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Lecture 4

The Growth of Science in Society

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The purpose of my lecture today is two-pronged. I want to outline the way scientific research is organised, and must be organised And for this purpose I shall have to speak again of the grounds on which science rests and the way in which discoveries are made.

I shall use as my starting point a notorious conflict between the scientific world and a considerable part of the lay public. I shall tell you how this controversy has recently led a distinguished group of sociologists to pronounce a wholesale condemnation of scientific procedures and I shall try then to resolve this situation by applying my own views on the nature of scientific discovery. This shall lead us then to a general theory of scientific life: of discipline and freedom in scientific institutions.

The current situation in the philosophy of science is a strange one. The movement of logical positivism, which aimed at a strict definition of validity and meaning, reached the height of its claims and prestige about twenty years ago. Since then it has become clearer year by year that this aim was unattainable. But since (to my knowledge) no alternative has been offered to the desired strict criteria of scientific truth, we have no accepted theory of scientific knowledge today.

Most writers on science have accepted science as an unquestionable fact which is not in need of philosophic justification nor capable of justification. You will rarely find this spelled out, but it is confirmed by current practice. Take a widely accepted account of science in Ernest Nagel's Structure of Science (1961). He writes that we do not know, whether the premisses assumed in the explanation of the sciences are true; and were the requirement that they be known to be true adopted, most of the widely accepted explanations in current science would have to be rejected as unsatisfactory. In other words, we must save our belief in the truth of scientific explanations by refraining to ask what they are based upon. Scientific truth is defined then as that which scientists say or think to be true.

Yet this lack of philosophic justification has not damaged the public authority of science, but rather increased it. Modern philosophers have played down this shortcoming, by declaring that the claims of science are only tentative and ever open to refutation by adverse evidence. And this added to the authority of science. It was taken to show that, while scientific knowledge was supremely reliable, scientists were at the same time supremely open-minded, setting an example of incomparable modesty and tolerance.

Yet there have been some occasions in this century when the very foundations of science have been challenged. Sporadic attacks came in the form of laws against teaching the theory of evolution and in a papal encyclical warning against this theory. An attack on a broad front was made by the Soviet Union. But a more recent and personal challenge to the foundations of science came from Dr. Velikovsky's highly unorthodox book Worlds in Collision published about fifteen years ago. Though emphatically rejected by scientists, it had a wide response among the lay public, who much resented the summary dismissal of the book by the experts. Then, some three years ago, new evidence turned up supporting Velikovsky's theory, but this was again bluntly rejected by scientists. This seemed utterly unjustified to many people and a group of sociologists took up the matter. They launched a systematic attack in the American Behavioral Scientist under the leadership of its Editor, Professor de Grazia, on the whole procedure by which contributions to science are tested, accepted or rejected.

A few words may cover for my present purpose Velikovsky's theory, since its truth or falsity will not be at issue. His theory is based on the acceptance of evidence from the Old Testament, the Hindu Vedas and Greek Roman mythology about the occurrence of catastrophic events in the earth's history from the fifteenth to the seventh century B.C. It interprets these disasters and upheavals as due to the repeated passage of the earth through the tail of a comet. This comet he claims, subsequently collided with Mars and by losing its tail transformed its head into the planet Venus. Further terrestrial upheavals ensued when in the year 687 B.C. Mars nearly collided with the earth; on one occasion the earth turned completely over, so that the sun rose in the west and set in the east. To account for these events Velikovsky supplements Newtonian gravitation by the assumption of powerful electrical and magnetic fields acting between planets.

As I said before, these ideas were rejected by astronomers, but appealed to a wide circle of laymen: the book became actually a best seller. And so bitter was the reaction of astronomers and other scientists to this, and the pressure they exercised, such that MacMillan's who had published Velikovsky's book, felt compelled to give up their rights in it and passed them on to Doubleday's who felt less vulnerable by the hostility of scientific opinion.

When eventually in September 1963 the American Behavioral Scientist published its protest against the treatment of Velikovsky, the editor, Alfred de Grazia, Professor of Government in New York University, stated the aim of the enquiry to be undertaken as follows: "The central problems are clear: Who determines scientific truth? What is their warrant?" and he adds that "some judgment must be passed upon the behavior of scientists and, if adverse, some remedies must be proposed."

Referring to the three articles in this issue of his journal de Grazia went on to say that: "if the judgment of the authors is correct, the scientific establishment is gravely inadequate to its professed aims, commits injustices as a matter of course, and is badly in need of research and reform."

If we accepted the positivist critique of science which leaves us today no other ultimate criterion of a scientific teaching, as that scientists accept it as valid, the answer to de Grazia's first question, Who determines scientific truth? would be: "The scientists." And to the second question, What is their warrant? we would answer that the decisions of scientists cannot be accounted for.

But this is not satisfactory. We must be able to ask whethe the reception of Velikovsky's ideas by scientists was fair, and, if not, what went wrong? Professor de Grazia sets out five points of a rational procedure for testing a proposed contribution to science. First, a contribution must not be rejected unread; second, it may claim to be tested and publicly discussed with its author; if it suggests radical innovations, this should be welcomed fourth, if its ideas were at first rejected, they should have a chance to return with additional proof and once more be cordially examined; and fifth, no authority should prevail against experimental evidence.

Professor de Grazia shows that all these rules were broken in Velikovsky's treatment by scientists. His work was condemned as utter nonsense by distinguished astronomers who frankly said they had not read his book. He asked to be admitted to a public discussion of his views and this was refused. He had concluded that the surface of Venus was hot and its atmosphere heavy with hydrocarbons. He asked the Harvard Observatory to test this prediction and this was refused.

This happened in 1946; in February 1963 the American space explorer, Mariner II, confirmed Velikovsky's predictions about Venus: its surface temperature was 800° F. and its clouds replete with hydrocarbons. But this striking success of the theory did not succeed in causing its discussion to be reopened by scientists; it was rated as a curious coincidence. Authority prevailed against facts.

I think it is understandable that Professor de Grazia was disappointed by the failure of scientists to live up to their professions of giving a ready hearing to any new ideas and submitting humbly to the test of any evidence contradicting their current views. No wonder perhaps that he went on then to affirm that the acceptance of new contributions by science does in fact not depend on the evidence of its truth, but takes place either at random, or in the service of ruling powers, or responding to economic or political interests, or simply as dictated by accepted dogma. He was certainly right in contrasting the principles which scientists profess to follow in dealing with a novel contribution to science, with the way they actually treated Velikovsky's ideas.

But the situation changes if these principles are qualified by their tacit assumptions. This is what I propose to do now; and once done, this should resolve also the dilemma produced by the modern philosophic critique of science. It will show that this critique leads to absurd results because in its search for strictly definable criteria of scientific truth it necessarily overlooks the tacit principles on which science is actually founded.

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A vital judgment practiced in science which is manifestly informal, is the assessment of plausibility. Only plausible ideas are taken up, discussed and tested by scientists. Such a decision may later be proved right, but at the time that it is made, the assessment of plausibility is based on a broad exercise of intuition guided by many subtle indications, and is thus manifestly undemonstrable.

To show what I mean I shall recall an example of a claim lacking plausibility to the point of being absurd, which I picked up twenty five years ago in a letter published in Nature. The author of this letter had observed that the average gestation period of different animals ranging from rabbits to cows was an integer multiple of the number π. The evidence he produced was ample, the agreement good. Yet the acceptance of this contribution by the journal was only meant as a joke. No amount of evidence could convince a modern biologist that gestation periods are equal to integer multiples of π. Our conception of the nature of things tells us that such a relationship is absurd. A more technical example from physics can be found in a paper by Lord Raleigh published in the Proceedings of the Royal Society in 1947. It described some fairly simple experiments which proved in the author's opinion that a hydrogen atom impinging on a metal wire could transmit to it energies ranging to a hundred electron-volts. Such an observation, if correct, would be far more revolutionary than the discovery of atomic fission by Otto Hahn in 1939. Yet when this paper appeared and I asked various physicists' opinions about it, they only shrugged their shoulders. They could not find fault with the experiment, yet they not only did not believe its results, but not even thought it worthwhile to consider what was wrong with it, let alone check upon it. They just ignored it. About ten years later some experiments were brought to my notice which accidentally offered an explanation of Lord Raleigh's findings. His results were apparently due to some hidden factors of no great interest, but which he could hardly have identified at the time. He should have ignored his observation, as his colleagues were quite rightly to do.

In my early writings I have also described how the possible reality of a certain type of observations which had long been denied, was for a time accepted, then again rejected, only to be soon accepted again and presently rejected once more, these two consecutive alternations of acceptance and rejection taking place within 25 years. The observations in question were the apparent transformations of elements. Ever since the immutability of elements had been accepted, such observations have been cast aside as dirt effects, but after Rutherford and Soddy established the fact of radioactive disintegration, they were taken seriously and accepted for publication, but disappeared from journals again, as it was recognised that radioactivity occurs only in very few comparatively rare elements. New reports of apparently well authenticated cases appeared in journals once more in response to Rutherford's discovery of artificial disintegration of elements, and these presently vanished again as the nature of such disintegration became clear and showed that it could not happen in a chemical laboratory. These apparent chemical transformations of elements must again continue to turn up, but are unhesitatingly ignored once more as they had been all along until the advent of radioactivity.

Suppose then that Velikovsky's claims were as unplausible as the parallelism between periods of gestation and the number π ; or as unplausible as Lord Raleigh's results published in the Proceedings of the Royal Society; or, again, as unplausible as the chemical transformation of elements appears to be now - or if it were to appear even more absurd than these possibilities do - then it would certainly correspond to the current custom of science to reject them at a glance unread, and to refuse discussing them publicly with the author. Indeed, to drop one's work in order to test some of the claims of Velikovsky as requested by him, would appear a culpable waste of time, of expense and of effort.

But how about the predictions of Velikovsky which came true? Should these not have caused his book to be reconsidered? No, a theory rejected as absurd will not always be made plausible by the confirmation of some of its predictions. The fate of Eddington's cosmic theories may illustrate this. Far back in 1946 I put on record an anxious remark by a dinstinguished mathematician, (then professor at the University of Manchester), who complained that recent measurements had much strengthened the evidence for Eddington's equation relating the masses of a proton and an electron. He feared that this confirmation of Eddington's theory, which he held to be absurd, might gain acceptance for it. His anxiety proved unjustified. Though a few years later I could note that a quite different set of new measurements had recently improved thirtyfold the accuracy of another prediction of Eddington's theory, this too was disregarded as fortuitous by the great majority of physicists. The theory has since passed into limbo - from which no conceivable future confirmation can retrieve it. In refusing to take notice of the fact that some of Velikovsky's predictions came true, scientists acted on the same lines as they did, and did rightly, in respect of Eddington's theories and in many other similar instances.

This does not mean of course that scientists were always right in such actions. I have shown how they changed their opinion twice within a few years on the significance of the same kind of experimental evidence. I have myself suffered from the mistake of such judgments. In the same month when The American Behavioral Scientist came out protesting against the treatment of Velikovsky at the hand of scientific opinion, I published in Science an account of the way a theory that I had put forward almost half a century earlier on the adsorption of gases on solid surfaces was disregarded by science for much of that period, because its presuppositions were contrary to the current views about the nature of intermolecular forces. I recalled how the striking evidence I had produced for the theory was shrugged aside, while flimsy observations, which have since been recognised to be misleading, were given a central position in contradicting my views. Yet after a while it turned out that my theory was right.

Still, I did not complain about this mistaken exercise of authority. Hard cases make bad laws. The kind of discipline, which had gone wrong in this case, was indispensable. Journals are bombarded with contributions offering fundamental discoveries in physics, chemistry, biology or medicine, most of which are nonsensical. Science cannot survive unless it can keep out such contributions and safeguard the basic soundness of its publications. This may lead to the neglect or even supression of valuable contributions, but I think this risk is unavoidable. If it turned out that scientific discipline is keeping out a large number of important ideas, a relaxation of its severity might be indicated. But, if this led to the intrusion of a great many bogus contributions, the situation might become desperate.

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Yet we may well wonder how the continuity enforced by current judgments of plausibility can allow the appearance of any true originality. It certainly does allow it: Science presents a panorama of surprising developments. How can such surprises be produced on effectively dogmatic grounds?

A phrase often heard in science may point towards the explanation. We often hear of surprising confirmations of a theory. The discovery of America by Columbus was a surprising confirmation of the earth's sphericity; the discovery of electron diffraction was a surprising confirmation of de Broglie's wave-theory of matter; the discoveries of genetics brought surprising confirmations of the Mendelian principles of heredity. We have here the paradigm of all progress in science: discoveries are made by pursuing unsuspected possibilities suggested by existing knowledge. This is how science retains its identity through a sequence of successive revolutions.

The sight of a solid object before me indicates that it has another side and a hidden interior, which I could explore, and the sight of another person indicates unlimited hidden workings of his mind and body. Perception has this inexhaustible profundity because what we perceive is an aspect of reality and aspects of reality are clues to yet boundless undisclosed and perhaps yet unthinkable experiences. This is what the existing body of scientific thought is to the productive scientist: he sees in it an aspect of reality which as such is an inexhaustible source to new and promising problems. And his work bears this out. Science continues to be fruitful, because it offers an insight into the nature of reality.

This view of science recognises once more what all scientists actually believe. For they must believe that science offer us an aspect of reality, and may therefore manifest its truth inexhaustibly and often surprisingly in the future. Only in this belief, can the scientist conceive problems, pursue enquiries, claim discoveries; this belief is the ground on which he teaches his students and exercises his authority over the public. And it is by transmitting this belief to succeeding generations that scientists grant their pupils independent grounds from which to start towards their own discoveries and innovations, possibly in opposition to their own teachers. This belief both justifies the discipline of scientific soundness and safeguards the freedom of scientific originality.

However, this can work only so long as scientists have similar conceptions of the nature of things. What if a substantial difference arises about the nature of reality? If such basic differences arise, we must expect it to result in a scism. And indeed, reasoned discussion breaks down in science between two opinions based on different foundations. Neither side can produce then an argument which the other cannot interpret in his own terms. I have described this some years ago for a number of important scientific polemics. A famous case was the violent controversy over the question whether fermentation is caused by living germ cells or by inanimate catalysts. It went on for more than half a century, involving such great chemists as Woehler and Liebig on the side of inanimate catalysts and including Pasteur on the side of living germ cells. Effective argument being impossible, it was sometimes supplemented by ridiculing the opponent's views and pouring scorn on them. The controversy was resolved only after the contestants had died, by the discovery of enzymes which are the inanimate products of living cells. In a way both sides proved right. But I can see no guarantee that such a happy resolution of a conflict will always be possible and arrive in time for avoiding a fatal break in the rational procedure of scientific argument.

And this brings us back to the origins of the Velikovsky case. Basic assumptions about the nature of things will tend to lie more widely apart between people inside and outside of science than they ever can be between scientists. Laymen normally accept the teachings of science not because they share its conception of reality, but because they submit to the authority of science. Hence if they ever venture seriously to dissent from scientific opinion, a regular argument may not prove feasible. And it will almost certainly prove impracticable when the question at issue is whether a certain set of evidence is to be taken seriously or not. There may be nothing strange to the layman in the suggestion that the average periods of pregnancy of various animals are integer multiples of the number π, but he will only drive the scientist to despair if he challenges him to prove that this is absurd. He may feel affronted by the scientist's undemonstrable judgment which rejects at a glance a set of data that seem convincing to himself as a layman. He will demand that the evidence should at least be properly examined and cannot understand why the scientist, who prides himself on welcoming any novel idea with an open mind and on holding his own scientific theories only tentatively, sharply refuses his request. This is how Velikovsky's ideas and facts, as other theories that are attractive to wide ranges of the public, evoked only angry rejection from scientists.

Such conflicts between science and the general public may imperil science. It is generally supposed that science will always be protected from destructive lay interference on account of its economic benefits; but this is not so. The Soviet government adopted the theories of Lysenko and gravely hampered all branches of biological research for thirty years, overlooking altogether the damage to its agriculture. The ruling Party believed in fact that it was improving the cultivation of grain by Lysenko's use of the hereditary transmission of acquired characters, which scientific genetics declared impossible. Far from preventing the attack on science, the economic motive greatly reinforced it - and this may frequently be the case.

To defend science against lay rebellions on the grounds of its technical achievements may be very precarious. To pretend that science is open minded, that it is certainly not, may prove equally perilous. On the other hand, to declare that the purpose of science is to understand nature may seem dull and old fashioned, and to confess how greatly such explanation of nature relies on vague and undemonstrable conceptions of reality, may sound positively scandalous. But since this is in fact true, it might eventually prove safest to say so.

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In any case, it is only on these grounds that the rules governing scientific life can be understood. They alone can explain how an immense number of independent scientists, largely unknown to each other, co-operate step by step sharing the same undefinable assumptions and submitting to the same severe unwritten standards, Let me show how this works.

For a scientific community, comprising large numbers, to exist, there must be a large area of hidden and yet accessible truths, far exceeding the capacity of one man to fathom. There must be work for thousands. Each starts then with sensing a point of deepening coherence and continues by feeling his way towards that coherence. His questing imagination, guided by intuition, forces its way, until he has achieved success or admitted failure. The clues supporting his surmises are largely unspecifiable, his feeling of their potentialities hardly definable. Scientific research is one continued act of tacit integration - like making out an obscure sight, or being engaged in painting a picture, or in writing a poem. It is rare therefore for two scientists to contribute to one enquiry on equal terms or for one scientist to be engaged in more than one problem at a time. But it is not rare for two or more scientists to make independently the same discovery, for different scientists can actualise only the same available potentialities.

We have here the reason why theinititiative to scientific enquiry, and its pursuit, must be left to the free decision of the individual scientist, and why it is effective. The scientist must be granted independence, because only his personal vision can achieve essential progress in science. Enquiries can be conducted as surveys according to plan, but they will never add up to a major new idea.

Independence will safeguard originality which is the essence of progress in science; but there is another requirement which has to be sustained by the authority of scientific opinion over scientists. To form part of science a statement of fact must not only be true, but also be interesting, and, more particularly, be interesting to science. Reliability, exactitude, counts as one of the factors, contributing to scientific interest, but it is not enough. Two further important factors enter into the assessment of scientific value. One is the way a new fact enters into the systematic structure of science, correcting or expanding this structure. The other factor is independent both of the reliability and systematic interest of a scientific discovery for it lies in its subject matter, as known before it was taken up by science: it consists in the intrinsic pre-scientific interest of the matter, studied by science.

The scientific interest - or the scientific value - of a contribution to science is thus jointly formed by these three factors, which we may name as its <u>exactitude</u>, its <u>systematic importance</u> and the <u>intrinsic interest</u> of <u>its subject matter</u>. The proportion in which these factors enter into scientific value varies greatly over the various domains of science; deficiency in one factor may be balanced by greater excellence in another. The highest degree of exactitude and widest range of systematisation are found in mathematical physics, and this compensates here for a lesser intrinsic interest of its inanimate subject. At the other end of the sciences, we have domains like zoology and botany which lack exactitude and have no systematic structure comparable in range to that of physics, but which make up for this deficiency by the far greater intrinsic interest of living things compared with inanimate matter.

A scientist engaged in research must have a keen sense of scientific value, he must be attracted by problems promising a result having such value, must be capable of feeling his way towards it and of realising the value of an important result when he has found it. Failure to recognise the importance of one's result will delay its publication, hamper its development and presentation. Failure to abandon a line of enquiry which can bring only unimportant results is part of the same weakness. Anyone who has conducted a research school will know of such heavily damaging and mortifying mistakes. They do indeed mark a serious deficiency in one's scientific ability.

The assessment of scientific value is also the principal standard by which the social structure of the scientific community is determined. Funds and appointments serving scientific research must be distributed in a way that promises the highest total increment to science. Authority at influential centers must be given to scientists distinguished by their exceptional capacity for advancing science and rewards be distributed so as to encourage the greatest total advancement of science. Every decision of this kind requires comparisons of scientific value. It can be made in rational way only if there exist true standards for comparing the value of contributions to science all along the range of sciences from astronomy to medicine.

This does not mean that one has to compare the scientific value of one entire branch of science with another, say astronomy with medicine. It requires only that we be able to compare the scientific increment achieved at similar costs of effort and money in the various branches of science and among rivals within those branches. The marginal principle of economics offers the conceptual model for this. It tells us that the marginal yield in terms of scientific value must be kept equal all along the advancing borders of the sciences.

But how can anybody compare the scientific value of discoveries in astronomy with those in medicine? Nobody can, but nobody need to do. All that is required is that we compare these values in closely neighbouring fields of science. Judgments extending over neighbourhoods will overlap and form a chain spanning the entire range of sciences. This principle, the principle of overlapping neighbourhoods, fulfils here the functions which a capital market performs in comparing the profitability of competing enterprises through the thousand branches of an economic system.

We must ask now how the two great principles which underly the growth of science, can effectively control the joint enterprise of the scientific community. How can the initiatives of scientists independently chosing their problems be co-ordinated? And how can they be subjected to scientific rigour, how be guided by true scientific value and have their merits rightly assessed and rewarded? These aims will be achieved by two twin principles: namely, self-coordination by mutual adjustment and discipline under mutual authority.

Of self-coordination by mutual adjustment I have written often in the past fifteen years. Each scientist sets himself a problem and pursues it with a view to the results achieved before by all other scientists, who had likewise set themselves problems and pursued them with a view to the results achieved by others before. Such self-coordination represents the highest possible efficiency in the use of scientific talent and material resources, provided only, that the professional opportunities for research and the money for its pursuit are rationally distributed.

This brings us to the principle of mutual authority. It consists in the fact that scientists keep watch over each other. Each scientist is both subject to criticism by all others and is encouraged by their appreciation. This is how scientific opinion is formed, which enforces scientific standards and regulates the distribution of professional opportunities and research grants. Naturally, only fellow-scientists working in closely related fields are competent to exercise authority over each other; but their restricted fields form chains of overlapping neighbourhoods extending over the entire range of sciences.*

Thus an indirect consensus is formed between scientists so far apart that they could not understand more than a tiny fraction of each other's subjects. It suffices that the standards of plausibility and worthwhileness be equal around ever single point; this will keep them equal over all the sciences. Hence scientists from the most distant branches of science will rely on each other's results and will support each other against any laymen seriously challenging their scientific authority.

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* Mutual authority, based on overlapping competences will apply also to other cultural fields and indeed to a wide range of consensual activities of which the participants know only a small fragment. It suggests a way by which resources can be rationally distributed between any rival purposes that cannot be valued in terms of money. All cases of public expenditure serving collective interests are of this kind. This is, I believe, how the claims of a thousand government departments can be fairly rationally adjudicated, though no single person can know well more than a tiny fraction of them.

Such is the constitution of the scientific community. It is governed by beliefs, by values and practices transmitted to succeeding generations. Each new independent member adheres to this tradition, while assuming at the same time the responsibility shared by all members for re-interpreting the tradition and possibly revolutionising its teachings.

The opportunities for discovery offered by nature to the human mind are not of our making. We have developed a body of thought capable of exploiting these opportunities and have organised a body of men for this task. The laws of this community are determined by the nature of its task. But the task itself is indeterminate: it merely demands that we advance into the unknown. And even after this is done, the advances are not known to any single man, for no man can know more than a tiny fragment of science.

Thus the opportunities offered by nature to the human mind, have evoked in our society a response over which we have no control. This is what I meant by speaking in my title of the growth of science in society. Maybe we should rejoice to be servants of such great transcendent powers. Maybe we are becoming, and even ought to become, in other ways too a Society of Explorers dedicated to the pursuit of aims unknown to us. Perhaps the freedom of thought in which we take pride, has such awsome implications. I cannot tell.