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Experiment and Discovery

Ever since Bacon's day experimentation has been thought to be so deeply and so very necessarily a part of science that exploratory activities that are not experimental are often denied the right to be classified as sciences at all.

Experiments are of four kinds;[1] in the original Baconian sense, an experiment is a contrived, as opposed to a natural, experience or happening — is the consequence of "trying things out" or even of merely messing about.

The reason why Bacon attached such great importance to experiments of this kind is explained later, but it was of Baconian experiments — those that answer the question "I wonder what would happen if..." — that Hilaire Belloc must have been thinking when he wrote the following passage:

Anyone of common mental and physical health can practise scientific research.... Anyone can try by patient experiment what happens if this or that substance be mixed in this or that proportion with some other under this or that condition. Anyone can vary the experiment in any numbers of ways. He that hits in this fashion on something novel and of use will have fame.... The fame will be the product of luck and industry. It will not be the product of special talent.[2] {70}

Baconian Experimentation. In the early days of science,[3] it was believed that the truth lay all around us — was there for the taking — waiting, like a crop of corn, only to be harvested and gathered in. The truth would make itself known to us if only we would *observe* nature with that wide-eyed and innocent per-ceptiveness that mankind is thought to have possessed in those Arcadian days before the Fall — before our senses became dulled by prejudice and sin. Thus the truth is there for the taking if only we can part the veil of prejudice and preconception and *observe things as they really are;* but alas, we might spend a whole lifetime observing nature without ever witnessing those conjunctions of events that could reveal so much of the truth if by chance they came our way. It is no use, Bacon explained, relying upon good fortune — on "the casual felicity of particular events" — to furnish us with all the factual information we need for apprehending the truth, so we must *devise* happenings and contrive experiences. In John Dee's words, the natural philosopher must become the *"archmaster"* who *stretches* experience. The "electrification" of amber by rubbing and the communication of magnetic properties to iron nails from a lodestone are good examples of the experiments Bacon advocated; again, we know what happens if we distill fermented liquors once, but what happens if we distill the distillate a second time? Only by experimenting in this fashion can we build up that majestic pile of factual information from which, according to the mistaken canon of inductivism (see Chapter 11, "The Scientific Process"), our understanding of the natural world will necessarily grow.

It may have been their perseverence in experimentation of this kind — often involving messy manipulations and even offensive smells — that caused scientists to be looked down upon by the genteel.

Aristotelian Experiments. In explaining this second kind of experimentation I have followed a lead of Joseph Glanvill's. This experiment, too, was contrived — to demonstrate the truth {71} of a preconceived idea or to act out some calculated pedagogic plot: apply electrodes to the frog's sciatic nerve, and lo, the leg kicks; always precede the presentation of the dog's dinner with the ringing of a bell, and lo, the bell alone will soon make the dog dribble. Joseph Glanvill, in common with many of his contemporary Fellows of the Royal Society had the utmost contempt for Aristotle, whose teachings he regarded as major impediments to the advancement of learning. In *Plus Ultra* he wrote of such experiments thus: "Aristotle... did not use and imploy Experiments for thc erecting of his Theories: but having arbitrarily pitch'd his Theories, his manner was to force Experience to suffragate, and yield countenance to his precarious Propositions."

Galilean Experiments. Neither the Baconian nor the Aristotelian but rather the Galilean is the sense in which most scientists use the word *experiment* today.

A Galilean is a *critical* experiment — one that discriminates between possibilities and, in doing so, either gives us confidence in the view we are taking or makes us think it in need of correction.

Galileo's having been born in Pisa made it inevitable that his superlative critical experiment on gravitational acceleration should be taken by everyone to have been executed'by the dropping of cannonballs of different weights from the Leaning Tower. In reality, it was conducted without endangering life.

Galileo saw this kind of experiment as the ordeal (*il cimento*) to which we expose our hypotheses or the implications that follow from them.

Because of the asymmetry of proof explained below, experiments are very often designed not in such a way as to *prove* anything to be true — a hopeless endeavor — but rather to refute a "null hypothesis." As Karl Popper has pointed out, most general laws can be so construed as to *prohibit* the occurrence or deny the existence of certain phenomena or events. Thus the "law of biogenesis" declares that all living things are and always were the progeny of living things, and may thus be taken to prohibit the occurrence of spontaneous generation, the existence of which was made extremely doubtful by Louis Pasteur's brilliant experiments on bacterial putrefaction. Likewise, the {72} Second Law of Thermodynamics prohibits the occurrence of a great many phenomena that do not occur even in these permissive days. All prohibitions enforced by the Second Law are so many variants of the principle that speaks of the very extreme unlikelihood of passing spontaneously from a more probable to a less probable state. These prohibitions unfortunately include many plausible and profitable-sounding enterprises for designing self-energizing machines or machines of perpetual motion or for using twenty gallons of tepid bathwater to boil a kettle for one's coffee, and so on.

This possibility.of casting many hypotheses into a negative form explains why so many experiments attempt to refute a null hypothesis — that which denies the validity of a hypothesis under investigation. The same principle applies to many statistical tests, where an example of R. A. Fisher's is as good as any: a tea drinker who professes always to be able to tell whether the milk has gone in first or last is exposed to *il cimento*, in which the null hypothesis is that her score of right and wrong guesses could perfectly well have been due to luck alone.

Although these various considerations can be spelled out logically, most scientists pick them up so quickly and so naturally that they seem almost instinctual in the way they go about their business. It is seldom said of any series of experiments that they "prove" the hypothesis under investigation, for long experience of human fallibility has taught scientists rather to say that their experimental findings or analyses "are (or are not) consistent with" the hypotheses under experimental investigation.

No experiment should be undertaken without a clear preconception of the forms its results *might* take; for unless a hypothesis restricts the total number of possible happenings or conjunctions of events in the universe, the experiment will yield no information whatsoever. If a hypothesis is totally permissive — if it is such that *anything* goes — then we are none the wiser. A *totally* permissive hypothesis says nothing.

The *"result"* of an experiment is never the *totality of ob-servables;* the result of an experiment is almost always the *difference* between at least two sets of observables. In a simple, one-factor experiment, the two sets of observables are called the "experiment" and the "control." In the former, the factor {73} under investigation is allowed to be present or to exercise its effects, and in the latter it is not. The "result" of the experiment is then the difference between the readings or counts in the experiment and the control. An experiment executed without a control is not Galilean in style but might still qualify as an experiment in the Baconian style — that is, as a little contrived performance of nature, though not a very informative one. In the performance of what is intended to be a critical experiment, clarity of design and fastidiousness of execution are the qualities to be aimed at.

It is a common failing — and one that I have myself suffered from — to fall in love with a hypothesis and to be unwilling to take no for an answer. A love affair with a pet hypothesis can waste years of precious time. There is very often no finally decisive yes, though quite often there can be a decisive no.

Kantian Experiments. Baconian, Aristotelian, and Galilean are not the only kinds of experiment. There are thought experiments, too; Kantian, I have called them in honor of the most breathtaking conceptual exploit in the history of philosophy: Kant's suggestion that instead of acquiescing in the ordinary opinion that our sensory intuitions are patterned by "objects" — by that which is perceived — we should take the view that the world of experience is patterned by the character of our faculties of sensory intuition. "This experiment succeeds as well as could be desired," Kant complacently remarked, and it led him to formulate his well-known opinion that *a priori* knowledge — knowledge independent of all experience — can exist; he reasoned that both space and time *axe forms* of sensory intuition and as such are only "conditions of the existence of things as appearances." Before dismissing such an opinion as the merest metaphysical fancy, scientists should reflect that sensory physiology is becoming increasingly Kantian in tendency.[4] Another famous Kantian experiment is that which generates the classical non-Euclidean geometries (hyperbolic, elliptic) by replacing Euclid's axiom of parallels (or something {74} equivalent to it) with alternative forms. Demographic and economic projections are other examples of Kantian experimentation: "Let's see what would follow if we took a somewhat different view..."

Kantian experimentation requires no apparatus except sometimes a computer. The forms of experimentation characteristic of the natural sciences are Baconian and Galilean; upon these, it may be said, all natural science rests. In the historical, behavioral, and mainly observational sciences, exploratory activities normally end in the formulation of opinions of which the implications can be tested either by sociological field surveys, carbon dating, ascertaining the facts of the matter, referring to historical documents, or turning a telescope to a predetermined region of the sky. All such activities are Galilean in spirit — that is, they are critical evaluations of ideas.

The effect of Galilean experimentation is to preserve us from the philosophic indignity of persisting unnecessarily in error (the constant working of the process of rectification is discussed at length in Chapter 11). Any experienced scientist knows in his heart what a good experiment is: it is not just ingenious or well executed in point of technique; it is something rather sharp; a hypothesis does well to have stood up to it. Thus the merit of an experiment lies principally in its design and in the critical spirit in which it is carried out.

Elaborate and costly apparatus will sometimes be required, but no one should be taken in by the romantic notion that any scientist worthy of the name can carry out an experiment with no more apparatus than string, sealing wax, and a few empty bean cans; there is no conceivable method by which a sedimentation coefficient could be estimated with a bean can and string, unless someone is capable of swinging the can around his head more than a thousand times a second.[5] On the other hand, scientists must exercise discretion about the cost and complexity of the instruments they feel they need to use. Before commandeering costly plants and the services of colleagues night {75} and day, scientists should make very sure that their experiments are worth doing. It has been well said that "if an experiment is not worth doing, it is not worth doing well."

Discoveries

Experiments, then, are of many different kinds. So are discoveries. Some discoveries *look* as if they were merely a recognition or apprehension of the way nature is; they are lessons learned, as it were, by humbly taking note of what is going on; they have the air of being no more than "uncoveries" of what was there all the time, waiting to be taken note of. I myself believe it to be a fallacy that any discoveries are made in this way. I think that Pasteur and Fontenelle (see Chapter 11) would have agreed that the mind must already be on the right wavelength, another way of saying that all such discoveries begin as covert hypotheses — that is, as imaginative preconceptions or expectations about the nature of the world and never merely by passive assimilation of the evidence of the senses. It may of course be that an information-hunting exercise is that which prompts a hypothesis to take shape. Darwin's letters show that in believing himself to be a "true Baconian" he was simply deceiving himself.

Even so seemingly straightforward a discovery as that of a fossil is often the outcome of covert-hypothesis formation — for why otherwise should anyone look at the fossil remains twice and maybe take them back for more detailed investigation later? But how can we fit into this scheme such a remarkable discovery as that of the "living fossil" fish, the coelacanth *Latimeria?* What made this discovery so striking was this: most fossils — for example, those of the lungfish — are discovered *after* their living descendants have been recognized and described; it is most unusual for a fossil to be discovered before a living relative, as happened with *latimeria*. This is why its discovery gave the impression of a privileged and in some ways frightening insight into the world of very many million years ago.

Although I believe the same acts of mind underlie them both, I think it useful to draw a broad distinction between synthetic and analytic discoveries. A synthetic discovery is {76} always a first recognition of an event, phenomenon, process, or state of affairs not previously recognized or known. Most of the stirring and deeply influential discoveries of science come under this heading. It is characteristic of a synthetic discovery that it need not have been made then and there — that it might, just conceivably, never have been made at all. Perhaps that is why we hold them in such awe.

My favorite example of this species is the discovery by Fred Griffith of the phenomenon of pneumococcal transformation,[6] which gave birth to modern molecular genetics. It turned out that the dead pneumococci that conferred some of their characteristics upon the living pneumococci in Griffith's famous experiment did not have to be whole and intact because extracts had the same effect. Some one particular chemical compound must have been responsible for the transformation. It was one of the great episodes in modern science when Avery, McLeod and McCarty showed this to be deoxyribonucleic acid (DNA). It in no way diminishes this discovery to describe it as "analytic" in character, for it was a triumph of intuition and experimental skill.

The character of an analytic discovery may also be illustrated by following the train of thought that led to the discovery of the structure of DNA. Ever since W. T. Astbury published the first X-ray crystallographs of DNA, imperfect though they were, it was recognized that DNA had a crystalline structure, probably of a repetitive or polymeric kind. The discovery of this structure was the outcome of the intellectual process described in Chapter 11 — that is, the result of sustained dialogue between conjecture and refutation. But of course the distinction between synthetic and analytic is not hard and fast, for in the discovery of the structure of DNA there was both an analytic and a synthetic element, the latter being that its structure was just such as to equip it to encode and transmit genetic information. {77} This perhaps was the greater discovery, and in describing it as "the greater," I am speaking for the very widespread belief that synthetic discoveries — those which open up new worlds not until then known to exist — are those which scientists would most like to make.

But it would be wrong to make too much of discoveries. The greatest advances in modern biology have grown out of the intent and unrelaxing study of the characteristics of a single biological phenomenon or a single biological "system." This was the story of pneumococcal transformation and of protein synthesis in *Escherichia colt*, which showed the stages by which the structure of nucleic acid is mapped into the structure of a protein. So it will be now, I suspect, with the detailed mapping of the cell surface in respect of "histocompatibility" antigens. An individual discovery is here less important than the deep analysis that will eventually make known the molecular basis of specificity and help to explain why, in development, some cells go here rather than there, and some stick together though others do not. Deep analyses such as those of molecular biology will one day enable detailed molecular specifications to be drawn up for the synthesis of an enzyme or of an enzyme cascade that will, say, degrade polyethylene and thus reduce the proportion of the earth's surface occupied by the detritus of affluence.

For these reasons, a young scientist must not be disheartened if he does not become the eponym of a natural principle, phenomenon, or disease. Although the importance of discoveries may be overrated, no young scientist need think that he will gain a reputation or high preferment merely by compiling information — particularly information of the kind nobody really wants. But if he makes the world more easily understandable by any means — whether theorctical or experimental — he will earn his colleagues' gratitude and respect.

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Prizes and Rewards

Scientists, like sportsmen and writers, are in the running for a whole variety of prizes and other rewards.

I knew a scientist who lost no opportunity to impress upon me his disapproval of the existence of such invidious distinctions, savoring as they did of elitism — the socially divisive notion that some people are better than others at some things — but when the opportunity came for his own nomination for the Fellowship of the Royal Society, he did not decline. Although in one of his Olympian moods that great mathematician G. H. Hardy referred to the Fellowship of the Royal Society as "a comparatively humble level of distinction," it is a greatly admired and eagerly sought-after reward for prowess in science. Ordinary membership is confined to British citizens, but honorary affiliations cast a wider net.

An elected F.R.S. is required to sign a book that contains the signatures of many of the greatest figures in the history of science; a new Fellow may indeed exult in being one of the company that includes Isaac Newton, Robert Boyle, Christopher Wren, Michael Faraday, Humphry Davy, James Clerk Maxwell, Benjamin Franklin and Josiah Willard Gibbs.

The Royal Society has a history going back to the days when a great revolution of the human spirit[1] inaugurated the modern {79} world. It is far otherwise with the Nobel Prize, for the simple and sufficient reason that most of the very greatest scientists lived long before Alfred Nobel got the knack of stabilizing the nitric acid esters of polyhydric alcohols (especially glyceryltrini-trate) and founded the prize on the proceeds.[2] The Nobel award owes its great popular reputation to many things: public satisfaction in the expiatory element in the foundation of the award, the grand ceremony of the accolade, the size of the sum that changes hands, and the element of real distinction it embodies. But — and this is, I believe, the only valid ground for objecting to all such distinctions — all electoral procedures are fallible and the failure to gain a distinction of which a scientist is, and feels himself to be, genuinely worthy may cause not only great un-happiness but also personal injury to those whose livelihood and research support depends upon the judgment of people (for example, administrative high-ups) who may not realize how very many scientists are not, though they deserve to be, Fellows of the Royal Society of London or other comparable bodies. The same applies to the Nobel Prize, though it is difficult to feel the same sympathy because those who are not Nobel laureates but are sufficiently accomplished to be judged in the running are not likely to be embarrassed by lack of research funds.

Conventional wisdom has it that it is "bad" for the young to be successful too early: too many prizes and too high a scholastic record bode no good, we are sometimes told. "I'm afraid I wasn't very brainy at school," declares the pompous chump giving out the prizes, leaving us to infer that because of his other still more praiseworthy abilities it didn't handicap *him* a bit.

The supposed correlation between early success and later failure arises, I suspect, from one of those tricks of selective memory I have referred to elsewhere: of those who come to dust, it is the golden boys and girls we remember best; if they succeed — why, that was only to be expected, and so we remember only the failures. {80}

I have emphasized a darker side of prizes and rewards, but there is a very bright side, too: all such elections or nominations depend upon the good opinion that scientists are most eager for — the high opinion of their peers. The effect upon good scientists of gaining an award is a great moral boost — this expression of the confidence and esteem of others will promote their research and perhaps help them to do better than before. Very likely, too, the prizewinner will want to show everyone that it wasn't all a fluke.

In these respects, awards are wholly beneficent, but sometimes, unhappily, they have the opposite effect I remember a fellow graduate student and I at Oxford telling each other in shocked voices of a university don who had said, "As soon as I get into the Royal, I shall give up research altogether." It seems only poetic justice that the occasion never arose for him to fulfill that ignoble ambition.

Of course, the head is sometimes turned by these distinctions, and there are Nobel laureates who give up research and spend their time traveling the world attending and sometimes addressing conferences with titles such as Science, Mankind, Values, and Human Endeavor (or any other such juxtaposition of abstract nouns). The vanity of such laureates is constantly inflamed by their being invited to sign and thus tip the scales in favor of the acceptance of some such manifesto as this: "The nations of the world must henceforward live together in amity and concord and abjure the use of warfare as a means of settling political disputes."

Can it be that a substantial number of people hold a contrary opinion but are suspending judgment until the signatures of fifty Nobel laureates convince them of its truth? It is all part of the human comedy, of course, but the exaggerated respect for prizewinners may sometimes be turned to useful ends — particularly in helping to secure the release from tyranny of prisoners of conscience, work in which Amnesty International has been particularly active.

It is fortunate that scientific honors cannot be worked for as one works for an exam; a young scientist can only hope that his work will be good enough for him to take his place

{81} one day among the candidature for such distinctions.

There is nothing ignoble about such an ambition, and that young scientists should cultivate it was often a principal purpose of the founders or sponsors of the award.

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11

The Scientific Process

Je cherche à comprendre

— Jacques Monod

How do scientists go about making discoveries, propounding "laws," or otherwise enlarging human understanding? The conventional answer, "by observation and experiment," is certainly not mistaken, but it needs to be interpreted with reserve. Observation is not a passive imbibition of sensory information, a mere transcription of the evidence of the senses, and experimentation is not only of the kind that I classified as Baconian in Chapter 9 — that is, the contrivance of phenomena or conjunctions of events that do not occur spontaneously in nature. Observation is a critical and purposive process; there is a scientific reason for making one observation rather than another. What a scientist observes is always a small part only of the whole domain of possible objects of observation. Experimentation, too, is a critical process, one that discriminates between possibilities and gives direction to further thought.

A young scientist has now a meter or so of bench space, let us say, a white coat, authority to use the library, and a problem that he has thought up himself or that a senior has asked him to look into. To begin with, anyway, it is almost certain to be a small problem — one of which the solution will facilitate the solution of something more important, and so on, until the long-term objective of the enterprise is in sight. Nonscientists cannot immediately see the connection between the lesser {83} problem and the greater. It must often occur to a humanist as he reads the minutes of the board of the faculty of science that young scientists are engaged in comically specialized activities. A scientist might equally well wonder what there could be to engage a grown man in the study of the parochial affairs of Tudor Cornwall, because he does not realize that such an investigation is about the Reformation, a very great affair indeed.

But what will a scientist *do* to resolve his problem? Something of which he can be quite certain is that no mere compilation of factual information will serve his purpose.[1] No new truth will declare itself from inside a heap of facts. It is true that Bacon and Comenius and Condorcet too (see below) sometimes wrote as if they believed that the collection and classification of empirical facts would lead to an understanding of nature, but in taking this view they were guided by a rather special consideration: they felt under a strong obligation to refute the idea that *deduction* was an act of mind that could lead to the discovery of new truths — that an act of mind alone could enlarge the understanding. The philosophic and scientific writing of the seventeenth century — particularly the writing of Bacon, Boyle, and Glanvill, for example — is full of dismissive references to Aristotle's way of thinking, in the tradition of which they had all grown up.

Bacon's exhortation to observe and to experiment does not, of course, tell the whole story of his scientific philosophy; he also propounded a number of rules for getting at the truth of things essentially similar to those which two hundred years later John Stuart Mill propounded as the rules of discovery in his *System of Logic*. These rules of induction are applicable only under special circumstances: when we have before us all and only the facts relevant to the solution of our problem — the whole truth and nothing but. Thus we may be called upon to conduct an epidemiological exercise to account for the violent sickness of {84} a member of a dinner party; we know what they all ate and drank, we know that all were hale when they sat at table, and that all but the victim remained so afterward. On this basis, the so-called rules of induction can be applied; the things eaten by everyone are not likely to be responsible for the illness of only one, nor is the dish that everyone refused: only the victim, it turns out, ate the cream syllabub. Only a singularity of exposure to risk can account for the victim's singular misfortune. These simple exercises in elementary logic and common sense are hardly worth dignifying by the long appellations Bacon gave them. The rationale of fact-hunting in the eyes of such as Mill and Bacon was that it would put the scientist in possession of the facts upon which such a calculus of discovery could be made to work.

In real life it is not like this. The truth is *not* in nature waiting to declare itself, and we cannot know *a priori* which observations are relevant and which are not; every discovery, every enlargement of the understanding begins as an imaginative preconception of what the truth might be. This imaginative preconception — a "hypothesis" — arises by a process as easy or as difficult to understand as any other creative act of mind; it is a brainwave, an inspired guess, the product of a blaze of insight. It comes, anyway, from within and cannot be arrived at by the exercise of any known calculus of discovery. A hypothesis is a sort of draft law about what the world — or some particularly interesting aspect of it — may be like; or in a wider sense it may be a mechanical invention, a solid or embodied hypothesis of which performance is the test.

Thus the day-to-day business of science consists not in hunting for facts but in testing hypotheses — that is, ascertaining if they or their logical implications are statements about real life or, if inventions, to see whether or not they work. In the Galilean sense (see Chapter 9) in which I said the word *experiment* is now most widely used, experiments are the acts undertaken to test a hypothesis.

In the outcome, science is a logically connected network of theories that represents our current opinion about what the natural world is like.

Once he has a hypothesis to work on, the scientist is in {85} business; the hypothesis will guide him to make some observations rather than others and will suggest experiments that might not otherwise have been performed. Scientists soon pick up by experience the characteristics that make a good hypothesis; as explained in Chapter 9, almost all laws and hypotheses can be read in such a way as to *prohibit* the occurrence of certain phenomena (the example I gave was the prohibition by the law of biogenesis of the occurrence of spontaneous generation). Clearly, a hypothesis so permissive as to accommodate *any* phenomenon tells us precisely nothing; the more phenomena it prohibits, the more informative it is.

Again, a good hypothesis must also have the character of *logical immediacy*, by which I mean that it must be rather specially an explanation of whatever it is that needs to be explained and not an explanation of a great many other phenomena besides. It is not wrong but equally it is not very helpful to interpret Addison's disease or cretinism as the consequence of a "malfunction of the hormone-secreting glands." The great virtue of logical immediacy in a hypothesis is that it can be tested by comparatively direct and practicable means — that is, without the foundation of a new research institute or by making a journey into outer space. A large part of the *art of the soluble* is the art of devising hypotheses that can be tested by practicable experiments.

Most of the everyday business of the empirical sciences consists in testing experimentally the logical implications of hypotheses — that is, the consequences of assuming for the time being that they are true. The experiments I described as critical or Galilean give direction to further speculation: their results either square with the hypothesis under consideration, in which case it remains on probation while some further and more searching tests are planned, or else cause the hypothesis to be revised or in the extreme case to be abandoned altogether, whereupon the dialogue must being anew. The dialogue I envisage is between the possible and the actual, between what *might* be true and what is in fact the case — a dialogue between two voices, the one imaginative and the other critical, between *conjecture and refutation*, as Popper has it.

These acts of mind are characteristic of *all* exploratory processes {86} and are certainly not confined to experimental sciences, for this is essentially how an anthropologist will proceed, a sociologist, or a physician intent upon diagnosis. It is also the process of mind used by the mechanic who tries to figure out what is wrong with a car. It is all very far removed from the fact-hunting of classical inductivism. As a point of logic that has some bearing on the way he thinks he goes about his business, a young scientist must always avoid saying or thinking that he "deduces" or "infers" hypotheses. On the contrary, a hypothesis is that *from* which we deduce or infer statements about matters of fact, so that, as the great American philosopher C. S. Peirce clearly recognized, the process by which we try to think up the hypotheses from which our observations will follow is an inverse form of deduction — a process for which he coined the terms *retroduction* and *abduction*, neither of which has caught on.

Some Implications of These Views

Feedback. Although it has been pointed out very often, there is no harm in pointing out again that if the inferences we draw from a hypothesis are thought of as its logical output, then the process by which we modify a hypothesis in accordance with the degree of correspondence of its predictions to reality is yet another example of the fundamental and ubiquitous stratagem of negative feedback (see "Falsification," below). This parallel reminds us that scientific research, like other forms of exploration, is, after all, a cybernetic — a steering — process, a means by which we find our way about, and try to make sense of, a bewildering and complex world.

Falsification and the Asymmetry of Proof. The recognition of the asymmetry of proof is fundamental to an understanding of the scheme of thought just outlined (the "hypothetico-deductive" scheme).

Consider a simple syllogism from schoolroom logic:

major premise: All men are mortal.
minor premise: Socrates is a man.
inference: Socrates is mortal. {87}

If correctly executed, the process of deduction brings with it the complete and unqualified assurance that if the premises are true, then the inference must also be true. Socrates must indeed be mortal. No question. But this is a one-way process; the mortality of Socrates, supposing that historical research confirms it, gives us no positive assurance of his having been a man or of the mortality of mankind generally. The syllogism and the inference would be equally binding upon us if Socrates were a fish and all fish mortal. We can, however, say with complete certainty that if Socrates were *not* mortal — that is, if the inference were wrong — then we must be thinking on the wrong lines: either Socrates was not a man or not all men are mortal.

The upshot of this asymmetry of inference is that falsification is a logically stronger process than what sometimes people rather recklessly refer to as "proof; indeed, a scientist does not very often speak with complete confidence of "proof." The more experienced he is, the less likely he is to do so. As they grow in experience, scientists soon come to appreciate the special strength of falsification and the precariousness of what beginners call "proof," for as explained in Chapter 9 (where a different reason for this experimental design was given), it is a well-known stratagem of research to investigate and mayhap refute the "null" hypothesis, which affirms the very opposite of whatever may be under investigation. For all these reasons no hypothesis in science and no scientific theory ever achieves apodictic certainty — never achieves a degree of certainty beyond the reach of criticism or the possibility of modification.

A scientist is, then, a *seeker after truth.* The truth is that which he reaches out for, the direction toward which his face is turned. Complete certainty is beyond his reach, though, and many questions to which he would like answers lie outside the universe of discourse of natural science. The last words of one of the greatest scientists of the twentieth century, Jacques Lucien Monod, which I have used as the motto of this chapter, embody an ambition that a scientist can always achieve: he can try to understand. {88}

What Is a Scientific Statement? Scientists who in their professional capacities make scientific statements may sometimes be too ready to accuse others of being "unscientific," so it would be useful to have a criterion, a line of demarcation to make it possible to distinguish between statements that belong to the world of science and of common sense and those that belong to some other world of discourse.

When logical positivists first tackled this problem, they felt they had the answer in the notion of "verification." Scientific statements were verifiable in fact or in principle; verifiability "in principle" was enjoyed by those statements of which it was possible to see what steps should be or could be undertaken to verify them. Statements not verifiable in principle were dismissed as "metaphysical" — a word clearly used as a euphemism for nonsense. Karl Popper, because of his special and well-founded views on the efficacy of falsification, substituted "falsifiability in principle" for "verifiability in principle." The new line of demarcation he proposed was *not*, he insisted, between sense and nonsense, but simply between two different worlds of discourse, the one belonging to the world of science and common sense, the other to metaphysics and serving altogether different purposes.

Where Does Luck Come into All This? "Serendip" was an old-fashioned name for Ceylon. It was a conceit of Horace Walpole's that the three princes of Serendip were forever coming upon felicitous discoveries or inventions by good luck alone: hence, "serendipity."

Luck plays a real part in scientific research, and after long periods of discouragement or following pathways of research that lead nowhere, scientists often say or think they are about duc for a lucky break. By this they do not mean anything that would be judged lucky by the criterion of induction — a lucky presentation to their senses, ready-made, of some important new phenomenon or conjunction of events. What they mean is that it's about time they had a right idea instead of a wrong one — about time they hit upon a hypothesis that not only ostensibly explains what is to be explained but also stands up to critical evaluation.

Dr. Roger Short has given a most interesting example of the {89} inadequacy of mere observation in discovery. It gains special force from the fact that William Harvey was a superlative *observer*. Writing of Harvey's conception of conception, Short points out that he dismissed altogether the complicity of the ovaries in mammalian reproduction, believing with Aristotle that the egg was a product of conception and especially of the male "seed." Short adds: "Harvey's dissections and observations were almost faultless, and it was only in their interpretation that he erred. His mistake may even serve as a lesson to many of us today."[2]

But what about luck in a more familiar and less intellectual sense? What about, for example, the discovery by Alexander Fleming of penicillin?

Fleming was a fine scientist and therefore not too grand to set up his own bacterial culture plates. The myth (for so I have been told it is), however, goes as follows. One day, when Fleming was setting up a plate of staphylococci or streptococci, a spore of the bread mold *penicillium* floated in through the window and settled upon his culture plate. Around the spore there developed a halo of inhibition of bacterial growth, the germinal discovery from which all the rest followed.

For very many years I accepted this story because I had no reason or inclination to do otherwise, but a cynical bacteriologist at the British Postgraduate Medical School in Hammersmith challenged it on several grounds. First of all, a spore of penicillin will not germinate in this way to give rise to a zone of inhibition of bacterial growth. The bacteriologist went on to tell me that St. Mary's was an old-fashioned building, the windows of which would either not shut or not open. Fleming's were of the latter kind; so much for the spore's floating in through the window.

I was sorry that the traditional story of Fleming's discovery did not stand up to critical scrutiny because I should have liked to have believed it true; but even if it had been true, it would not have told us very much about the efficacy of luck. Fleming {90} was a humane and gentle man who had been shocked and sickened by the gangrene and other horrible complications he had found in the battle casualties of World War I. The phenolic antiseptics that alone were available were almost completely inactivated by body fluids and would have damaged the tissues of the body more than the bacteria, thus adding to the complications of an infected wound. Fleming therefore had clearly in mind the special advantages of an antibacterial substance that did not damage tissues.

It is not methodologically an exaggeration to say that Fleming eventually found penicillin because he had been looking for it. A thousand people might have observed whatever it was that he did observe without making anything of it or building upon the observation in any way; but Fleming had the right slot in his mind, waiting for it. Good luck is almost always preceded by an expectation that it will gratify. Pasteur is well known to have said that fortune favors the prepared mind, and Fontenelle observed, *"Ces hasards ne sont que pour ceux quijouent bien!"* ("These strokes of good fortune are only for those who play well!").

There *was* one amazing stroke of pure good luck about penicillin for which no one's mind could possibly have been prepared because only recent research has brought it to light: most antibiotics are exceedingly toxic because they interfere with a department of bacterial metabolism shared by bacteria and ordinary body cells. Actinomycin D provides a good example because it interferes with the mapping of the DNA of the cell nucleus into the RNA through which its genetic effects are exercised; because the mechanism is common to both, actinomycin affects ordinary body cells as it does bacteria. Penicillin is not toxic because it affects metabolism of a kind peculiar to bacteria.

Limitations of Science. If we accept, as I fear we must, that science cannot answer questions about first and last things or about purposes, there is yet no known or conceivable limit to its power to answer questions of the kind science *can* answer. The founding fathers of the seventeenth century were not mistaken in taking *plus ultra* as a slogan — in believing that in science there is always more beyond. When Whewell first {91} propounded a view of science of the same general kind as that which Karl Popper has developed into a thoroughgoing system, his opponent John Stuart Mill was shocked by the reflection that hypotheses were products of the imagination and had no confinements, therefore, other than those of the imagination itself; yet what scared Mill is one of the great glories of science and our principal assurance that it has no limit. Science will dry up only if scientists lose or fail to exercise the power or incentive to imagine what the truth might be. One can envisage an end of science no more readily than one can envisage an end of imaginative literature or the fine arts. Some problems may be insoluble, of course; Karl Popper and John Eccles have commented that the connection between brain and mind might be one,[3] but it is not easy to think of a second.

The March of Paradigms

My partiality for the "hypothetico-deductive" account of the scientific process has been based on as accurate a study as I have found it possible to carry out on my own processes of thought, abetted by opinions of the fairly large number of scientists and physicians who have come to think it a fair representation of the exploratory process; but it would be very unfair to create the impression that the scheme I have outlined is the only prevailing interpretation of the scientific process. Great interest was aroused by the views expounded by Thomas Kiihn in *The Structure of Scientific Revolutions* and more recently in *Essential Tension.*[4] There is an illuminating discussion of Kuhn's view by Kuhn himself and others in a symposium entitled *Criticism and the Growth of Knowledge.*[5]

Kuhn's views have caught on — a sure sign that scientists find them illuminating because they haven't much time for what {92} they think of as mere philosophizing. Kuhn's views and Popper's are not antithetical.

Kuhn's position is in outline this. In the critical evaluation of hypotheses to which Popper rightly attaches such great importance, the evaluation of a hypothesis is not a private transaction between the scientist and reality — a competition, as it were, between fact and fancy. That which the scicntist measures his hypotheses against is the current "establishment" of scientific opinion — the current framework of theoretical commitments and received beliefs — the prevailing "paradigm" in terms of which the day-to-day problems arising in a science tend to be interpreted. A scientist who explores within its ambience is executing what Kuhn calls "Normal Science," and his researches are so much puzzle-solving.

It is no wonder that J. W. N. Watkins in the symposium to which I have referred above remarked that Kuhn sees the scientific community on the analogy of a religious community, with a science as a scientist's religion. It is true, certainly, that scientists are often reluctant to shake off received beliefs and sometimes feel impatient of notions that fall outside the prevailing paradigm, but normal science does not long persist unchallenged; every so often, an extraordinary scientist or extraordinary scientific phenomena supplant the prevailing paradigm by a new orthodoxy — a new paradigm that defines a "normal" science anew and lasts until the revolutionary appraisal is repeated. The "essential tension" to which Kuhn refers in the title of his latest book is between our inheritance of doctrine and dogma as they affect science and the occasional upheavals that inaugurate a new "paradigm" in the terminology Kuhn has made popular.

Kuhn's views throw some light on the psychology of scientists and are an interesting comment on the history of science, but they do not add up to a methodology — a system of canons of inquiry.

In real life, a scientist tends to believe in a hypothesis until he has reason to do otherwise. This, then, is his personal paradigm, reinforced perhaps by some pride of possession if it embodies an idea of his own. As for revolutions, they are constantly in progress; a scientist does not hold exactly the same opinions {93} about his research from one day to the next, for reading, reflection, and discussions with colleagues cause a change of emphasis here or there and possibly even a radical reappraisal of his way of thinking. In a laboratory there are continual movements of unrest. There is something about Kuhn's writing that makes me think that he sees normal scientific life as one of settled, Godfearing bourgeois contentment within an established order of things, but in reality it is more like a Maoist microcosm of continuing revolution; in any laboratory conducting original investigation, all is in flux. It may, of course, be different in the social sciences, which have a slower pulse and in which an opinion takes very much longer to appraise. Here perhaps we may speak of a "normal science," and the process by which it is supplanted may be likened more aptly to a revolution.

Is There Too Much Fuss About Method? Even though an episode of scientific inquiry can be shown in retrospect to have a hypothetico-deductive character, a young scientist may well wonder if there need be any great formality about it all; most scientists, he may reflect, have received no formal instruction in scientific method, and those who have seem to do no better than those who have not.

A young scientist has no need to exercise a methodology in any highfalutin sense; he must realize very clearly, though, that collecting facts could at best be only a kind of indoor pastime. There is no formulary of thought or program of ratiocination that can conduct him quickly from empirical observations to the truth. An act of mind always interposes between any observation and any interpretation of it. The generative act in science, I have explained, is imaginative guesswork. The day-today business of science involves the exercise of common sense supported by a strong understanding, though not using anything more subtle or profound in the way of deduction than will be used anyway in everyday life, something that includes the ability to grasp implications and to discern parallels, combined with a resolute determination not to be deceived either by the evidence of experiments poorly done or by the attractiveness — even lovableness — of a favorite hypothesis. Heroic feats of intellection are seldom needed. "The scientific method," as it is sometimes called, is a potentiation of common sense. {94}

Before he sets out to convince others of his observations or opinions, a scientist must first convince himself. Let this not be too easily achieved; it is better by far to have the reputation for being querulous and unwilling to be convinced than to give reason to be thought gullible. If a scientist asks a colleague's candid criticism of his work, give him the credit for meaning what he says! It is no kindness to a colleague — indeed, it might be the act of an enemy — to assure a scientist that his work is clear and convincing and that his opinions are really coherent when the experiments that profess to uphold them are slovenly in design and not well done. More generally, criticism is the most powerful weapon in any methodology of science; it is the scientist's only assurance that he need not persist in error. All experimentation is criticism. If an experiment does not hold out the possibility of causing one to revise one's views, it is hard to see why it should be done at all.

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