SCIENCE, FAITH AND SOCIETY

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APPENDIX

BACKGROUND AND PROSPECT

IN AUGUST 1938 the British Association for the Advancement of Science founded the Division for the Social and International Relations of Science, which was to give social guidance to the progress of science. A movement for the planning of science spread and became predominant among scientists interested in public affairs. A small number of scientists, to which I belonged, strenuously opposed this movement.

In December 1945 the Division called a meeting to discuss planning and asked me to open the proceedings. My address renewed my criticism of planning and upheld the traditional independence of scientific enquiry. I expected a hostile reaction, but, to my surprise, speakers and audience showed themselves in favour of science pursued freely for its own sake. Since then the planning movement has dwindled to insignificance in Britain, but the theoretical problems it has raised are still with us. They are part of the general impact made by the Russian Revolution on the minds of men everywhere.

After the Revolution, scientific research in Soviet Russia was divided into two sections. One was conducted in the light of dialectical materialism under the leadership of the Communist Academy founded in 1926. Membership in the Academy was confined to Party members. Scientists forming the other section worked freely in constant touch with Western scientists. In 1932 a change occurred affecting both sides. The Soviet government repudiated the wild dialectical speculations of the Communist Academy and covered them with ridicule. At the same time, however, the other part of science, hitherto conducted on traditional lines, was bidden to acknowledge the supremacy of dialectical materialism. A declaration to this effect can be found in the editorial opening of the new German-language physics journal of the Soviets, founded in that year; it was inserted at the request of the Party. Russia's most distinguished biologist, N. I. Vavilov, was induced, in the same year, to denounce the theoretical pursuit of genetics practised in the West and to accept instead the view of science planned to serve economic needs, as declared by the Conference on Planning Genetics Selection Research in Leningrad.

At Easter 1935 I visited N. I. Bukharin in Moscow. Though he was heading for his fall and execution three years later, he was still a leading theoretician of the Communist party. He explained to me that the distinction between pure and applied science, made in capitalist countries, was due to the inner conflict of this type of society which deprived scientists of the consciousness of their social functions, thus creating in them the illusion of pure science. Accordingly, Bukharin said, the distinction between pure and applied science was inapplicable in the Soviet Union. This implied no limitation on the freedom of research; scientists would follow their interests freely in the U.S.S.R., but, owing to the internal harmony of socialist society, they would inevitably be led to lines of research which would benefit the current Five Year Plan. The comprehensive planning of all research was to be regarded merely as a conscious confirmation of the pre-existing harmony between scientific and social aims.

In 1935 I could still smile at this dialectical mysterymongering, never suspecting how soon it would show terrible consequences. Vavilov's persecution at the hands of T. D. Lysenko had already begun. It led to his dismissal from office in 1939 and then to his arrest and death in a prison camp around 1943. This campaign wrought havoc among biologists and paralysed whole branches of biology in Soviet Russia from 1939 until well after Stalin's death in 1953. The physical sciences got off more lightly. By the time of this writing, the natural sciences have been almost completely liberated from ideological subservience to Marxism, which continues to be imposed on the study of economics, sociology and the humanities.

I have said that in England the campaign for the planning of science, evoked by the enforcement of Marxist philosophy in the Soviet Union, never became a serious menace. But the mental disturbance caused by it was profound. A distinguished scientist like Lancelot Hogben could write:

> From the landman's point of view the earth remained at rest till it was discovered that pendulum clocks lose time if taken to places near the equator. After the invention of Huyghens, the earth's axial motion was a socially necessary foundation for the colonial export of pendulum clocks.

Many such absurd theories were put forward in Hogben's famous book *Science for the Citizen* (1938), which had a vast circulation. An account of the considerable literature moving on similar lines is given in my book *The Logic of Liberty* (1951).

It was difficult to get a hearing for opposing views. Those who knew about the persecution of biologists in Soviet Russia would not divulge their information. My writings and those of J. R. Baker which, from 1943 on, exposed this persecution were brushed aside as anti-Communist propaganda. The way in which scientific research was organised in Soviet Russia was held up as an example to be followed. Public meetings, attended by distinguished British scientists, gave currency to this appeal.

It was in facing these events that I became aware of the weakness of the position I was defending. When I read that Vavilov's last defence against Lysenko's theories, in 1939, was to evoke the authority of Western scientists, I had to acknowledge that he was appealing to one authority against another: to the authority accepted in the West against the authority accepted in Soviet Russia. The meeting had been called by the editors of the journal *Under the Banner of Marxism*. Their acceptance of Lysenko's authority was based on their philosophy of science. What philosophy of science had we in the West to pit against this? How was its general acceptance among us to be accounted for? Was this acceptance justified? On what grounds?

Marxism has challenged me to answer these questions: the essays republished here were written in reply to them. Like the Marxist theory, my account of the nature and justification of science includes the whole life of thought in society. In my later writings it is extended to a cosmic picture. But the ultimate justification of my scientific convictions lies always in myself. At some point I can only answer, 'For I believe so'. This is why I speak of Science, Faith and Society.

I first analysed the process of knowing, as is usual, in isolation. There are an infinite number of mathematical formulae which will cover any series of numerical observations. Any additional future observations can still be accounted for by an infinite number of formulae. Moreover, no mathematical function connecting instrument readings can ever constitute a scientific theory. Future instrument readings cannot ever be predicted. But this is merely a symptom of a deeper inadequacy, namely, that the explicit content of a theory fails to account for the guidance it affords to future discoveries. To hold a natural law to be true is to believe that its presence will manifest itself in an indeterminate range of yet unknown and perhaps yet unthinkable consequences. It is to regard the law as a real feature of nature which, as such, exists beyond our control.

We meet here with a new definition of reality. Real is that which is expected to reveal itself indeterminately in the future. Hence an explicit statement can bear on reality only by virtue of the tacit coefficient associated with it. This conception of reality and of the tacit knowing of reality underlies all my writings.

If explicit rules can operate only by virtue of a tacit coefficient, the ideal of exactitude has to be abandoned. What power of knowing can take its place? The power which we exercise in the act of perception. The capacity of scientists to perceive the presence of lasting shapes as tokens of reality in nature differs from the capacity of our ordinary perception only by the fact that it can integrate shapes presented to it in terms which the perception of ordinary people cannot readily handle. *Scientific knowing consists in discerning Gestalten that are aspects of reality.* I have here called this 'intuition'; in later writings I have described it as the tacit coefficient of a scientific theory, by which it bears on experience, as a token of reality. Thus it foresees yet indeterminate manifestations of the experience on which it bears.

Every interpretation of nature, whether scientific, non-scientific or anti-scientific, is based on some intuitive conception of the general nature of things. In the magical interpretation of experience we see that some causes which to us are massive and plain (such as a stone's smashing a man's skull) are regarded as incidental or even irrelevant to the event, while certain remote incidents (like the passing overhead of a rare bird) which to us appear to have no conceivable bearing on it are seized upon as its effective causes. Such a general system may resist many facts which to those who do not believe in the system seem to refute it. Any general view of things is highly stable and can be effectively opposed, or rationally upheld, only on grounds that extend over the entire experience

of man. The premisses of science on which all scientific teaching and research rest are the beliefs held by scientists on the general nature of things.

The influence of these premisses on the pursuit of discovery is great and indispensable. They indicate to scientists the kind of questions which seem reasonable and interesting to explore, the kind of conceptions and relations that should be upheld as possible, even when some evidence seems to contradict them, or that, on the contrary, should be rejected as unlikely, even though there was evidence which would favour them.

The premisses of science are subject to continuous modifications. In the appendix to these lectures I have described a series of stages through which the premisses of physics have passed since Copernicus. Every established proposition of science enters into the current premisses of science and affects the scientist's decision to accept an observation as a fact or to disregard it as probably unsound. To show this, a long series of such cases is given in the appendix, and many other examples can be found in my later writings. This material refutes the widely held view that scientists necessarily abandon a scientific proposition if a new observation conflicts with it. The material collected in the appendix also refutes the view that the progress of science affects only the interpretation of the facts and leaves the accepted facts unchanged.

All this is accounted for by the view that the advancement of science consists in discerning Gestalten that are aspects of reality. We know that perception selects, shapes and assimilates clues by a process not explicitly controlled by the perceiver. Since the powers of scientific discerning are of the same kind as those of perception, they too operate by selecting, shaping and assimilating clues without focally attending to them. Thus it is ultimately left to the personal judgement of the scientist to decide what conflicting evidence invalidates a proposition, what things coming to his notice must be accepted as facts and what should be concluded from them.

Gestalt psychology and, more recently, transactional psychology have studied the shaping of percepts. This process consists in our selecting from the material presented to us and supplementing it. The result is an interpretation of the material which may be either compelling or to some extent optional.

The criteria of such shaping are qualitative, undefinable and often conflicting. This applies also to the shaping of experience by science. All great discoveries are beautiful, but the quality of beauty varies. The discovery of Neptune was a brilliant confirmation of hitherto accepted views, the discovery of radioactivity a dazzling revolution against them; each was beautiful in its own way. In *Personal Knowledge* I told of discoveries in mathematical physics, guided by pure theoretical beauty. In a recent paper entitled "The Evolution of the Physicist's Picture of Nature" (*Scientific American*, CCVIII [May, 1963]), P. A. M. Dirac emphatically confirms this: '. . . It is more important to have beauty in one's equations than to have them fit experiment'. I shall presently say more about the final arbitrament of such rival claims.

Maurice Merleau-Ponty's *La Phénoménologie de la Perception* (Paris, 1945) reached this country after these lectures were delivered. The book does not deal with the philosophy of science; yet by analysing perceived knowledge on the lines of Husserl, it arrives at views akin to those I have expressed here. A. D. Ritchie, who was for a number of years my colleague in Manchester, independently developed in *Essays in Philosophy* (London, 1948) and in *History and Methods of the Sciences* (Edinburgh, 1958), ideas on the nature of science basically akin to my own. Of later writers whose conclusions overlap my own, I shall cite W. I. Beveridge, J. D. Bronowski, Stephen Toulmin, N. R. Hanson, Konrad Lorenz, Thomas Kuhn, Gerald Holton, Ch. Perelman and A. I. Wittenberg.

The Art of Scientific Discovery (1950), by W. I. Beveridge, brought invaluable sketches drawn from life to illustrate scientific discovery as an art. J. Bronowski, in *Science and Human Values* (1956), has also developed the view that scientific discovery is a creative act akin to creation in the arts. In *The Philosophy of Science* (1953) Stephen Toulmin has shown systematically that the framework of scientific theories contains general suppositions which cannot be put directly to an experimental test of truth or falsity. Such general premisses overlap more specific statements which embody them. N. R. Hanson has observed in *Patterns of Discovery* (1958) that scientific facts are 'theory-laden'. An essay by Konrad Lorenz, 'Gestalt

Perception as Fundamental to Scientific Knowledge',[1] illuminatingly develops the analogy between the perception of Gestalt and the knowledge of science but does not enquire into the ultimate justification of science, which I have approached from this starting point in the present lectures (1946) and in my *Personal Knowledge* (1958). Thomas Kuhn, in *The Structure of Scientific Revolutions* (1962), pointed out that some major discoveries have profoundly affected the outlook of scientists, and he called these discoveries 'paradigmatic'. Gerald Holton in an essay published in *Eranos Jahrbücher*, XXXI (1962), under the title 'Über die Hypothesen welche der Naturwissenschaft zugrunde liegen', demonstrated the 'thematic' dimension of scientific propositions, which is what I described as their part in embodying general premisses of science. In *Vom Denken in Begriffen* (Basel and Stuttgart, 1957), A. I. Wittenberg shows that reason discovers and must acknowledge, in mathematics, an ultimate knowledge, the content of which cannot be fully explicited. This situation forms part of our intellectual existence. Ch. Perelman, in *La Nouvelle Rhetorique, Traité de l'Argumentation* (Paris, 1958), proceeds from the dubitability of all inferences to an enquiry into the convincing power of rhetorical argument, with which he abides. Wittenberg and Perelman both enquire, as I have done, into the role of decision and personal judgement in science and acknowledge their comprehensive powers. They would seem to share my view, that our dependence on these powers is the fundamental problem of epistemology.

Having dealt with the tacit coefficient of explicit scientific knowledge, we must now turn to the tacit process by which scientific knowledge is discovered. What do we know about the process of scientific intuition?

Surprising discoveries are often made on the grounds of observations that have been known for some time. Jeans quotes as examples the work of Copernicus, Galileo, Kepler, Newton, Lavoisier and Dalton, to which I would add Darwin's work, De Broglie's wave theory, Heisenberg's and Schrodinger's quantum-mechanics and Dirac's theory of the electron and

[1] English translation of 'Gestaltwahrnehmung als Quelle Wissenschaftlicher Erkenntnis', *Zeit. f. exp. u. angew. Psychol.*, 1959, No. 6, 118–65, in *General Systems*, Vol. VII (1962), ed. L. von Bertalanffy and A. Rappaport [Ann Arbor, Mich.].

positron. These inferences from known facts had to await the action of exceptional intuitive powers, and they clearly demonstrate the existence of such powers.

But in spite of much beautiful work done by Gestalt psychologists on problem-solving, of striking descriptions of the process of discovery by Poincaré and by Hadamard and of the pioneering enquiries of Polya into the heuristics of mathematics, we still have no clear conception of how discovery comes about. The main difficulty has been pointed out by Plato in the *Meno*. He says that to search for the solution of a problem is an absurdity. For either you know what you are looking for, and then there is no problem; or you do not know what you are looking for, and then you are not looking for anything and cannot expect to find anything. If science is the understanding of interesting shapes in nature, how does this understanding come about? *How can we tell what things not yet understood are capable of being understood?* The answer I gave here to this question was that we must have a foreknowledge sufficient to guide our conjecture with reasonable probability in choosing a good problem and in choosing hunches that might solve the problem. A potential discovery may be thought to attract the mind which will reveal it—inflaming the scientist with creative desire and imparting to him intimations that guide him from clue to clue and from surmise to surmise. The testing hand, the straining eye, the ransacked brain, may all be thought to be labouring under the common spell of a potential discovery striving to emerge into actuality. I feel doubtful today about the role of extra-sensory perception in guiding this actualisation. But my speculations on this possibility illustrate well the depth that I ascribe to this problem.

Admittedly, there are rules which give valuable guidance to scientific discovery, but they are merely *rules of art*. The application of rules must always rely ultimately on acts not determined by rule. Such acts may be fairly obvious, in which case the rule is said to be precise. But to produce an object by following a precise prescription is a process of manufacture and not the creation of a work of art. And likewise, to acquire new knowledge by a prescribed manipulation is to make a survey and not a discovery. The rules of scientific enquiry leave their own application wide open, to be decided by the scientist's

judgement. This is his major function. It includes the finding of a good problem, and of the surmises to pursue it, and the recognition of a discovery that solves it. In each such decision the scientist may rely on the support of a rule; but he is then selecting a rule that applies to the case, much as the golfer chooses a suitable club for his next stroke.

Viewed from outside, as I have just described him, the scientist may appear as a mere truth-finding machine steered by intuitive sensibility. But this view overlooks the curious fact that from beginning to end he is himself the ultimate judge in deciding on each consecutive step of his enquiry. He has to arbitrate all the time between his own passionate intuition and his own critical restraint of it. The reach of these ultimate decisions is wide: the great scientific controversies show the range of basic questions which may remain in doubt after all sides of an issue have been examined. The scientist must decide such issues, left open by opposing arguments, in the light of his own scientific conscience. My book *Personal Knowledge* (1958) attempts to buttress this final commitment against the charge of subjectivity.

Since an art cannot be precisely defined, it can be transmitted only by examples of the practice which embodies it. He who would learn from a master by watching him must trust his example. He must recognise as authoritative the art which he wishes to learn and those of whom he would learn it. Unless he presumes that the substance and method of science are fundamentally sound, he will never develop a sense of scientific value and acquire the skill of scientific enquiry. This is the way of acquiring knowledge, which the Christian Church Fathers described as *fides quaerens intellectum*, 'to believe in order to know'.

To learn an art by the example of its practice is to accept an artistic tradition and to become a representative of it. Novices to the scientific profession are trained to share the ground on which their masters stand and to claim this ground for establishing their independence on it. The imitation of their masters teaches them to insist on their own originality, which may oppose part of the current teachings of science. It is inherent in the nature of scientific authority that in transmitting itself to a new generation it should invite opposition to itself and assimilate this opposition in a reinterpretation of the scientific tradition.

Enforcement of discipline, combined with inducement to dissent, is also exercised by science in controlling the resources of scientific research and the organs of scientific publicity. A contribution to science is accepted only if, in the light of scientific beliefs about the nature of things, it appears sufficiently plausible. Only thus can contributions of cranks, frauds and bunglers be prevented from flooding scientific publications and corrupting scientific institutions. At the same time, scientific authority ascribes the highest merit to originality, which may dissent to some extent from the established teachings of science. This internal tension and its dangers are inevitable.

The authority of science resides in scientific opinion. Science exists as a body of wide-ranging authoritative knowledge only so long as the consensus of scientists continues. It lives and grows only so long as this consensus can resolve the perpetual tension between discipline and originality. Every succeeding generation is sovereign in reinterpreting the tradition of science. With it rests the fatal responsibility of the self-renewal of scientific convictions and methods. To speak of science and its continued progress is to profess faith in its fundamental principles and in the integrity of scientists in applying and amending these principles.

Each scientist is confronted with the criticism of his neighbours, who in their turn are criticised by their own neighbours. Thus the chain of mutual appreciation spreads throughout the body of science, from mathematics to medicine, and maintains the same fundamental beliefs and standards of scientific interest everywhere. Rooted in the same tradition as his colleagues, each scientist independently plays his part in maintaining this tradition over an immense area of scientific enquiry of which he knows next to nothing.

There are differences in rank between scientists, but these are of secondary importance: everyone's position is sovereign. The Republic of Science realises the ideal of Rousseau, of a community in which each is an equal partner in a General Will. But this identification makes the General Will appear in a new light. It is seen to differ from any other will by the fact that it cannot alter its own purpose. It is shared by the whole

community because each member of it shares in a joint task. This community would instantly dissolve if this task came to an end and the members of the community had to decide on doing something else.

We can generalise this to other modes of discovery in literature, in the arts, in politics. All these can advance only fragmentarily by the efforts of individuals within a community organised essentially on the lines of scientific life. The community must guarantee the independence of its active members in the service of values jointly upheld and mutually enforced by all. The creative life of such a community rests on a belief in the ever continuing possibility of revealing still hidden truths. In *Science, Faith and Society*, I interpreted this as a belief in a spiritual reality, which, being real, will bear surprising fruit indefinitely. To-day I should prefer to call it a belief in the reality of emergent meaning and truth.

The mental pursuits of society depend for their resources and their protection on its economic and legal order. Consequently, the pursuit of profit and power will interact with the growth of thought in society. The extent of this interaction will vary among the different branches of thought. As for the effect on science, its progress can hardly be deflected at all from its intrinsic interests; it can only be stunted or stopped by an infringement of its autonomy.

This recognition of the symbiosis between thought and society brings us closer to the Marxist position and at the same time makes our difference from it clear. Marxism-Leninism denies the intrinsic creative powers of thought. Any claim to independence by scientists, scholars or artists must then appear as a plea for self-indulgence. A dedication to the pursuit of science, wherever it may lead, becomes disloyalty to the power responsible for the public welfare.

Since this power regards itself as the embodiment of historic destiny and as dispenser of history's promises to mankind, it can acknowledge no superior claims of truth, justice or morality. Alternatively, materialistic (or romantic) philosophies, denying any universal claims to the standards of truth, justice or morality, may deprive citizens of any grounds for appealing to these standards and thus endow the government with absolute power. The two processes are in fact fused in their joint justification of force as superior to mind.

But we must add here an additional process which makes violence the embodiment of the values it overrides. Those who in our day brought into power governments exempt from the standards of humanity were themselves prompted by an intense passion for the ideals which they so contemptuously brushed aside. They had rejected the overt professions of these ideals as philosophically unsound, hypocritical and specious, but they had covertly injected the same ideals into the new despotisms which they set up. Thus these ideals became immanent in the violence which ruthlessly rejected them. By virtue of this *moral inversion* (as I have later called it), the very immoralism of this power became a token of its moral purity. In view of its internal structure it could honestly reject any accusations of immorality in the very breath of proclaiming its own immorality.

A régime thus constituted claims to embody, besides morality, the ideals of justice, of the arts and sciences—in short all manner of truth. But here it overreaches itself. The rebellious movement which has transformed the régime of most Communist countries since Stalin's death was stirred up by seething demands for truth. I shall quote here from the writings of Nicolas Gimes, a Hungarian Communist who, though he had shortly before been a faithful Stalinist, turned against Stalinism in the Hungarian Revolution of October 1956. The following passage was published three weeks before the revolution.

> Slowly we had come to believe, at least with the greater, the dominant part of our consciousness . . . that there are two kinds of truth, that the truth of the Party and the people can be different and can be more important than the objective truth and that truth and political expediency are in fact identical. This is a terrible thought . . . if the criterion of truth is political expediency, then even a life can be 'true' . . . even a trumped up political trial can be 'true' . . . . And so we arrived at the outlook which infected not only those who thought up the faked political trials but often affected even the victims; the outlook which poisoned our whole public life, penetrated the remotest corners of our thinking, obscured our vision, paralysed our critical faculties and finally rendered many of us incapable of simply sensing or apprehending truth. This is how it was, it is no use denying it.

The author of these lines was executed in Budapest in 1958 at the orders of Moscow.

Since 1956, every successive report has made it clearer that the demand for truth is the motive force of renewal throughout the Soviet empire. It revives the great tradition of the intellectuals which originated in the Enlightenment. Marxist revisionism is an attempt to restore the original humanism of the Enlightenment and to stabilise it against the kind of self-destruction which led to Stalinism. Western writers have ascribed this movement of liberation to a higher level of industrialisation. They are still prisoners of the philosophic corruption which has plunged man's hopes into darkness. Nicolas Gimes and his comrades fought to redeem man's faith in truth from this corruption.

I have argued that a general respect for truth is all that is needed for society to be free. The way freedom and truth have proved identical in the battle against Stalinism bears out my views. I hope to see a modern theory of freedom, conceived on these lines, emerging from this battle.

Oxford
December 1963