was not until the discovery of relativity that the idea of the ether was really given up, and thereby the hope of reducing electromagnetism to mechanics.

A similar development can be seen in the theory of heat but here the alienation from the mechanical concepts can be seen only in rather subtle points. To begin with, everything looked very simple. A piece of matter consists of many atoms or molecules. Statistical considerations about the mechanical motions of these many particles should be sufficient to describe the behaviour of matter under the influence of heat or chemical changes. The concepts of temperature and entropy seemed just adequate to grasp this statistical behaviour.

I think it was Gibbs who first understood what an abyss had been opened up in physics by these concepts. His idea of the canonical ensemble demonstrates that the word 'temperature' characterized our degree of knowledge of the mechanical behaviour of the atoms but not the objective mechanical behaviour. The word refers to a certain kind of observation, namely an exchange of heat between the system and the measuring equipment – the thermometer. It requires a thermodyna-

mical equilibrium. Therefore, if we know the temperature of a system, we cannot know its energy accurately, the inaccuracy depending on the number of degrees of freedom in the system. Of course, tradition worked very strongly against this kind of interpretation, and I believe that the majority of physicists did not accept it finally until quantum theory was completed. But I would like to mention that when I entered Niels Bohr's institute in Copenhagen in 1924, the first thing Bohr demanded was that I should read the Gibbs' book on thermodynamics. And he added that Gibbs had been the only physicist who had really understood statistical thermodynamics.

The clash between the old and new concepts became worse in other fields. In the theory of relativity and in quantum theory we had to learn that some of the oldest traditional concepts did not work satisfactorily and had to be replaced by better ones. Space and time are not as independent of each other as Newton had believed – they are related by the Lorentz transformation. The state of a system in quantum mechanics can be characterized mathematically by a vector in a space of many dimensions. This vector implies statements concerning the statistical behaviour of the system under given conditions of observation. An objective description of the system in the traditional sense is impossible. I need not go into the details. You know how difficult it has been for the physicists to accept these changes in fundamental concepts.

Hindrance or help

Since it is my task to speak about the role of tradition in science, I have to ask whether tradition has been only a hindrance in these developments; whether it has just filled the minds of the scientists with prejudices or preconceptions the removal of which is the most important condition for progress. At this point the problem comes from the word prejudice. When we speak about our investigations, about the phenomena we are going to study, we need a language, we need words, and the words are the verbal expression of concepts. In the beginning of investigations it cannot be avoided that the words are connected with old concepts, because the new ones do not yet exist. Therefore these so called prejudices are a necessary part of our language and cannot be eliminated simply.

We learn language by tradition; traditional concepts form our way of thinking about problems and determine our questions. When the experiments of Lord Rutherford suggested that the atom consisted of a nucleus surrounded by electrons, one could not help asking: what is the location or the motion of the electrons in these outer parts of the atom? What are the electronic orbits? Or when one observed events on very distant stars, it was only sensible to ask: are these two events simultaneous or not? To realize that such questions have no meaning is a very difficult and painful process. It should not be belittled by the word prejudice. Therefore one may say that in a state of science where fundamental concepts are to be changed, tradition is both the condition for progress and a hindrance. Therefore it usually takes a long time before the new concepts become generally accepted.

Let me finally apply these ideas to the present state of physics. In our time the fundamental structure of matter is one of the central problems and the concept of the elementary particle has dominated this problem since the time of Democritos. This can be clearly recognized in our pictures and our questions. A lump matter consists of molecules; a molecule consists of atoms; an atom consists of nucleus and electrons; a nucleus consists of protons and neutrons. A proton – well, that could be an elementary particle. But we would term it 'elementary' only if it could not be divided again. We would then wish that it would be a point of mass and charge. But a proton has a finite size and can be divided. From a collision between two energetic protons many pieces may emerge. But these pieces are not smaller than the proton; they are just particles like the protons: for example some objects out of a whole spectrum of particles, the charge of which – if it is not zero – is not smaller than that of the proton. So what we see in such a collision should perhaps not be called a division of the proton. It is the creation of new particles out of the kinetic energy of the colliding protons.

If the proton is not elementary what does it consist of? Of matter, but matter consists of particles. Hence the proton consists of any number and of any kind of particles, and so on. We see that we do not get a sensible answer to those questions which we have asked and do ask according to the tradition. This tradition goes back 2500 years to the time of Democritos. But we cannot help asking these questions since our language is bound up with this tradition. We must use words like 'divide', or 'consist of', or 'number of particles', and at the same time we learn from the observations that these words have only a very limited applicability.

Again it is extremely difficult to get away from the tradition. In one of the most recent papers on elementary particles I saw the statement: 'From the results of Bjorkén we can conclude that the proton in its electric properties has a granular structure.' It did not occur to the author that such words as 'granular structure' have perhaps no other meaning here than just the scaling law of Bjorkén – that they do not carry any further information. Or another example: Many experimental physicists nowadays look for 'quark' particles – particles with a charge $\frac{1}{3}$ or $\frac{2}{3}$ of the charge of the proton. I am convinced that this intense search for quarks is caused by the conscious or unconscious hope of finding the really elementary particles – the ultimate units of matter. But even if quarks could be found, for all that we know they could be divided again into two quarks and one antiquark, *etc.* They would not be any more elementary than a proton. You see how extremely difficult it is to get away from an old tradition.

What is really needed is a change in the fundamental concepts. We will have to abandon the philosophy of Democritos and the concept of fundamental elementary particles. We should accept instead the concept of fundamental symmetries which is a concept out of the philosophy of Plato. Just as Copernicus and Galileo in their method abandoned the descriptive science of Aristotle and turned to the structural science of Plato, so we are probably forced to abandon the atomic materialism of Democritos and to turn to the ideas of symmetry in the philosophy of Plato. Again we would return to a very old tradition. But, as I said before, such changes are extremely difficult. Even with the change many complicated details will have to be worked out, both experimentally and theoretically, in elementary particle physics; but I do not believe that there will be any spectacular breakthrough without this change in concepts.

Future development

After going through the three most important influences of tradition in science – those in the selection of problems, in the method and in the concepts – I should perhaps, in conclusion, say a few words about the future development of science. I am not interested in futurology, but since we can scarcely work on other problems but those that are given to us by the historical process, we may ask where this process has led to new and interesting questions. In physics I would like to mention astrophysics. In this field the strange properties of the pulsars and the quasars, and perhaps also the gravitational waves can be considered a challenge. Then there is the new and wide field of molecular biology where concepts of very different origins, namely physical, chemical and biological concepts meet and produce a great wealth of interesting new problems. Finally on the practical side we have to solve the very great problems posed by the deterioration of our environment.

I have mentioned these points not to make predictions about the future, but to emphasize that we do not need to invent our problems. The scientific tradition, that is the historical process, gives us many problems and encourages our efforts. And that is a sign for a very healthy state of affairs in science ■

Werner Carl Heisenberg, Hon FInstP. Born in Würzburg in 1901, Professor Heisenberg studied under Sommerfeld at the University of Munich. In 1923 he was awarded his Doctorate of Philosophy and then became Max Born's assistant. During 1924–5 he worked with Niels Bohr in Copenhagen, returning to Germany to become Lecturer in Theoretical Physics at the University of Leipzig. He was awarded the Nobel prize for physics in 1932. During the war he was Director of Kaiser Wilhelm Institute for Physics in Berlin Dahlem. After the war he became Director of the Max Planck Institute for Physics in Göttingen later in Munich. He is a foreign member of the Royal Society.
This article is based on a lecture given to the National Academy of Sciences in Washington to commemorate the 500th anniversary of Copernicus' birth.