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GENIUS IN SCIENCE

1962

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We accept the results of science, and we must accept them, without having any strict proof that they are true. Strictly speaking all natural sciences are inexact. They could all conceivably be false, but we accept them as true because we consider doubts that may be raised against them to be unreasonable. Juries base their findings on the distinction between "reasonable doubts" which they must accept, and "unreasonable doubts" which they must disregard. They are instructed to make this distinction and to do it without having any set rules to rely upon. For it is precisely because there are no rules for deciding certain factual questions of supreme importance that these questions are assigned to the jury to decide them by their personal judgment. The scientist combines the functions of judge and jury. Having applied to his findings a number of specifiable criteria, he must ultimately decide in the light of his own personal judgment whether the remaining conceivable

STRICTLY SPEAKING ALL NATURAL SCIENCES ARE INEXACT

THE SCIENTIST COMBINES THE FUNCTIONS OF JUDGE AND JURY doubts should be set aside as unreasonable.

ALL SCIENTIFIC DISCOVERIES ULTIMATELY REST ON THE SCIENTIST'S PERSONAL JUDGMENT

Once it is recognised that all scientific discoveries ultimately rest on the scientist's personal judgment, the path seems open for unifying the whole sequence of personal decisions, beginning with sighting a problemand then pursuing the problem throughout an enquiry, all the way to the discovery of a new fact of nature.

We shall meet the main features of the principle that controls scientific enquiry—from the dawn of a problem to the finding of its solution—by looking first at its highest actions in the work of genius.

Genius is known for two faculties which may seem incompatible. Genius is a gift of inspiration; poets back to Homer have asked their Muse for inspiration, and scientists back to Archimedes have acknowledged the coming of a bright idea as an event that suddenly visited them. But we have also ample evidence of an opposite kind; genius has been said to consist in taking infinite pains, and all kinds of creative pursuits are in fact extremely strenuous.

How can these two aspccts of genius hang together? Is there any hard work which will induce an inspiration to visit us? How can we possibly conjure up an inspiration without even knowing from what corner it may come? And since it is ourselves who shall eventually producc the inspiration, how can it come to us as a surprise?

Yet this is what our creative work actually does. It is precisely what scientific discovery docs: we make a discovery and yet it comes as a surprise to us. The first task of a theory of creativity, and of scientific discovery in particular, must be to resolve this paradox.

WE MAKE A DISCOVERY AND YET IT COMES AS A SURPRISE TO US

The solution can be found on a biological level, if wc identify inspiration with "spon taneous integration" and look out for the effort that induces such integration. Suppose I move an arm to reach for an object: my intention sets in motion a complex integra tion of my muscles, an integration that car ries out my purpose. My intention is about something that docs not yet exist, in other words it is a project, a project conceived by my imagination. So it seems that it is the imagination that induces a muscular integra tion to implement a project that I form in my imagination.

Could we say that this integration is "spontaneous"? I think that in an important sense we can call it spontaneous, for wc have no direct control over it. Suppose a physio logist were to demonstrate to us all the mus cular operations by which wc have carried out our action, wc would be amazed at the wonderful mechanism that wc had contrived in achieving our project. Wc would find that wc had done something that profoundly surprises us.

This exemplifies a principle that controls all our deliberate bodily actions. Our imagination, thrusting towards a desired result, induce in us an integration of parts over which we have no direct control. We do not perform this integration: we cause it to happen. The effort of our

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imagination evokes its own implementation.

And the way we evoke a desired event by the action of our body offers in a nutshell a solution of the paradox of genius. It suggests that inspiration is evoked by the labours of the thrusting imagination and that it is this kind of imaginative labour that evokes the new ideas by which scientific discoveries are made.

INSPIRATION IS EVOKED BY THE LABOURS OF THE THRUSTING IMAGINATION AND THAT IT IS THIS KIND OF IMAGINATIVE LABOUR THAT EVOKES THE NEW IDEAS BY WHICH SCIENTIFIC DISCOVERIES ARE MADE.

These conclusions may seem too fast; but they will be confirmed and enlarged by passing from voluntary action to visual perception. The constancy of objects seen is achieved by an integration of clues which takes place beyond our direct control. We see objects and their surroundings coherently by integrating two to three snapshots per second, which present to us overlapping images ranging over the area before us. The intelligent scanning of these consecutivc snapshots shows that our imagination is at work guiding our integration. And we can add that in case of any difficulty in recognising what it is that wc see, the imagination explores alternative possibilities by letting our eyes move around to look for such possibilities.

Different branches of science are based on different ways of seeing. When an object is composed of parts that function jointly, our vision integrates the sight of these parts into the appcarancc of a coherent functioning entity. This is how the engineer, who knows the way a machine works, sees the machine as a working whole. Such integration underlies all biology and psychology. The view of an organism, the sight of an intelligent animal, the image of a human person, are all based on such integrations. We may call these visual integrations spontaneous because their parts are not directly controlled and often cannot even be directly noticed. The process of scientific discovery consists generally of such integrations evoked by the work of the imagination.

THE PROCESS OF SCIENTIFIC DISCOVERY CONSISTS GENERALLY OF SUCH INTEGRATIONS EVOKED BY THE WORK OF THE IMAGINATION.

POWERS OF ANTICIPATION

The progress of discovery falls into three main periods. The *first* is the sighting of a problem and the decision to pursue it; the *second*, the quest for a solution and the drawing of a conclusion; the *third,* the holding of the conclusion to be an established fact.

I have spoken of the way our eyesight organises consecutive snapshots by scanning

them in an intelligent way, and how, in case of difficulty, the imagination explores alterna tive possibilities to lind out what it is that wc are seeing. These efforts of our eyesight are based on the assumption that any curious things before us are likely to have some hidden significance. Scientists speculating about strange things in nature act on a similar assumption. They try to interpret the facts they know, and go on collecting more facts, in the hope that these will reveal a coherence that is of interest to science. Such is the act of seeing a problem and pursuing it.

IN ACCEPTING THE TASK OF PURSUING A PROBLEM, THE SCIENTIST ASSUMES IT TO BE A GOOD PROBLEM

And here we meet a strange fact. In accepting the task of pursuing a problem, the scientist assumes it to be a good problem: a problem that he can solve by his own gifts and equip ment and that it is worth undertaking in com parison with other available possibilities. He must estimate this; and such estimates are guesses. But such guesses have proved suffici ently good to secure the progress of scientific enquiry with a reasonable degree of efficiency. It is rare to come across years of futile efforts wasted, or else to find that major opportunities were patently missed. Indeed, the opportuni ties for discovery are so effectively exploited that the same discovery is often made simul taneously by two or three different scientists. There is no doubt, therefore, of the scientist's capacity to assess in outline the course of an enquiry that will lead to a result which, at the time he makes his assessment, is essentially indeterminate.

SCIENTIFIC DISCOVERY IS IN ESSENCE AN EXTENSION OF PERCEPTION

How can we explain this capacity? I have said that scientific discovery is in essence an extension of perception. Remember how the different images of an object presented to our eyes from various distances, at different angles and in changing light, are all seen Jointly as one single object; and that it is in terms of this coherence that our eyes perceive real things. This bears deeply on science. Copernicus laid the foundations of modern scicnce by claiming his discovery of the heliocentric system in these very terms. He showed that his system inclu ded a parallelism between the solar distances of the planets and their orbital periods, and on this coherence he based his insistence that his system was no mere computing device, but a real fact.

But such claims to know reality are ques tioned by our anti-metaphysical age. Can we define what it means to claim that an object is real? 1 think we can.

To say that an object is real is to anticipate that it will manifest its existence indefinitely hereafter. This is what Copernicus meant by insisting that his system was real. Copernicus anticipated the coming of future manifestations of his system, and these were in fact discovered by later astronomers who had ac cepted his claim that his system was real. *We can conclude then that, in nature. the coherence of an aggregate shows that it is real and that the knowledge of this reality foretells the coming of yet unknown future manifestations of such realty. This concept of reality extended to include all the phases of a scientific enquiry. It explains the way discovery is anticipated. from the sighting of a problem to finding its solution.*

TO SAY THAT AN OBJECT IS REAL IS TO ANTICIPATE THAT IT WILL MANIFEST ITS EXISTENCE INDEFINITELY HEREAFTER

THE KNOWLEDGE OF THIS REALITY FORETELLS THE COMING OF YET UNKNOWN FUTURE MANIFESTATIONS OF SUCH REALTY

But let me first stop for a moment to recall the antecedents which led to this theory. I began my work on the nature of science twenty-five years ago, guided by the idea that we make scientific discoveries in the same way we strain our eyesight to perceive an obscure object; and that in this effort we are guided by anticipating to some extent the direction which will prove most fruitful.

> A potential discovery (I wrote) 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[1].

(1) Michael Polanyi, Science, *Faith and Society* (Oxford University Press, 1946: Phoenix Edition, 1964), p. 33

For years I have written about this kind of anticipation[2]. At one stage I was joined in this idea by George Polya[3] whose observations of mathematical discovery I had relied on from the start of my enquiry. And more recently I met with a brilliant description of anticipation in the posthumous work of C. F. A. Pantin, who wrote that:

(2) Michael Polanyi, "Problem Solving", *British Journal of Philosophy of Science*, vol. VIII (August 1957), pp. 89-103

(3) George Polya, *Mathematical Discovery* (1965), vol. II, p. 63

> [Intuition] does not only suddenly present solutions to our conscious mind, it also includes the uncanny power that somehow wc know that a particular set of phenomena or a parti cular set of notions arc highly significant: and we arc aware of that long before wc can say what that signification is[4].

(4) F. A. Pantio, *The Relation between the Sciences* (ed. Pantin and Thorpe), (1968), p. 121- 122. We may look aho at other creative works. Kant has described in the *Critique of Pure Reason* the pari of anticipation in the pursuit of philosophic problems. He wrote: "It is unfortunate that not until we have unsystematically collec ted observations for a long time to serve as build ing materials, following the guidance of an idea which lie* conccalcd in our minds, and indeed only after we have spent much time in the technical disposition of these materials, do wc first bccome capable of viewing the idea in a clearer light and of ouilining it architectonically as one

But only now can I see at last an explanation for such anticipations. I now see that the anticipations offered to as by good problems should be understood in the same way as the anticipations aroused by all true facts of nature. Thus, when a coherent set of clucs presents us with the sense of a hidden reality in nature, we are visited by an anticipation similar to that which we feel in seeing any object already recognised to be real. The expectations attached to a good problem differ only in their dynamic intensity from the expectations that will be attached to any facts, eventually to be discovered in the end, once the problem has been solved. Of course, the sense of reality implied in adopting a problem is pointing more clearly in a particular direction. And the results anticipated in this kind of reality arc cxpectcd to appear sooner than the prospects implied in affirming the reality of an established fact. But I regard such differences as mere matters of degree.

whole according to the intentions of reason... H. W. Janson (*History of Art*, 1962, p. II) describes anticipation in making a painting. "It is a strange and risky bovine** in which the maker never quite know* what he is making until he has actually made it. or to put it another way. it is a game of find-and-seek in which the seeker is not sure what he is looking for, until he has found iL.. Northrop Frye (T. S. Eliot, 1963. p. 28) speaks of Eliot's account of anticipation: "The poet has no idea of what he wants to say until he has found the words of his poem"... "[He] may not know what is coming up, but whatever it is, his whole being is bent on realising it." Anticipations of this kind resolve the problem of Meno in which Plato questions the possibility of pursuing an enquiry in our inevitable ignoranco of what we are looking for.

The whole of science, as it is known to us, has come into existence by virtue of good problems that have led to the discovery of their solution. The fact that scientists can espy good problems is therefore a faculty as essential to science as is the capacity to solve problems and to prove such solutions to be right. In other words, the capacity rightly to choose a line of thought the end of which is vastly indeterminate, is as much part of the scientific method as is the power of assuring the exactitude of the conclusions eventually arrived at. And both faculties consist in recognising real coherence in nature and sensing its indeterminate implications for the future.

PROBLEMS ARE DISCOVERED BY A ROAMING SPECULATIVE IMAGINATION

This conclusion fulfils in substance my hopes of finding the same principles of personal judgment at work at all stages of a scientific enquiry, from the sighting of a problem to the discovery of its solution. Problems are discovered by a roaming speculative imagination, and once a problem is adopted, the imagination is thrust in the direction of the problem's expectations. This evokes new ideas of coherence which, if true, reduce the indeterminacy of the enquiry. The speculative or experimental examination of these ideas then directs a new thrust of the imagination that evokes yet further surmises; and so the pursuit is narrowed down ever further, until eventually an idea turns up which can claim to solve the problem.

This rough sketch must suffice for the moment to outline the sequence of "infinite pains" that finally evoke a surprise, claiming to be the solution of the problem.

RATIONALITY TO THE RESCUE?

But scientific opinion has been reluctant to accept the fact that the scientist is guided essentially by a vague sense of still unrevealed facts. Hence textbooks of physics have taught for dccadcs that Einstein discovered relativity as an explanation of Michelson's observation that the earth's rotation causes no flow in the surrounding ether. And when I pointed out about twelve years ago that this was a pure invention[5], the only response I evoked was from Professor Adolf Grtinbaum of Pittsburgh[6] who said that my description of Einstein's way to discovery was like Schiller's story that his poetic inspiration came to him by smelling rotten apples. Fortu-

(5) Michael Polanyi, *Personal Knowledge* (1958), pp. 9-13

(6) Adolf Grumbaum, *Philosophical Problems of Space and Time* (1963), pp. 385-386

nately, a study recently published by Professor Gerald Holton has shown at last, in great detail, that 1 bad been right[7].

(7) Gerald Holton, "Einstein, Michelson and the "Crucial Experiment" ", *Isis*, vol. 60 (1969), pp. 163-197

I have mentioned this story to illustrate the temper of our age which prefers a tangible explanation to one relying on more personal powers of the mind, even though the plain facts do show these lew tangible forces at work. The theory of scientific discovery most influential today expresses this preference by dividing the process of discovery sharply into the choice of a hypothesis and the testing of the chosen hypothesis. The first part (the choicc of a hypothesis) is deemed to be inexplicable by any rational procedure, while the second (the testing of the chosen hypothesis) is recogniscd as a strict procedure forming the scientist's essential task.

This theory of scientific discovery would save the strictness of sciencc by declaring that scientific discoveries arc merely tentative hypo theses which can be strictly tested by confront ing their implications with experience. And that if any of the implications of a hypothesis con flicts with experience, the hypothesis must be instantly abandoned; that indeed, even if the hypothesis is accepted on the grounds of having been confirmed in its predictions, it will ever remain on trial ready to be abandoned if any cxpcricncc turns up that contradicts any of its claims. We arc told that unless a hypothesis produces testable conclusions it should be disregarded as lacking any substantial significance[8].

(8) Clearly, the position to which I am referring Is that which Sir Karl Popper stated in Logic der Forschung (1934). translated

Let me test this theory. There may be cases into English as *The Logic of Scientific Discovery* (1946). It is this statement that has been most widely influential and though it was modified in some parts in Popper's *Conjectures and Refutations* (1963). the changes do not substantially affect the principles of "rcfutationaihrn" which I am examining here.

where a scientific discovery was made and only claimed as such after some additional implica tions of it had been tested; but there is plenty of evidence to show that this is not necessary and is indeed, often impracticable.

On 11 November 1572 Tycho Brahe obser ved a new star in Cassiopeia, and this discovery refuted the Aristotelian doctrine of an unchangeable empyrean. This happened before the days of the tclcscopc, and indeed the same observation was also made in China. The discovery was complete without produc ing testable implications, exactly as the erup tion of Vesuvius (on 24 August, A.D. 79) was established as a fact, without any test of its vast implications. Or take Kepler's dis covery that for the six then known planets the square of the orbital period was pro portional to the cube of the solar distance. The figures underlying this discovery had been known for eighty years or more; I happened to test the relative solar distances of the planets made available by the work of Copernicus aud found that they agree with Kepler's Third Law within two per cent All that Kepler did was to recognise this relationship, which is his Third Law. Yet Kepler hailed his discovery as the crowning of his search for celestial harmony, even though no testable implications of it were known at the time and indeed for a long time after his death.

ADMITTEDLY, MANY DISCOVERIES WERE NOT MADE AT ONE STROKE.

Admittedly, many discoveries were not made at one stroke. But of these too many fail to exemplify the orthodox "hypothetico-deductive model." On 13 March 1781, William Herschd observed a slowly moving nebulous disk which he first took to be a comet; but, after a few weeks of watching its motion, he recognised it as a new planet, to be named Uranus. Later on, Leverrier and Adanvs, basing themselves on the irregularities of Uranus, derived the existence of one more planet, and the pre diction of its position promptly led to its discovery. It was named Neptune. Thus the existence of Uranus and Neptune were claimcd the moment they were observed and this observation completed their discovery without regard to particular testable implications.

Turning to physics, we can take Max von Laue's discovery of the diffraction of X-rays in 1912 as a parallel case to this. A conversation with P. P. Ewald aroused in Laue the idea that X-rays would show optical diffraction when passing through a crystal. His attempt to find experimental help to test his idea met with opposition from the director of the laboratory, but when his request finally prevailed, the result confirmed his expectation, and he announced his discovery which was accepted on this evidence.

Further, we sometimes find examples of "beautiful" discoveries neither based on any new observation nor predicting anything which would confirm or refute them—ex. in theo retical work in physics and physical chemistry. Van't HofTs derivation of the chcmical mass action law from the Second Law of Thermo dynamics was a fundamental discover)' based only on known

WE SOMETIMES FIND EXAMPLES OF "BEAUTIFUL" DISCOVERIES NEITHER BASED ON ANY NEW OBSERVATION NOR PREDICTING ANYTHING WHICH WOULD CONFIRM OR REFUTE THEM facts and predicting nothing. In a period extending close on half a century, no one has been able to find a test for the statistical interpretation of quantum mechanics that we owe to Max Born. Its radically new conception of physical laws as predicting only the probability and not the actual course of events controlled by the law is generally ac cepted today, though it was originally grounded on no new facts and has never offered factual implications that could test it.

SUCH UNEMPIRICAL THEORIES CAN BE OF SUPREME IMPORTANCE IN ALL THE EXPERIMENTAL SCIENCES, INCLUDING BIOLOGY.

Such unempirical theories can be of supreme importance in all the experimental sciences, including biology. Darwinism is an example of it and indeed in two senses. First, for seventy years Darwinism was accepted by science, even though its evolutionary mechan ism could not be understood in terms of known facts, and sc-cond, up to this day no such empirical implications of it arc known which, if experimentally tested, could disprove the theory. The second point is widely recog nised, so I shall only demonstrate the first.

During the first forty years following the publication of the *Origin of Species* in 1859, it bc-camc increasingly clear that the kind of variations known at that time were not sufficiently hereditary to form the basis of a selective process producing evolutionary transformation[9]. Yet the authority of scientific opinion continued to support the theory of evo lution by natural selection and has spread its deep influence on the world view of humanity. After the discovery of Mendelian mutations in 1900 the opposite dif-

(9) C. D. Darlington, *Darwin's Place in History* (l960), p. 40. Professor Darlington has described in Chapter 8, entitled "The Retreat from Natural Selection", how in the successive editions of Darwin's *Origin of Species* natural selection is gradually abandoned and evolution "shored up with Lamarckian inheritance".

ficulty arose. These varia tions were hereditary, but they were much too massive for producing a process of gradual adaptation. Yet the acccp-tancc of Darwinism as our world view, supported by science, remained unshaken-—while the new contradic tions remained unexplained for another three dccadcs. This difficulty may have been over come since 1930 through the rise of neo-Darwinlsm; and if this new theory holds, the previous disregard of the fact that the theory of natural selection conflicted with the known laws of nature may turn out to have been justified[10].

To sum up, we have seen examples to show that important scientific discoveries can be made at a glancc and established without any subsequent tests; and that there have been great theoretical discoveries which had no testable experimental content at all. We have seen also that a theory interpreting in a novel way a vast range of experience was accepted by science, and then firmly held for many years by science, though its assumptions contradicted the laws of nature as known at the time; and also that it continues to be held by sciencc, as other important theories arc, though it has never been testable by predictions that could be empirically refuted.

(10) The present situation was described as follows by Julian Huxley, Evolution the Modern Synthesis (1942), p. 116: and repeated in the same words in its revised version in 1963.
"It must be admitted that the direct and com plete proof of the utilization of mutations in evolution under natural conditions has not yet been given.... Thus it is inevitable that for the present wc must rely mainly on the con vergence of a number of separate lines of evi dence each partial and indirect or incomplete, but severally cumulative and demonstrative."
J. Maynard Smith in an article entitled ~The Status of Neo-Darwinism," pp. 82-89, in *Towards A Theoretical Biology*, (cd. C. H. Waddington, 1969), has listed some evidence as proving that Neo-Darwinism is not "tautological". But he merely shows, as Huxley docs, that the evidence so far supports the theory.

PERSONAL JUDGMENT IN SCIENCE

My own theorv of scientific knowledge is, and

SCIENCE IS THE OUTCOME OF DELIBERATE INTEGRATION REVEALING A HITHERTO HIDDEN REAL ENTITY

AS I CAME TO REALISE THAT ALL SUCH INTEGRATION IS LARGELY BASED, LIKE PERCEPTION ITSELF, ON TACIT ELEMENTS OF WHICH WE HAVE ONLY A VAGUE KNOWLEDGE, I CONCLUDED THAT SCIENCE TOO WAS GROUNDED ON AN ACT OF PERSONAL JUDGMENT

has bocn from the start twenty-five years ago, that science is an extension of perception. It is a kind of integration of pares to wholes, as *Gestalt* psychology has described; but in contrast to *Gestalt*, which is a mere equilibration of certain bits to form a coherent shape, it is the outcome of deliberate integration revealing a hitherto hidden real entity. There are no strict rules for discovering things that hang together in nature, nor even for telling whether we should accept or reject an apparent coherence as a fact There is always a residue of personal judgment involved in deciding whether to accept or reject any particular piece of evidence be it as a proof of a true regularity or, on the contrary, as a refutation of an apparent regularity. This is how I saw and accepted the fact that, strictly speaking, all empirical science Is inexact. And as I came to realise that all such integration is largely based, like perception itself, on tacit elements of which we have only a vague knowledge, I concluded that science too was grounded on an act of personal judgment.

To show this, I became for many years a scandal-monger, collecting cases where the most generally accepted rules of scientific procedure had been flaunted, and flaunted to the advantage of science. My first such case showed that even though a new idea conflicts from the start with experience, it may be generally accep ted by science. The periodic system of elements shows that the sequence of rising atomic weights produces a striking pattern of the ele ments in respect of their chemical charactcr. But two pairs of elements fit into the pattern only the wrong way round, that is, in the direction of decreasing weights. Yet at no time has this caused the system to be called in question, let alone to be abandoned.

Another example: the idea that light is com posed of particles was proposed by Einstein—and upheld, still unexplained, for twenty years—in spite of its being in sharp conflict with the well-established wave nature of light. Commenting on the later history of these eases, (which were among my first scandals), I concluded that any exception to a rule may involve not its refutation but the elucidation of its deeper meaning.

And I went on to declare that the process of explaining away observed deviations from accepted teachings of science is in fact indis pensable to the daily routine of research. In my laboratory—I said—I find the laws of nature formally contradicted every day; but I explain these events by the assumption of experimental eiror. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discover. Such things have often happened in the history of science. Yet I shall continue to explain away my odd results, for if every anomaly observed in a laboratory were taken at its face value, research would degenerate into a wildgoose chase after fundamental novelties.

THE PROCESS OF EXPLAINING AWAY OBSERVED DEVIATIONS FROM ACCEPTED TEACHINGS OF SCIENCE IS IN FACT INDIS PENSABLE TO THE DAILY ROUTINE OF RESEARCH

But these products of my early scandal mon-

(11) W. Heisenberg, "Theory, Criticism and a Philosophy", in *From a Life of Physics*, special supplement of the Bulletin of the International Atomic Energy Agcncy (Vienna), pp. 36-37. The publication is quite recent: the conversation with Einstein must have occured in 1925 or just before this date.

(12) Max Planck in *Positivismus und Reale Aussenwell,* (Leipzig: Akademische Veriagsgesell- schaft, 1931), says (p. 21) "... there exists abso lutely no physical magnitude which can be measured in itself."

gcring were surpassed by a statement of Einstein which recently came to my notice[11]. Werner Heisenberg has told the story how, in the course of shaping his quantum theory, he told Einstein that he proposed to go back from Nils Bohr's theory to quantities that could be really observed. To which Einstein replied that the truth lay the other way round. He said: "Whether you can observe a thing or not depends on the theory which you use. It is the theory which decides what can be observed." Max Planck has also rejected Heisenberg's claim to deal with observable*, on the grounds that science is a theory bearing on observations, but never including observations[12].

The position of observations in the face of prevailing theories is of course precarious. Take once more the famous experiment of Michelson and Morley which demonstrated the absence of the ether drift corresponding to the rotation of the earth. Far from rejoicing at this great discovery, which was eventually to form the main experimental support for Einstein's relativity. Michelson called his result a failure. Professor Holton has told in the paper that 1 quoted before how both Kelvin and Rayleigh spoke of Michelson's result as "a real disappointment", and Sir Oliver Lodge even said that this experiment might have to be explained away. Thus the ether theory, which was firmly supported by the current interpretation of physics, caused the experiment to be distrusted. But when some thirty-five years later the same experiment was repeated (with improved instruments) by D. C. Miller and this time showed the presence of an ether drift, this result was rejected in its turn for by this time relativity had overthrown the ether theory. And this time, of course, the theory was rightly preferred to the experiment.

I have no space here to tell in detail the story that I picked up at the very beginning of my scandal-mongering, of the way scientists of the first rank came out with experiments showing a transformation of elements, because they were encouraged by the radio-active trans mutations discovered by Rutherford. There was one epidemic of such publications from 1907 to 1913 that was evoked by Rutherford's discovery (in 1903) that radioactivity involves a transformation of elements. A second epidemic spread from 1922 to 1928, in response to Rutherford's discovery (in 1919) of an artificial transformation of elements. The observations published during these epidemics would other wise have been cast aside as mere "dirt-effect"[13].

(13) For details see my *Science, Faith and Society* (1946), apêndice 2

Let me add a counter example, where plausibility justly triumphed over observation. 1 have in mind Eddington's derivation from his theory of the universe, developed in the mid- 1920s, that the reciprocal of the "fine-structure constant"—usually denoted by hc/2pie^2—is equal to the number 137. The theory was generally rcjcctcd and this was facilitated by the fact that the experimental value for Eddington's figure was at the time 137-307 with a probable error of only ±0-048. However, by the passing of twenty yean new experiments gave a value of 137 009, which brilliantly confirmed Eddington's theory. But this agreement was rcjcctcd as fortuitous by the overwhelming majority of scientists; and they were right[14].

(14) For further details see *Personal Knowledge* (1958), pp. 43, 151, 158, 160

SCIENCE IS THE RESULT OF AN INTEGRATION, SIMILAR TO THAT OF COMMON PERCEPTION. IT ESTABLISHES HITHERTO UNKNOWN COHERENCES IN NATURE

To sum up: science is the result of an integration, similar to that of common perception. It establishes hitherto unknown coherences in nature. Our recognition of these cohcrcnccs is largely based, like perception is, on clues of which we arc not focally aware and which arc indeed often unidentifiable. Current conceptions of science about the nature of things always affect our recognition of coherence in nature. From the sighting of a problem to the ultimate decision of rejecting still conceivablc doubts, factors of plausibility are ever in our minds. This is what is meant by saying that, strictly speaking, all natural science is an expression of personal judgment.

WIDER VIEWS

The machinery of genius, which I have described, is at work all the way

from the start to the finish of an enquiry. And once we have recognised this mechanism we can sec that wc are ourselves the ultimate masters of its workings. Exactitude is recognised then to be always a matter of degree and ceases to be an all surpassing ideal. The supremacy of the exact sciences is rcjcctcd and psychology',

EXACTITUDE IS RECOGNISED TO BE ALWAYS A MATTER OF DEGREE AND CEASES TO BE AN ALL SURPASSING IDEAL sociology, and tho humanities are sec free from the vain and misleading efforts of emulating mathematical physics.

I started on this line many years ago in a short paper entitled "The Value of the Inexact"[15]. I pointed out that if we insisted on exactitude of procedure, wc would have no chemistry, or at least none to speak of. For chemistry relies for its guidance on judgments of "stability", "affinity", "tendency", as descriptions of chemical processes and also on the skilful application of rules of thumb as guides for acting on such judgments. And the value of the inexact goes much further. It alone makes possible the science of biology. For the structure of living things can be recognised only if we allow our vision to integrate the sight of parts to the view of a coherently functioning entity, an entity which vanishes if analysed in terms of physics and chemistry.

(15) See *Science, Faith and Society,* (1946), chapter II, section II

THE STRUCTURE OF LIVING THINGS CAN BE RECOGNISED ONLY IF WE ALLOW OUR VISION TO INTEGRATE THE SIGHT OF PARTS TO THE VIEW OF A COHERENTLY FUNCTIONING ENTITY

I have defined scientific value as the joint product of three virtues, (I) accuracy, (2) range of theory, and (3) interest of subjoct matter[16]. This triad of values distributes our appreciation evenly over the whole range of sciences. We have then greater exactitude and elegance being balanced by a lesser intrinsic interest of subject matter, or else the other way round. For example, most subjects of modern physics arc interesting only to the scientists, while the horizon of biology ranges over our experience of animals and plants, and of our own lives as human beings. So the glory of mathematical preci-

(16) Michael Polanyi, "The value of the inexact", *Philosophy of Science*, vol. 13 (1936), p. 233 ff.

sion and elegance, in which physics far surpasses biology, is balanced in biology by the much greater interest of its subject matter.

Once science is appraised by a three-fold grading, all scholarship is elevated to the same pride: a pride free of pangs about not being a real science. The foolish hierarchy of Auguste Comte is smashed and flattened out.

I AM NOT MAKING EXCUSES FOR THE INEXACTITUDE OF SCIENCE, NOR FOR OUR PERSONAL ACTIONS

I am not making excuses for the inexactitude of science, nor for our personal actions, which ultimately decide what to accept as the truth in science. I do not see our intervention as a regrettable necessity, nor regard its result as a second-rate kind of knowledge. It appears second-rate only in the light of a fallacy which systematically corrupts our conception of knowledge and distorts thereby wide regions of our culture.