A certain relativity of such outlooks to the standpoint and interests of the thinker may be found to be unavoidable. But I believe that this relativity need not dismay us, provided that we recognise that our diverse worlds of interpretation, including even our empiricism, can be brought into a common venture of communication, grounded in the conviction of a common ethical responsibility.

Dorothy M. Emmet.

(N.b. Some parts of the foregoing were contributed to a discussion of „Science, the Universities and the Modern Crisis", held by the Literary and Philosophical Society of Manchester).

SCIENCE AND REALITY

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This essay contains in abbreviated form the first of the author's three Riddell Memorial Lectures of 1946. It is published here by permission of the Vice-Chancellor of the University of Durham.

I

What is the nature of science ? Given any amount of experience, can scientific propositions be derived from it by the application of some explicit rules of procedure ? Let us limit ourselves for the sake of simplicity to the exact sciences and conveniently assume that all relevant experience is given us in the form of numerical measurements; so that we are presented with a list of figures representing positions, masses, times, velocities, wavelengths, etc. from which we have to derive some mathematical law of nature. Could we do that by the application of definite operations ? Certainly not. Granted for the sake of argument that we could discover somehow which of the figures can be connected so that one group determines the other; there would be an infinite number of mathematical functions available for the representation of the former in terms of the latter. There are many forms of mathematical series — such as power series, harmonic series, etc. — each type of which can be used in an infinite variety of ways to approximate the existing relationship between any given set of numerical data to any desired degree. Never yet has a definite rule been laid down by which any particular mathematical function can be recognised, among the infinite number of those offering themselves for choise, as the one which expresses a natural law. It is true that each of the infinite number of available functions will, in general, lead to a different prediction when applied to new observations, but this does not provide the requisite test for making a selection among them. If we pick out those which predict rightly, we still have an infinite number on our hands. The situation is in fact only changed by the addition of a few more data — namely the 'predicted' data — to those from which we had originally started. We are not brought appreciable nearer towards definitely selecting any particular function from the infinite number of those available.

Now, I am not suggesting that it is impossible to find natural laws; but only that this is not done, and cannot be done, by applying some explicitly known operation to the given evidence of measurements. And to bring my argument a little closer to the actual experience of science, I shall now re-state it as follows. We ask: Could a mathematical function connecting observable instrument readings ever constitute what we are accustomed to regard as a natural law in science ? For example if we were to state our knowledge concerning the path of a planet in these terms: "That setting certain telescopes at certain angles at certain times a luminous disc of a certain size will be observed" — does that properly express a natural law of planetary motion ? No. It is obvious that such a prediction will often prove false even though the underlying proposition on planetary motion was correct: for a cloud may make the planet invisible to the eye, or else the soil may give way under the observatory, or some other of a hundred and one possible errors or obstacles may falsify observation or make it unworkable. Secondly, we should be claiming too little, since the presence of a planet at certain points of space — as postulated by its law of motion — may manifest itself in an indefinite variety of ways, the vast majority of which could not on account of their sheer multitude, ever be explicitly predicted; and many of which may even be unthinkable today as they may be due to arise from yet unknown properties of matter or a host of other factors unknown at present, and yet inherent in our system.

There is in fact an essential feature lacking in both of the foregoing representations of science, which can be perhaps best pointed out by using yet a third picture of science. Suppose we wake up at night to the sound of a noise as of rummaging in a neighbouring unoccupied room. Is it the wind ? A burglar ? A кat ?.... We try to guess. Was that a footfall ? That means a burglar ! Convinced, we pluck up courage, rise and proceed to verify our assumption.

Here are some of the features of a scientific discovery that we had missed before. The theory of the burglar — which represents our discovery — does not involve any definite relation of observational data from which further new observations can be definitely predicted. It is consistent with an infinite number of possible future observations. Yet the theory of the burglar is substantial and definite enough; it may even be capable of proof beyond any reasonable doubt in a court of law. In the light of common sense there is nothing curious in this: it merely makes it clear that the burglar is being assumed to be a real entity. A real burglar. We may even reverse this by saying that science is assuming something real whenever its propositions resemble the theory of the burglar. In this sense an assertion concerning the path of a planet may be said to be a proposition concerning something real;

it being open to verification not only by some definite but also by many as yet quite undefined observations. We often hear of scientific theories gaining confirmation by later observations in a manner described as most surprising and audacious. The feat of Max v. Laue (1912) jointly confirming by the diffraction of X-rays in crystals both the wave nature of the X-rays and the lattice structure of crystals, is often praised as a striking feat of genius. It appears of the essence of scientific propositions that they are capable of bearing such distant and unexpected fruit; and we may conclude therefore that it is also of their essence to be concerned with reality.

A second significant feature of the discovery of the burglar, closely connected with what has just been said, is the way in which it is made. Curious noises are noticed; speculations about wind, rats, burglars follow, and finally one more clue being noticed and taken to be decisive, the burglar theory is established. We see here a consistent effort at guessing — and at guessing right. The process starts with the very moment when certain impressions are felt to be unusual and suggestive, a 'problem' is presenting itself to the mind; it continues with the collection of clues with an eye to a definite line of solving the problem; and it culminates in the guess of a definite solution.

But there is a difference between the solution offered by the burglar theory and that offered by a new scientific proposition. The first selects for its solution a known element of reality — namely burglars — the second postulates an entirely new one. The vast growth of science in the last 300 years proves massively that new aspects of reality are constantly being added to those known before. Whence can we guess the presence of a real relationship between observed data, if its existence has never before been known ?

Let us go back to the process by which we usually first establish the reality of certain things around us. Our principal clue to the reality of an object is its possession of a coherent outline. It was the merit of Gestalt psychology to make us aware of the remarkable performance involved in perceiving shapes at a glance. Yet suppose that instead of the impression made on our eye by an aggregate of white points forming the surface of an egg, we were presented with another, logically equivalent, presentation of these points as given by a list of their spacial co-ordinate values. It would take years of labour to discover the shape inherent in this aggregate of figures — provided it could be guessed at all. The perception of the egg from the list of co-ordinate values would in fact be a feat rather similar in nature and measure of intellectual achievement to the discovery of the Copernican system. We can say therefore that the capacity of scientists to guess the presence of shapes as tokens of reality 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. The scientist's intuition can integrate widely dispersed data, camouflaged by sundry irrelevant connections, and indeed seek out such data by experiments guided by a dim foreknowledge of the possibilities which lie ahead. These perceptions may be erroneous; just as the shape of a camouflaged body may be erroneously perceived in everyday life. I was concerned here only with showing that some of the characteristic features of the propositions of science exclude the possibility of deriving these by definite operations applied to primary observations; and to demonstrate that the process of their discovery must involve an intuitive perception of the real structure of natural phenomena. In the rest of this lecture I shall examine this position further and also point out (in Section IV) the necessity of amplifying it in some important respects.

II

The part played by new observations and experiment in the process of discovery in science is usually over-estimated. The popular conception of the scientist patiently collecting observations, unprejudiced by any theory, until finally he succeeds in establishing a great new generalisation, is quite false. "Science advances in two ways" — remarks Jeans "by the discovery of new facts, and by the discovery of mechanisms or systems which account for the facts already known. The outstanding landmarks in the progress of science have all been of the second kind." As examples he quotes the work of Copernicus, Newton, Darwin and Einstein. We could add Dalton's atomic theory of chemical combination, de Broglie's wave theory of matter, Heisenberg's and Schrödinger's Quantummechanics, Dirac's theory of the electron and positron. In a number of these discoveries predictions of the highest importance were involved which often came to light only years after the discovery was made. All this new knowledge of nature was acquired merely by the reconsideration of known phenomena in a new context which was felt to be more rational and more real.

The assumptions guiding these discoveries were the premises of science, that is the fundamental guesses of science concerning the nature of things. With these premises I shall not deal in detail but only note that great discoveries achieved by the mere reconsideration of known phenomena are a striking illustration of the presence of these premises and a mark of their rightness.

It will be objected — following yet another widespread popular misconception — that even though scientists do occasionally put forward in advance of evidence assumptions that appear *a priori* plausible to them, they only use them as a 'working hypothesis' and are ready immediately to abandon them in face of conflicting observational evidence. This however is either meaningless or untrue. If it means that a scientific proposition is abandoned whenever some new observation is accepted as evidence against it, then the statement is of course tautologous. If it suggests that any new observation which formally contradicts a proposition leads to its abandonment, it is, equally obviously, false. The periodic system of elements is formally contradicted by the fact that argon and potassium as well as tellurium and iodine fit in only in a sequence of decreasing, instead of increasing, atomic weights. This contradiction, however, did at no time cause the system to be abandoned. The quantum theory of light was first proposed by Einstein — and upheld subsequently for 20 years — in spite of its being in sharp conflict with the evidence of optical diffraction.

This position was indeed to be expected on the grounds of our introductory analysis. We had established there that scientific propositions do not refer definitely to any observable facts but are like statements about the presence of a burglar next door — describing something real which may manifest itself in many indefinite ways. We have seen that there exist therefore no explicit rules by which a scientific proposition can be obtained from observational data, and we must therefore accept it also that no explicit rules can exist to decide whether to uphold or abandon any scientific proposition in face of any particular new observation. The part of observation is to supply clues for the apprehension of reality: that is the process underlying scientific discovery. The apprehension of reality thus gained forms in its turn a clue to future observations: that is the process underlying verification. In both processes there is involved an intuition of the relation between observation and reality; a faculty which can range over all grades of sagacity, from the highest level present in the inspired guesses of scientific genius down to a minimum required for ordinary perception. Verification, even though usually more subject to rules than discovery, rests ultimately on mental powers which go beyond the application of any definite rules.

Such a conclusion may appear less strange if we consider the phases through which the propositions of science are usually brought into existence. In the course of any single experimental enquiry the mutual stimulus between intuition and observation goes on all the time and takes on the most varied forms. Most of the time is spent in fruitless efforts, sustained by a fascination which will take beating after beating for months on end and produce ever new outbursts of hope, each as fresh as the last so bitterly crushed the week or month before. Vague shapes of the surmised truth suddenly take on the sharp outlines of certainty, only to dissolve again in the light of second thoughts or of further experimental observations. Yet from time to time certain visions of the truth, having made their appearance, continue to gain strength both by further reflection and additional evidence. These are the claims which may be accepted as final by the investigator and for which he may assume public responsibility by communicating them in print. This is how scientific propositions normally come into existence.

Such propositions can possess therefore no certainty different, except by degree, from that of previous preliminary results — many of which had appeared final at first and only later turned out to have been only preliminary. Which is not to say that we must always remain in doubt, but only that our decision what to accept as finally established cannot be wholly derived from any explicit rules but must be taken in the light of our personal judgment of the evidence.

Nor am I saying that there are no rules to guide verification; but only that there are none which can be relied on in the last resort. Take the most important rules of experimental verification: reproducibility of results; agreement between determinations made by different and independent methods; fulfilment of predictions. These are powerful criteria; yet I could give you examples in which they were all fulfilled and yet the statement which they seemed to confirm later turned out to be false. The most striking agreement with experiment may occasionally by revealed later as it did in these cases, to be based on mere coincidence. Agreement with experiment will therefore always leave some conceivable doubt as to the truth of a proposition and it is for the scientist to judge whether he wants to set aside such doubt as unreasonable or not.

Similar considerations apply of course to the accepted rules of refutation. It is true enough that the scientist must be prepared to submit at any moment to the adverse verdict of observational evidence. But not blindly. That is what I have illustrated by the examples of the periodic system and the quantum theory of light, both upheld in spite of contradicting evidence. There is always the possibility that, as in these cases, a deviation may not affect the essential correctness of a proposition. The example of the periodic system and of the quantum theory of light both show how the objections raised by contribution to a theory may eventually be met not by abandoning it but rather by carrying it one step further. Any exception to a rule may thus conceivably lead, not to the refutation, but to the elucidation of the rule and hence to the confirmation of its deeper meaning.

The process of explaining away deviations is in fact quite indispensable to the daily routine of research. In my laboratory I find the laws of nature formally contradicted at every hour, but I explain this away by the assumption of experimental error. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discovery. 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 my laboratory were taken at its face value, research would instantly degenerate into a wild goose chase after imaginary fundamental novelties.

We may conclude that just as there is no proof of a proposition in natural science which cannot conceivably turn out to be incomplete, so also there is no refutation which cannot conceivably turn out to have been unfounded. There is a residue of personal judgment required in deciding — as scientists eventually must — what weight to attach to any particular set of evidence in regard to the validity of a particular proposition.

III

The propositions of science thus appear to be in the nature of guesses. They are founded on the assumptions of science concerning the structure of the universe and on the evidence of observations collected by the methods of science. They are subjected to a process of verification in the light of further observations according to the rules of science. But their conjectural character remains inherent in them.

Being convinced that there is great truth in science I do not consider its guesses as unfounded. Let me resume therefore my examination of this guesswork and see what method if any can be discovered in its operations.

In science the process of guessing starts when the novice feels first attracted to science and is then attracted further towards a certain field of problems. This guesswork involves the assessment in many particulars of the young person's own yet largely undisclosed abilities and of a scientific material, yet uncollected or even unobserved, to which he may later successfully apply his abilities. It involves the sensing of hidden gifts in himself and of hidden facts in nature, from which two, in combination, will spring one day his ideas that are to guide him to discovery. It is characteristic of the process of scientific conjecture that it can guess, as in this case, the several consecutive elements of a coherent sequence — even though each step guessed at a time can be justified only by the success of the further yet unguessed steps with which it will eventually combine to the final solution. This is particularly clear in the case of a mathematical discovery consisting of a whole new chain of arguments. G. Polya has compared it with an arch where every stone depends for its stability on the presence of the others, and pointed out the paradox that the stones are in fact put in one at a time. The sequence of operations leading up to the chemical synthesis of an unknown body is in the same category; for unless final success is achieved, all the work is largely or entirely wasted. In order to guess a series of such steps, an intimation of approaching nearer towards a solution must be received at every step. There must be a sufficient foreknowledge of the whole solution to guide conjecture with reasonable probability in making the right choice at each consecutive stage. The process resembles the creation of a work of art which is firmly guided by a fundamental vision of the final whole, even though that whole can be definitely conceived only in terms of its yet undiscovered particulars — with the remarkable difference, however, that in natural science the final whole lies not within the powers of our shaping, but must give a true picture of a hidden pattern of the outer world.

I have previously suggested that the process of discovery is akin to the recognition of shapes as analysed by Gestalt psychology. Köhler assumes that the perception of shapes is caused by the spontaneous reorganisation of the physical traces made by sense impressions inside our sense organs. He assumes that these traces somehow interact and coalesce to a dynamic order, the formation of which produces in the observer the perception of a shape. We may follow up our parallel between discovery and Gestalt perception by regarding the process of discovery as a spontaneous coalescence of the elements which must combine to its achievement. Potential discovery may be thought to attract the mind which will reveal it — inflaming the scientist with creative desire and imparting to him a foreknowledge of itself; guiding him from clue to clue and from surmise to surmise. The testing hand, the straining eye, the ransacked brain, may be thought to be all labouring under the common spell of a potential discovery striving to emerge into actuality.

The conditions in which discovery usually occurs and the general way of its happening certainly show it to be much rather a process of emergence than a feat of operative action. Operational skill, such as the facility for carrying out rapidly and accurately a large number of measurements and calculations counts for little in a scientist. There exist many excellent manuals on methods of computation and on every form of experimental technique. There are specifications for testing materials and rules for drawing up statistics. There are also manuals for triangulation and the drawing of exact maps. But there are no manuals prescribing the conduct of research; clearly because its method cannot be definitely set out. Only routine progress can be made — such as the production of good maps and charts of all kind — by rules alone. The rules of research cannot usefully be codified at all. Like the rules of all other higher arts, they are embodied in practice alone. There is a popular belief that a procedure of empirical discovery has been revealed and established by Francis Bacon. But actually his prescription of making discoveries by collecting all the facts and passing them through an automatic mill was a travesty of research. The study of heuristics, i.e. the enquiry into the general method of solving problems in mathematics has been recently revived by G. Polya. But his excellent little book only proves that discovery, far from representing a definite mental operation, is an extremely delicate and personal art which can be but little assisted by any formulated precepts.

There can actually be no doubt that, at any rate in mathematics, the most essential phase of discovery represents a process of spontaneous emergence. This was first described by Poincaré, who in "Science et Méthode" has analysed the way some of his own great mathematical discoveries were made. He noted that discovery does not usually occur at the culmination of mental effort — the way you reach the peak of a mountain by putting in your last ounce of strength — but more often comes in a flash after a period of rest or distraction. Our labours are spent as it were in an unsuccessful scramble among the rocks and in the gullies on the flanks of the hill and then when we would give up for the moment and settle down to tea we suddenly find ourselves transported to the top. All the efforts of the discoverer are but preparations for the main event of discovery, which eventually takes place — if at all — by a process of spontaneous mental reorganisation uncontrolled by conscious effort.

This outline of mathematical discovery has been confirmed by all subsequent writers and a similar rhythm has been observed over a wide field of other creative activities of the mind. The four phases observed in mathematical discovery, namely Preparation, Incubation, Illumination and Verification (as Wallas has called them) were found also in the course of discovery in natural science and they can be traced similary through the process leading to the creation of a work of art. They are very clearly reproduced also in the mental effort leading to the recovery of a lost recollection. The solution of riddles, the invention of practical devices, the recognition of indistinct shapes, the diagnosis of an illness, the identification of a rare species, and many other forms of guessing right seem to conform to the same pattern. Among these I would include also the prayerful search for God. The report of St. Augustine of his long labours to achieve faith in Christianity, abruptly culminating in his conversion, which he immediately recognised as final, and followed up by the lifelong vindication of the suddenly acquired faith, certainly reveals all the characteristic stages of the creative rhythm.

All these processes of creative guesswork have in common that they are guided by the urge to make contact with a reality, which is felt to be there already to start with, waiting to be apprehended. That is why the egg of Columbus is the proverbial symbol of great discovery. It suggests that great discovery is the realisation of something obvious; a presence staring us in the face, waiting until we open our eyes.

In this light it may appear perhaps more appropriate to regard discovery in natural sciences as guided not so much by the potentiality of a scientific proposition as by an aspect of nature seeking realisation in our minds. The process of scientific intuition is then brought into analogy with extra-sensory perception as established by Rhine (1934). It would appear particularly kindred to the acts of pre-cognition or apparent clairvoyance, that is the guessing of objects not known to anyone. The intuitive phase of natural discovery and extra-sensory perception have it in common that they rely on an effort of mental concentration to evoke the knowledge of a real thing never seen before. There is ample evidence that, like extra-sensory perception, heuristic intuition works in a fairly determinate fashion. Two scientists faced with a similar set of facts will often hit on the same problem and discover the same solution to it. Coincident or nearly coincident discoveries by independent investigators are quite common and would be even more frequently observed but for the fact that rapid publication of an earlier successful piece of work often prevents the completion of others which would soon follow after. Therefore, when denying that discovery can ever be achieved by carrying out a set of definite operations we need not place the process altogether outside the laws of nature but may continue to regard its course as closely limited by the circumstances facing the investigator. (The factors lying outside the control of circumstance will be dealt with in Section IV).

But the study of extra-sensory perception may have further lessons for the understanding of intuition. One of the most curious coincidences in the history of science was the almost simultaneous discovery of quantummechanics by Heisenberg and Born in the form of matrices and by Schrödinger in the form of wave mechanics, for in this case the two claims were first considered as conflicting. The starting points of the two theories and their presentations of the problem, their whole mathematical apparatus were different; and above all — as Schrödinger pointed out in his paper eventually establishing the mathematical identity of the two — their departure from classical mechanics lies in diametrically opposite directions. It seems most reasonable to describe this event by saying that both investigators had an intuitive perception of the same hidden reality present in nature, but that they drew different descriptions of it; so different that on comparing them they thought them to represent disparate objects. Actually, Dirac was soon to prove that both representations were considerably off the mark, as they were in conflict with relativity. When corrected for this shortcoming the formulation of quantummechanics was found to be once more transformed practically out of recognition. This seems to conform to the experience of extra sensory perception. When the drawing of an object is sensed by telepathy or precognition there is no tendency to reproduce its physical outline independent of its meaning but on the contrary "....everything seems to happen (writes Mr. Whateley Carington [1]) much more as if those who scored hits had been told, 'Draw a Hand' for example, (rather) than 'Copy this drawing of a Hand'. It is, as one might say, the 'idea' or 'content', or 'meaning' of the original that gets over, not the form." Thus we may think of Heisenberg and Schrödinger both penetrating to the same meaning but drawing different pictures of it; so different that they did not themselves recognise their identical meaning.

It is tempting to include in this picture also the fact, which I have heard mentioned with surprise among mathematicians, than when a problem which had appeared insoluble for a long time is finally solved, there are often discovered a series of solutions which appear to be quite independent of one another. This could be accounted for by assuming that intuition had sensed a reality of which these various solutions represent different descriptions or aspects. Again among mathematicians I have heard a series of discoveries by one person described as follows. The first discovery is like a solitary island in a borderless expanse of sea. Then a second and third island are discovered without any apparent connection. But gradually it becomes clear that the waters are ebbing away in mass and leaving behind what were at first little isolated islands as the peaks of one great chain of mountains. That is precisely what one would expect to happen if intuition first sensed the fundamental chain of thought, i.e. the moun-

[1]) Telepathy, p. 36.

tain range, and consciousness then proceeded to describe it little by little. Actually, these unusual processes do not actually differ in essence from the ordinary event of a hidden chain of mathematical reasoning being discovered by a series of stepwise advances.

Lastly I mention with some hesitation, but with the conviction that they must be at least tentatively considered in this context, the curious coincidences between theoretical and experimental discovery, of which some remarkable cases occurred in the last 20 years or so. In 1923 de Broglie suggested that electrons may possess wave nature and in 1925 Davisson and Germer, not knowing of this theory, made their first observations of the phenomenon soon after to be recognised as the diffraction of these waves. The prediction of the positive electron, which was implied in Dirac's relativistic quantummechanics of 1928, was confirmed by the discovery of the particle by Anderson in 1932, who had no knowledge of Dirac's work. And we may add the prediction of the meson by Yukawa's theory of nuclear fields (1935) and its contemporaneous discovery in cosmic rays, finally established by Anderson (1938). Could it be that the same intuitive contact guided these alternative approaches to the same hidden reality ?

Intuition is always imperfect. Different pictures of the same reality will be of unequal value and most of them will contain but a vague or excessively distorted form of the truth. We must also consider the possibility of completely erroneous shots in the dark. These are common enough in all forms of guesswork as well as in tests of extra-sensory perception. If the mind is uninformed by intuitive contact with reality, it is bound to place unreal and fruitless interpretations on the evidence before it. A passer-by called in from the street on chance to conduct scientific investigations would undoubtedly demonstrate this clearly enough.

But if science is but guesswork, why consider one guess better than another ? In other words, what, if any, is the basis for considering a proposition of science as valid ? We shall answer this question in stages throughout the subsequent lectures. At the moment we are only claiming that who ever accepts natural science, or any part of it, as true, must recognise also our faculty to guess the nature of things in the outer world.

The two somewhat disparate formulations of discovery achieved up to this point — namely (1) spontaneous organisation of mind and clues to the realisation of potential discovery and (2) extra-sensory perception of reality called into consciousness by the aid of relevant clues — would become identical if we were to assume that the ordinary perception of Gestalt includes a process of extra-sensory perception. That is, if sense impressions were normally accompanied by an extra-sensory transmission of the meaning to be attached to them. The uncertainty of the latter process, as observed in the usual tests of extra-sensory perception, could be taken to account for illusions and other interpretative errors. Such speculations may however appear premature in view of our yet too scanty knowledge of extra-sensory perception. So let us return once more to the closer analysis of scientific discovery.

IV

We have yet to recognise an important element of all personal judgments affecting scientific statements. Viewed from outside as we described him the scientist may appear as a mere truth-finding machine steered by intuitive sensitivity. But this view takes no account of the curious fact that he is himself the ultimate judge of what he accepts as true. His brain labours to satisfy its own demands according to criteria applied by its own judgment. It is like a game of patience in which the player has discretion to apply the rules to each run as he thinks fit. Or, the vary the simile, the scientist appears acting here as detective, policeman, judge and jury all rolled into one. He apprehends certain clues as suspect; formulates the charge and examines the evidence both for and against it, admitting or rejecting such parts of it as he thinks fit, and finally pronounces judgment. While all the time far from being neutral at heart, he is himself passionately interested in the outcome of the procedure. For he must be: otherwise he will never discover a problem at all and certainly not advance towards its solution. "....to solve a serious scientific problem (writes Polya) will-power is needed that can outlast years of toil and bitter disappointments...." "We are elated when our forecast comes true. We are depressed when the way which we have followed with some confidence is suddenly blocked, and our determination wavers." There is a strong temptation here to avoid discomfiture by paying insufficient attention to such evidence as obstructs our path. Starting from some intuitive preconception of the truth, and straining every nerve to prove this to be correct — it may be very difficult for the scientist not to overshoot the mark in trying to verify his suppositions. The Bible says: "Correct a wise man and he will love you." The scientist ought to be delighted when his theory, supported by a series of previous observations, appears to collapse in the light of his latest experiments. If he was wrong, then he has just escaped establishing a falsehood and been given a timely warning to turn in a new direction. But that is not how he feels. He is dejected and confused, and can only think of possible ways of explaining away the obstructive observations.

And of course there is always the possibility that this may in fact be just the right thing to do. This may be precisely one of those cases when one has to disregard exceptions to start with and leave them for later consideration. His emotion, born of an intuition which penetrates deeper than the day to day evidence, may be quite right, and his correct procedure may be to persevere in following its guidance, even against the apparent evidence.

I have said before that problems of this kind can be resolved by no established rule and that the decision to be taken is a matter for the scientist's personal judgment; we now see that this judgment has a moral aspect to it. We see higher interests conflicting with lower interests. That must involve questions of conviction and of faithfulness to an ideal; it makes the scientist's judgment a matter of conscience.

Faithfulness to the scientific ideals of care and honest self-criticism is of course indispensable even for the execution of the simplest jobs in the workshop of science. It is the first thing that a student is taught on being apprenticed to science. But, alas, many students only learn to be "conscientious" in the sense of being pedantic and sceptical, which may be paralysing to all advance in research. Scientific conscience cannot be satisfied by the fulfilment of any rules, since all rules are subject to its own interpretation. To verify references for example is a matter of mere routine conscientiousness and not of the kind of conscience of which I am thinking here. But real scientific conscience is involved in judging how far other people's data can be relied upon and avoiding at the same time the dangers of either too little or too much attention. And similarly all the more difficult decisions to be taken in the pursuit of a scientific investigation and its subsequent publication and public defence, involve matters of conscience, each of which is a test for the scientist's sincerity and devotion to scientific ideals.

The scientist takes complete responsibility for every one of these actions and particularly for the claims which he puts forward. If his statements are confirmed by others, in whatever form and in whatever manner, even though quite unthought of at the time when he first propounded them, he will claim to have been right. And conversely, if his work is proved wrong he will feel that he has failed. He cannot plead to have observed the rules, or to have been misled by other investigators' evidence or his own collaborators', or that he could not at the time have made the tests which eventually disproved his thesis. Such reasons can serve to explain his error but they can never justify it — for he is bound to no explicit rules and is entitled to accept or reject any evidence at his own discretion. The scientist's task is not to observe any allegedly correct procedure but to get the right results. He has to establish contact, by whatever means, with the hidden reality of which he is predicating. His conscience must therefore give its ultimate assent always from a sense of having established that contact. And he will accept therefore the duty of committing himself on the strength of evidence which can, admittedly, never be complete; and trust that such a gamble, when based on the dictation of his scientific conscience, is in fact his competent function and his proper chance of making his contribution to science.

We can clearly distinguish in all these phases of discovery the two different personal elements which enter into every scientific judgment and make it possible for the scientist to be judge in his own case. Intuitive impulses keep arising in him stimulated by some of the evidence but conflicting with other parts of it. One half of his mind keeps putting forward new claims, the other half keeps opposing them. Both these parties are blind, as either of them left to itself would lead indefinitely astray. Unfettered intuitive speculation would lead to extravagant wishful conclusions; while rigorous fulfilment of any set of critical rules would completely paralyse discovery. The conflict can be resolved only through a judicial decision by a third party standing above the contestants. The third party in the scientist's mind which transcends both his creative impulses and his critical caution, is his scientific conscience. We recognise the note struck by conscience in the tone of personal responsibility in which the scientist declares his ultimate claims. This indicates the presence of a moral element in the foundations of science; this point is elaborated much further in the author's Riddell Memorial Lectures of 1946.